

Essays on the Political Economy of Democratization and Democratic Backsliding



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Declaration

I hereby declare that except where specific reference is made to the work of others, the contents of this dissertation are original and have not been submitted in whole or in part for consideration for any other degree or qualification in this, or any other university. This dissertation is my own work and contains nothing which is the outcome of work done in collaboration with others, except as specified in the text and Acknowledgements. This dissertation contains fewer than 80,000 words including appendices, bibliography, footnotes, tables and equations and has fewer than 150 figures.

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Abstract

This four-paper dissertation addresses three fundamental questions in the political economy of democracy. First, does economic development cause democratization? Second, to what extent are citizens willing to defend democracy after it has been established, but is then threatened from within by an anti-democratic state executive? Third, what influence has the recent episode of democratic backsliding within the United States had on America's soft power abroad?

In the first paper, I provide a new theory of the relationship between economic development and democracy. I argue that a large share of employment in manufacturing (i.e., industrialization) makes mass mobilization both more likely to occur and more costly to suppress. This increases the power of the masses vis-à-vis autocratic elites, making democracy more likely. Using novel manufacturing employment data for 145 countries over 170 years (1845–2015), I find that industrialization is strongly correlated with democracy, even after accounting for country and time fixed effects, time trends, theoretically grounded controls, and other economic determinants of democracy (e.g., income and inequality). Unlike with other economic determinants, the effect occurs on both democratic transitions and consolidations, and is equally large after 1945. Importantly, the data suggests that many potential outliers (e.g., China, the USSR, and Latin America during import substitution industrialization) have in fact never reached the level of industrialization that existed in the West, South Korea, and Taiwan before democratization.

In the second paper, I exploit a unique quasi-experiment in 19th- and early 20th-century Norway to test whether the correlation between manufacturing employment and democracy is causal.¹ Using novel roll-call data from the Norwegian national parliament, I study whether MPs that represented more rapidly industrializing districts were more likely to vote for suffrage extensions over the 1891 to 1906 period. For causal identification, I exploit the fact that Norwegian districts with a greater geographical potential for hydropower generation were significantly more likely to industrialize after the nationwide introduction of hydroelectricity in 1892. In line with the first paper, I find that industrialization tended to induce democratization in Norway.

In the third paper, I turn to the contemporary period and study whether politicians who clearly violate democratic norms lose significant public support, or whether voters tend to form little constraint on democratic backsliding. To examine this fundamental question, I study a novel natural experiment created by the fact that Donald Trump's incitement of the January 6 insurrection at the U.S. Capitol unexpectedly occurred while Gallup was conducting a nationally representative public opinion survey using random digit dialing. Comparing party identification among respondents who happened to be interviewed just before, and just after, January 6, 2021, suggests that the Republican Party retained 78% of its pre-insurrection support base during the first 1.5 weeks after the January 6 insurrection. Even this modest loss was only short-lived—in February 2021 the Republican Party already stood at 93% of its pre-insurrection support level.

In the last paper, I examine the consequences of democratic backsliding within the United States on America's standing abroad. To do so, I exploit the fact that the January 6 insurrection unexpectedly occurred while Gallup was conducting nationally representative surveys in India, Indonesia, Malaysia, Romania, and Vietnam using random digit dialing. In contrast to many soft power theories in international relations, I find that the January 6 insurrection had *no* effect on U.S. leadership approval abroad.

¹This paper is co-authored with professors Magnus Rasmussen and Tore Wig (University of Oslo).

I would like to dedicate this thesis to my parents Gijs and Marli, and to my beautiful wife Tanushree.

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Chapter 1

Introduction

1.1 Summary of results

Political democracy—that is, a system of free and fair elections, whereby all adults are allowed to vote and stand for office, and where the information necessary to make reasonable judgments about political, economic, and social affairs is widely available—is fundamental to a just and well-functioning society.¹ The one-person, one-vote rule underlying democratic elections allows for all to have at least some say over the conditions that affect their lives. This is particularly important for the poor, who have little power under the one-dollar, one-vote rule that tends to govern modern economic life. Perhaps closely related to the fact that otherwise relatively powerless groups are better represented under democracy, democratization tends to lower infant mortality rates (Kudamatsu, 2012), reduce poverty rates (Gao and Zang, 2022), lessen ethnic favoritism in public goods provision (Burgess et al., 2015; Kroth, Larcinese

¹The term “democracy” refers fundamentally to an ideal type institutional system wherein all those affected by collective decisions have an equal chance of affecting what decisions are made, and thereby whose interests are served or violated (Dahl, 1973). Nothing restricts this necessarily to the sphere of politics and public policy making (e.g., “economic” democracy refers to the inclusion of all interested parties—including workers and consumers—in business decision-making). Because this dissertation focuses exclusively on political democracy, I simply refer to political democracy as “democracy” in the rest of the text.

and Wehner, 2016), and increase both the rate and the stability of economic growth (Acemoglu et al., 2019; Mobarak, 2005).

Although democracy is among the most fundamental outcomes in the social sciences, we still know relatively little about how countries become and remain well-functioning democracies (Coppedge, 2012; Geddes, 1999, 2007). This four-paper dissertation contributes to the existing academic literature (1) by highlighting a new causal mechanism through which transitions to and consolidations of democracy can occur, and (2) by studying the causes and effects of the recent period of democratic backsliding in the United States.²

In the first two papers, I provide evidence that suggests that a large share of the workforce employed in manufacturing (i.e., industrialization) makes the introduction and sustainment of democratic forms of government more likely. I show that all currently highly developed countries in the West and East Asia (Japan, Taiwan, and South Korea) democratized shortly after reaching high shares of employment in manufacturing, but several decades *before* reaching high levels of income, equality, education, and urbanization (the common “economic” variables studied in the existing democratization literature). This suggests that industrialization is a distinct and arguably more important economic determinant of democracy than are income, equality, education, and urbanization. This result is confirmed in a wide range of dynamic panel models with country and time fixed effects, using newly collected manufacturing employment data for 145 countries over 170 years (1845-2015). Evidence from a unique quasi-experiment in 19th- and early 20th-century Norway—where regions with a greater geographical potential to generate hydropower were significantly more likely

²Democratic backsliding is a process of democratic deterioration that stops short of a full transition to autocracy, but does significantly reduce the functioning of a democratic political system. It is typically driven by a state executive who incrementally weakens the political institutions meant to constrain his or her political power (Bermeo, 2016).

to industrialize after the invention of hydroelectricity—suggests that the effect of industrialization on democratization is causal.

I provide a novel theory that explains these empirical findings and illustrates how industrialization can lead to democracy beyond the other, more commonly studied economic determinants of democracy. The key causal mechanism that I highlight is how a large share of employment in manufacturing makes mass political mobilization both more likely to occur and more costly to suppress. I derive this prediction from three well-established structural economic properties of manufacturing: a deep division of labor, economies of scale, and linkages. Unlike agriculture, mining, and petty services (which are the predominant economic sectors in little-industrialized countries), manufacturing production combines significant economies of scale with a large potential for fine-grained labor specialization. Manufacturing capitalists have a strong incentive to exploit these features to reduce production costs and maximize profits. Exploiting these features typically requires the establishment of large firms where many workers work together in long and complex production chains. Doing so efficiently requires instilling in manufacturing workers a wide range of organizational capabilities that foster cooperation on a large and impersonal basis (e.g., negotiating and working together with strangers, addressing disagreements, disciplining free-riders). Many of these organizational capabilities are transferable to mass organization in general, and therefore, as an unintended consequence, increase manufacturing workers' capacity to mobilize politically. Importantly, industrialization not only boosts the capacity of manufacturing workers to organize mass political revolts, but also simultaneously increases the political efficacy of such revolts. This is because the significantly greater forward-and-backward linkages in manufacturing make it easier to disrupt economies that heavily rely on manufacturing (i.e., strikes in one industry directly affect many other industries that supply or demand goods to/from the disrupted industry). Manufacturing

workers in highly industrialized countries are therefore able to impose unusually high costs on autocratic elites. Once the cost of political suppression under autocracy exceeds the expected cost of redistribution under democracy, autocratic elites stop resisting and democracy becomes a stable political equilibrium.³

In line with this causal mechanism, I find that industrialization strongly increases the probability of mass revolts dominated by industrial workers, that such mass revolts are particularly likely to lead to democratization,⁴ and that accounting for mass industrial worker revolts on the right-hand side of a regression equation explains a significant part of the effect of industrialization on democracy.

While economic structure is one important channel through which stable democracy can arise, it is far from the only factor that matters. Democracy is ultimately a set of institutions that distribute political power toward the majority of the population. This means that there often will be minority groups in society that have an interest in undermining democracy in order to establish some form of minority rule. Given that smaller groups tend to have more homogenous interests and tend to have organizational advantages (Olson, 1965), the survival of democracy ultimately depends on ordinary citizens being willing to defend democracy when it is at risk of being overthrown (Almond and Verba, 1963; Diamond, 1999; Maravall and Przeworski, 2003; Svoboda, 2020; Weingast, 1997).

In the third paper, I examine whether contemporary Americans in fact value democracy enough to overcome partisanship when a choice between partisanship and democracy is presented. To study this fundamental question, I exploit a novel natural experiment created by the fact that Donald Trump's incitement of the January 6 insurrection at the U.S. Capitol unexpectedly occurred while Gallup was conducting a

³Note that the cost of democracy to elites is often relatively limited as they typically find ways to remain disproportionately influential under it (e.g., through campaign contributions or by embedding anti-majoritarian institutions in the new democratic constitution) (Albertus and Menaldo, 2018).

⁴This latter result was established before by Dahlum, Knutsen and Wig (2019).

nationally representative public opinion survey. Because Gallup recruits respondents using random digit dialing, I am able to identify the effect of the January 6 insurrection by comparing party identification among respondents who happened to be interviewed just before, and just after, January 6, 2021. The results of this natural experiment suggest that the Republican Party retained 78% of its pre-insurrection support base during the first 1.5 weeks after the January 6 insurrection. Even this modest loss was only short-lived—in February 2021 the Republican Party already stood at 93% of its pre-insurrection support level. Worryingly, this suggests that for most Republican voters, even a violent attempt to overturn the results of a free and fair election is insufficient to abandon the party.

While the first three papers focus on domestic factors that affect the introduction and sustainment of democracy, we know from previous research that international factors also play an important role (Coppedge et al., 2022). In particular, we know from Boix (2011) that it matters significantly whether the world order is dominated by pro-democratic or anti-democratic world powers. The United States, while largely playing a negative role for democracy during the Cold War (particularly in Latin America), has had a substantial positive effect on global democracy in the post-Cold War period (Levitsky and Way, 2010). A crucial question for global democracy going forward, therefore, is whether the United States can continue playing this role even though it has itself experienced significant democratic backsliding in the recent past.

I examine this crucial question using another novel natural experiment. Similar to paper III, I exploit the fact that the January 6 insurrection at the U.S. Capitol unexpectedly occurred while Gallup was conducting nationally representative public opinion surveys in India, Indonesia, Malaysia, Romania, and Vietnam using random digit dialing. Comparing U.S. leadership approval among respondents who happened to be interviewed just before and just after January 6, 2021, suggests that the insurrection

had *no* effect on U.S. leadership approval abroad. This suggests that American soft power may rely significantly less on the United States actually living up to its “beacon of democracy” mantra than is typically presumed in soft-power theories of international relations (e.g., Nye (2004)). Interestingly, I do find that approval of U.S. leadership increased by approximately 20% when President Trump was replaced by President Biden on January 20, 2021. America’s soft power, therefore, appears to respond to who is in power, rather than to the state of American democracy. I conjecture that this is because the identity of the American president is more likely to have a direct impact on the lives of foreign publics (through U.S. foreign policy) than are domestic political events, however shocking these domestic events may be.

1.2 Contributions to the democracy literature

This dissertation contributes first and foremost to the literature on the causes and effects of democratization and democratic backsliding.

The first two papers contribute to the long-standing debate in political science and economics on the two-way relationship between economic development and democracy. The dissertation questions the common assumption in political science that all aspects of economic development (e.g., economic growth, equality, education, urbanization, and industrialization) tend to “move together” and collectively make democratic forms of government more likely (see Lipset (1959) for the original statement of this view and Treisman (2020) for a recent reiteration). Instead, I show (1) that all currently highly developed countries in the West and East Asia industrialized (and democratized) several decades before they reached high levels of income, equality, education, and urbanization; (2) that changes in income, equality, education, and urbanization share only 18.1%, 4.5%, 27.4%, and 11.1% of their respective variances with changes in industrialization

at the same point in time; and (3) that industrialization has a stronger and more robust effect on democracy than do income, equality, education, and urbanization.⁵

The first two papers also question the common assumption in economics that political institutions tend to cause economic development, rather than vice versa (see most prominently North (1990), Acemoglu, Johnson and Robinson (2001, 2005), and Acemoglu and Robinson (2012)). While it can in principle be true that the causality runs in both directions, the dissertation provides strong evidence for Chang’s (2002) view that political institutions tend to follow, rather than precede, industrialization. In fact, if one considers universal adult suffrage as a necessary condition for calling a country “democratic,” *no* country has ever experienced a significant industrial revolution under a democratic system of government. Instead, democracy was introduced in all currently highly developed countries *after* they had already industrialized.

The third paper of the dissertation contributes to the rapidly growing literature on democratic backsliding in general, and on the role of voters in punishing or tolerating democratic backsliding in particular. Existing studies that have sought to answer the question of whether politicians who clearly violate democratic norms lose significant public support come with important limitations. First, all existing studies are survey experimental in nature.⁶ While clearly valuable, it remains unclear in survey experiments as to whether voters would react differently to real-life democratic transgressions that have the potential of directly affecting their own lives. Second, all existing studies examine important, but nevertheless not truly fundamental and decisive, forms of democratic backsliding (e.g., whether a candidate supported a proposal to reduce the number of polling places in areas that tend to vote for the opposing party). It is

⁵That being said, many other economic factors remain significantly related to democracy even after accounting for industrialization. My theory is thus a complement to, not a substitute for, other theories of the economic roots of democracy.

⁶Graham and Svolik (2020) are an exception to this. They also analyze the electoral effect of Republican Greg Gianforte hitting a journalist in the face. I contribute by focusing on a more extreme form of undemocratic behavior that has had a more structural effect on American democracy.

unclear, therefore, whether American voters would not react more strongly to more extreme forms of democratic backsliding that would truly change the rules of the game (e.g., a president refusing to leave office after losing an election).

I circumvent both of these limitations of the existing literature by studying a novel natural experiment generated by the fact that Donald Trump’s incitement of the January 6 insurrection unexpectedly interrupted a public opinion survey that was conducted using random digit dialing. My result that the American electorate forms little effective constraint on democratic backsliding contrasts sharply with Carey et al. (2020), Touchton, Klostad and Uscinski (2020), and Weingast (1997), who have argued that American politicians have strong incentives to behave democratically because they will otherwise be punished by voters, while aligning closely to the results of Graham and Svobik (2020), McCoy, Littvay and Simonovits (2022), and Albertus and Grossman (2021).

The last paper of this dissertation is, to the best of my knowledge, the first to examine the international consequences of democratic backsliding in the United States. In contrast to the popular hypothesis in international relations that U.S. soft power depends significantly on the United States living up to its mantra of being the world’s “beacon of democracy” (e.g., Nye (2004)), I find that the January 6 insurrection did not have significant impact on support for U.S. leadership in India, Indonesia, Malaysia, Romania, and Vietnam (whose 1.8 billion people alone represent approximately 23.1% of the world population).

1.3 Contributions to development studies

Besides contributing to the literature on democracy and democratic backsliding, the dissertation makes several contributions to the field of Development Studies more generally.

First, papers I and II broaden the discussion on the role of manufacturing in political-economic development. Many scholars in the field of Development Studies have long held that the structural economic properties of manufacturing make it uniquely suited to generate long-term increases in living standards (Amsden (1989, 2001), Chang (1996), Kaldor (1967), Kuznets (1966), Hirschman (1959), and Wade (1990)). I contribute by demonstrating that manufacturing is not only “special” in the economic sense, but also in the political sense—i.e., it is likely to increase a country’s prospects of becoming and remaining a well-functioning democracy. This implies that developing countries that actively seek to industrialize are not only likely to increase their long-run economic prosperity, but also increase the prospect of becoming a more free and fair society. Very much in the interdisciplinary spirit of Development Studies this insight follows from combining development economics theory on the structural economic properties of manufacturing with comparative politics theory on the causes of democratic transitions and consolidations.

Second, paper III contributes to an important line of research in Development Studies on the underlying societal foundations of inclusive political institutions (e.g., Khan (2012)). The paper shows that even in the United States—a longstanding democracy, where citizens are used to the process of elections and have easy access to a wide range of relevant political information—the general electorate cannot be relied upon to effectively sanction undemocratic behavior by elected officials. This gives credence to perspectives in Development Studies that argue that political-economic elites are relatively unconstrained in their capacity to undermine or support inclusive political-economic institutions (e.g., O’Donnell and Schmitter (1986)), and thus underlines the importance of elites behavior and values.⁷ More practically, this result suggests that in democratic developing countries—where in almost all cases democracy has

⁷In other words, the results suggests that elites have substantial agency with regard to what political institutions are established and maintained.

been established relatively recently and is thus arguably less societally ingrained—one should be skeptical of the idea that voters will come to the rescue of democracy if it comes under attack by an anti-democratic state executive. Sadly, in many developing countries exactly such attacks from within are currently ongoing (e.g., Turkey, Brazil, Venezuela, India).

Last, paper IV shows that while constraints on democratic backsliding within the United States are unlikely to come from domestic public opinion, they are also unlikely to come from international public opinion. This is important for the field of Development Studies because we know from existing research that the degree to which the world order is dominated by well-functioning democracies is important for political-economic development around the world. This is both because democratic superpowers tend to be more likely to support democratically-minded opposition movements in dictatorships (with of course many important exceptions, particularly during the Cold War) (Boix, 2011) and because when democracy fails in the United States this tends to negatively reflect on the idea of democracy in yet to democratize countries (Diamond (2021), Haggard and Kaufman (2021), and Nathan (2015)).⁸

1.4 Implications for policymakers

The results of this dissertation paint a relatively bleak picture for the current state of democracy in the world.

The first two papers find that one particularly robust path to stable democratic institutions is having a large fraction of the workforce employed in manufacturing. Unfortunately, most developing (and yet to democratize) countries have in fact *deindustrialized* (from already very low starting points) in recent decades (Palma, 2005; Rodrik, 2016).

⁸Such illustration effects are also well-established with regard to other political-economic metrics. Miller (2016) for example finds that democratic transitions are more (less) likely to occur after decades where the democracies in the world have seen greater (lower) economic growth than autocracies.

This suggests that the tried and tested path from industrialization to democracy is unlikely to come to fruition for many developing countries in coming years, and that in those “prematurely deindustrializing” countries, stable democracy will have to arise for other reasons (e.g., democratic diffusion from other countries—Goldring and Greitens (2020)).

Much of the phenomenon of premature deindustrialization can be explained by the meteoric rise of China as the world’s global manufacturing powerhouse. Because China has been so successful and because it has such a large workforce, it has absorbed many of the manufacturing jobs that would otherwise have existed in other developing countries (Haraguchi, Cheng and Smeets, 2017).⁹

The upside of this process, of course, is that China may at some point democratize through the industrialization channel. This in itself would raise the percentage of the world’s population living under democracy from 52% to 70% (Boese et al., 2022). But although my theory predicts that industrialization has increased the long-run probability that China will become a stable democracy, as compared to what this probability would have been in the absence of widespread industrialization, it is far from certain that China’s current level of industrialization will be sufficient to lead to a successful democratic transition.

First, while it is true that China harbors a very large group of manufacturing workers, China is also the largest country in the world. With the exception of Japan (which arguably democratized “early” because of unrelated processes triggered by its defeat in World War II) China is actually somewhat *less* industrialized than all of today’s highly developed countries were when they democratized. More specifically, the 2021 Chinese Statistical Yearbook suggests that approximately 22.3% of the Chinese workforce was employed in manufacturing in 2020. The level of industrialization that existed in the

⁹Another important cause of premature deindustrialization is automation (Rodrik, 2016).

West, Taiwan, and South Korea meanwhile was at least 22.9%—and typically more than 25%—at the time of democratization (median=27.0%; mean=28.5%).¹⁰

Second, the pre-democratic political structure in China may differ in relevant ways from the pre-democratic political structure in the countries where industrialization appears to have caused democratization in the past. Unlike in the West, South Korea, and Taiwan the current regime in China was the result of a violent revolution, and autocratic regimes that are born in a violent revolution have proved to be unusually resilient because they tend to have a ruling elite that is particularly cohesive, particularly loyal, and particularly ruthless in disseminating alternative centers of political organization (Levitsky and Way, Forthcoming). It is possible therefore that the evidence that we can derive from the past will not be representative for the specific case of modern China.

Taken together the results of the first two papers highlight the need for public policies that increase the fraction of the workforce employed in manufacturing, particularly in yet to democratize countries. As such the papers provide “political” justifications for the use of industrial policy.¹¹ Many development economists have long argued that policies specifically targeted at manufacturing development are crucial for successful economic development (e.g., Amsden (1989, 2001), Chang (1996), Kaldor (1967), Kuznets (1966), Hirschman (1959), and Wade (1990)). The key idea is here that while industrialization is particularly conducive to high and widespread economic growth, developing a productive manufacturing sector is subject to significant market failures as a result of demand complementarities, externalities in R&D investments, learning by doing, and economies of scale in risk taking (see Chang and Andreoni (2016) for an excellent survey). The need for industrial policies is controversial in economics, however,

¹⁰See section 2.5.1 of paper I for more details on how these results are obtained.

¹¹Following Pack and Saggi (2006) I define industrial policy as any type of selective government intervention that attempts to alter the structure of production in favor of manufacturing in a way that would not have occurred in the absence of such intervention.

with most mainstream economists considering industrial policies either unnecessary (e.g., Pack and Saggi (2006)) or harmful (e.g., Beason and Weinstein (1996)); although this view may be changing now that several quasi-experimental papers have found important positive effects of industrial policies (e.g., Criscuolo et al. (2019) and Juhász (2018)). The papers in this dissertation do not intend to contribute to this debate within economics. Instead, the papers highlight how a manufacturing-focused development strategy is important for political development (i.e., democracy) besides any potential economic advantages or disadvantages.

Paper III uncovers another bleak picture for the state of democracy today—i.e., the fact that for many Republican voters even a violent insurrection to overturn the results of a free and fair election is insufficient to abandon the Party. While it is unclear whether the American electorate was an unconditional supporter of democracy in the past (or whether the American electorate was not, but democracy survived anyway), this finding highlights the urgent need for public policies that would increase the likelihood that American voters will strongly oppose overtly undemocratic politicians. Such policies should find ways to increase the general public's genuine commitment to democracy and/or should focus on reducing political polarization, so that the “cost” of defecting from one's own preferred political party/candidate, in case it acts undemocratically, is lowered (Graham and Svolik, 2020; Svolik, 2020). While American democracy survived the Trump era it is far from certain that it will survive in the future if large parts of the electorate remain willing to prioritize partisanship over democratic principle.

Paper IV shows that the January 6 insurrection did not significantly reduce America's soft power abroad. This is a positive finding for global democracy if one follows Levitsky and Way (2010) in assuming that the United States has had a net positive effect on democracy in the world since 1990. Importantly, however, paper IV also found

that support for U.S. leadership increased significantly after January 20, 2021, the day that president Biden was inaugurated. This suggests that while undemocratic events within the United States may not (as of yet) affect America's standing abroad, electing Trump-like figures to office may well reduce America's soft power,¹² and by extension may well undermine some of the foundations of the liberal democratic world order.

¹²See Bateson and Weintraub (Forthcoming) and Carreras, Visconti and Acácio (2021) for quasi-experimental evidence that further supports this view.

Chapter 2

Paper I: Industrialization and Democracy

Abstract

I provide a new theory of the relationship between economic development and democracy. I argue that a large share of employment in manufacturing (i.e., industrialization) makes mass mobilization both more likely to occur and more costly to suppress. This increases the power of the masses vis-à-vis autocratic elites, making democracy more likely. Using novel manufacturing employment data for 145 countries over 170 years (1845–2015) I find that industrialization is strongly correlated with democracy, even after accounting for country and time fixed effects, time trends, theoretically-grounded controls, and other economic determinants of democracy (e.g., income and inequality). Unlike with other economic determinants the effect occurs on both transitions and consolidations, and is equally large after 1945. Importantly, many potential outliers (e.g., China, USSR, Latin America during ISI) have in fact never reached the level of industrialization that existed in West, South Korea, and Taiwan before democratization.¹

¹I am grateful to Fernando Bizarro, Dawn Brancati, Brian Burgoon, Richard Clark, Andrew Eggers, Grzegorz Ekiert, Patrick Emmenegger, Tanushree Goyal, Hanna Kleider, Steven Levitsky, Michael Miller, Julie Rabinowitz, Peter Rosendorff, David Rueda, David Samuels, Jan Vogler, and seminar participants at Oxford University, Princeton University, Harvard University, Yale University, University College London, the Historical Political Economy Working Group, and the Annual Meetings of the American, Midwest, and European Political Science Associations, for helpful comments and suggestions.

2.1 Introduction

In 1988, South Korea transitioned from a military dictatorship to a democracy. Many scholars agree that this democratic transition was, in no small part, due to the process of rapid economic development that occurred in South Korea between 1963 and 1988 (e.g., Ahn (1997), Armstrong (2007), Huntington (1991), and Kim (2000)). Yet, when one focuses on the common economic variables used in the existing democratization literature—i.e., income (Boix, 2011; Epstein et al., 2006; Miller, 2012; Treisman, 2015), income equality (Boix, 2003; Acemoglu and Robinson, 2006), education (Glaeser, Ponzetto and Shleifer, 2007; Dahlum and Wig, 2019, 2021), and urbanization (Beissinger, Forthcoming; Cirone, 2021; Fresh, 2022)—it seems difficult to explain why South Korea democratized in 1988 at all. For example, of the countries that the Varieties of Democracy (V-dem) Project coded as nondemocracies in 2015, 34.8% had higher levels of GDP per capita (PPP), 14.0% had lower levels of Gini inequality, 15.8% had more years of education, and 41.3% had a higher level of urbanization than did South Korea in 1988.² As I show later in this paper, all the other currently highly developed countries in the West and East Asia also democratized on levels of income, equality, education, and urbanization that are relatively low even by the standard of many of today’s most autocratic developing countries.

I argue that this discrepancy occurs because a shift towards a large share of employment in manufacturing (i.e., industrialization) is an important overlooked channel through which economic development can lead to democracy.³ I provide novel microfoundations for this hypothesis and use novel long-run manufacturing

²I here use the 5 category ordinal version of V-dem’s electoral democracy index. Income data comes from Bolt et al. (2018). Gini inequality data comes from Kang (2001, p. 31) for South Korea (1988) and from the World Bank Development Indicators for currently autocratic countries. Years of education data comes from Lee and Lee (2016). Urbanization data comes from Coppedge et al. (2021) and the World Bank Development Indicators.

³I refer to a shift towards a large share of manufacturing in total employment as “industrialization”. Note that confusion may arise because “industrialization” is sometimes equated with economic growth (e.g., Fukuyama (2014)) or urbanization (e.g., Fresh (2022)).

employment data to show that industrialization is robustly related to both transitions to and consolidations of democracy. The South Korean case illustrates the strength of this hypothesis. In 1988, 24.2% of the South Korean workforce was employed in manufacturing. No country (including China and the USSR) has ever reached this level of industrialization without eventually becoming a stable democracy.

The key causal mechanism that I highlight is how a large share of employment in manufacturing makes mass political mobilization both more likely to occur and more costly to suppress. I derive this prediction from three well-established structural economic properties of manufacturing: deep divisions of labor, economies of scale, and linkages. Unlike agriculture, mining, and petty services (the predominant economic sectors in little industrialized countries) manufacturing production combines significant economies of scale with a large potential for fine-grained labor specialization. Manufacturing capitalists have a strong incentive to exploit these features to reduce production costs and maximize profits. Exploiting these features typically requires the establishment of large firms, or network of firms, where many workers work together in long and complex production chains. Doing so efficiently requires instilling manufacturing workers with a wide range of organizational capabilities that foster cooperation on a large and impersonal basis (e.g., negotiating and working together with strangers, addressing disagreements, disciplining free riders). Many of these organizational capabilities are transferable to mass organization in general, and, therefore, unintentionally increase manufacturing workers' capacity to mobilize politically. Importantly, industrialization not only increases the capacity of manufacturing workers to organize mass political revolts, but also increases the political efficacy of such revolts. This is because the significantly greater forward and backward linkages in manufacturing make that economies that heavily rely on manufacturing are more easily disrupted by political mobilization (e.g., strikes in one industry directly affect

many other industries that supply or demand goods to/from the disrupted industry). Manufacturing workers in highly industrialized countries are therefore able to impose significantly higher economic costs on autocratic elites, as compared to disenfranchised groups in little industrialized countries. Once the cost of political suppression exceeds the cost of (potential) redistribution autocratic elites stop resisting and democracy becomes a stable political equilibrium.

To test this theory I assemble a new dataset on manufacturing employment and democracy for a sample of 145 countries over 170 years (1845–2015). I generate this data by scraping the census data incorporated in Mitchell (2013), and combining this with manufacturing employment data from both the United Nations Industrial Development Organization (UNIDO) and the Groningen Growth and Development Centre (GGDC).

Using this novel data, I report four key findings.

First, industrialization is clearly empirically distinct from the other more widely studied economic determinants of democracy. While Lipset (1959) famously argued that the factors of industrialization, urbanization, wealth, equality, and education are so closely related to each other that they can better be considered as attributes of the same underlying factor, I show that this is only because all highly developed countries eventually tend to reach high levels of industrialization, urbanization, income, equality, and education. When one looks within countries over time, however, one sees that all currently highly developed countries in the West and East Asia have reached high levels of manufacturing employment several decades *before* they reached high levels of income, equality, education, and urbanization. In contrast to Lipset (1959), changes in industrialization within countries over time only share 18.1%, 4.5%, 27.4%, and 11.1% of their variance with changes in income, equality, education, and urbanization *at the same point in time*.

Second, I show that the timing of democratization in the currently most highly developed countries in the West and East Asia is consistent with the hypothesis that industrialization tends to induce democracy. Virtually all of today's highly developed countries democratized shortly after reaching high shares of employment in manufacturing, and all democratized on levels of industrialization virtually unprecedented by any undemocratic country today or in the past. Again highlighting the importance of studying the effect of industrialization as distinct from other standard economic variables, I also show that all of today's highly developed countries democratized on levels of income, equality, education, and urbanization that are relatively low even by the standard of many of today's autocratic developing countries.

Third, I find that industrialization is strongly positively correlated with democracy. Crucially, I find that many of the existing empirical challenges to the hypothesis that economic development tends to induce democracy disappear when one focuses on industrialization rather than on income, equality, education, or urbanization. In particular, I find that the effect remains robust to country and time fixed effects (Acemoglu et al., 2005, 2008), occurs on both transitions to and consolidations of democracy (Houle, 2009; Przeworski and Limongi, 1997), is equally present in more recent time periods (Boix and Stokes, 2003)⁴, and occurs in both the long and the short term (Treisman, 2015). Using standardized regression coefficients I find that the effect of industrialization on democracy is at least several times larger than the effect of income, inequality, urbanization, and education, as well as many other standard determinants of democracy (e.g., oil rents).⁵ While my results remain correlational I do

⁴More specifically, the effect size of industrialization remains roughly the same, while the explained variance moderately declines after 1945, and particularly after 1975. This is because while industrialization also tended to lead to democratization after WWII (e.g., South Korea, Taiwan, etc.), there were many more countries in the post-WWII period that democratized for other reasons than industrialization.

⁵That being said, many countries that are democracies today never reached high levels of industrialization, and must thus have democratized for other reasons. Industrialization is far from the only factor that matters for democracy.

find that assigning the entire association between industrialization and democracy to endogeneity requires the effect of unobserved confounders to be considerably stronger than the strongest combination of observed co-determinants of industrialization and democracy highlighted in the existing literature (in addition to country and time fixed effects).

Last, I find that industrialization strongly increases mass political protests dominated by industrial workers over the 1900 to 2006 period, and that such protests are particularly likely to lead to democratization.⁶ In line with my causal mechanism I find that accounting for mass industrial worker protests on the right-hand side of a regression equation accounts for a significant part of the effect of industrialization on democracy.

The paper contributes to the large literature on the economic roots of democracy. It does so by showing that industrialization has an important effect on democracy net of other, more widely studied, economic variables (e.g., income and inequality). It is important to note here that I see the role of industrialization as complementary (not as a substitute) to other more widely studied economic determinants of democracy. Indeed, it is perfectly possible that higher levels of income, equality, education, urbanization, and industrialization all to different degrees, and under different conditions, have an independent effect on democracy.

The paper is most closely related to the seminal work of Rueschemeyer, Stephens and Stephens (1992). Rueschemeyer, Stephens and Stephens (1992) argue that “capitalist development”—defined as economic development driven by capital interests in competition with each other (p. 1)—leads to democracy because it increases the organizational capacity of the working class. While my theory is related through its focus on how economic development increases the organizational capacity of disenfranchised groups, it differs in terms of both the driving force and the causal mechanism

⁶This latter result was established before by Dahlum, Knutsen and Wig (2019).

linking economic development with democracy. Rather than “capitalist development,” I identify manufacturing employment as the key independent variable. This is significant because “capitalist development” (as defined by Rueschemeyer, Stephens and Stephens (1992)) includes many economic production processes that do little to change a country’s socio-economic structure (e.g., tourism), or may, in fact, move a country’s socio-economic structure in an anti-democratic direction (e.g., natural resource extraction – Ross (2015)). Furthermore, my theory is not restricted to the working class. A significant number of members of the urban middle class in highly industrialized countries work as managers and engineers in the manufacturing sector, and this group often plays an important role in the establishment of democracy.⁷ If it was simply a working-class—as opposed to a manufacturing—story one would expect to find that employment in other more working-class dominated sectors (e.g., mining and construction) would also positively affect democracy. I do not find this to be the case.

My theory is also related to the seminal work of Llavador and Oxoby (2005). Llavador and Oxoby (2005) theorize how partial extensions of the franchise in the 19th century may have been the result of industrialists strategically enfranchising higher earning industrial workers, but not agricultural workers, to shift public spending towards public goods that large landowners opposed. Llavador and Oxoby’s (2005) model is specific to the first wave of democratization (with its process of limited suffrage extensions) and to cases with powerful landed elites (which is not applicable to several important cases where industrialization plausibly led to democratization – e.g., South Korea). I show that industrialization can in fact explain transitions to *full* democracy in all waves of democratization. I do so by arguing that besides the top-down mechanism highlighted by Llavador and Oxoby (2005), industrialization also increases the pressure for democracy from below.

⁷This only notably changes with the shift to modern service employment, which typically only occurs several decades after industrialization and democratization have already taken place (see section 2.4 below).

While novel, my theory naturally builds upon a wide range of existing work in the democratization literature. First, my theory argues that industrialization makes democracy more likely is through increasing the mobilization capacity of disenfranchised groups. This links up to a longstanding literature that argues that mass mobilization is often an important part of democratic transitions and, importantly, that such mobilization is made difficult by a wide range of difficult to overcome collective actions problems (e.g., Aidt and Franck (2015), Aidt and Franck (2019), Aidt and Jensen (2014), Aidt and Leon (2016), Dahlum, Knutsen and Wig (2019), Glaeser, Ponzetto and Shleifer (2007), Olson (1965), and Rueschemeyer, Stephens and Stephens (1992)). Second, my theory argues that autocratic elites, when pressured from below, tend to weight the cost of maintaining autocracy against the expected cost of redistribution under democracy. This part of the theory relates very directly to the seminal political economy models of Acemoglu, Johnson and Robinson (2005) and Boix (2003). Last, and unlike the political economy models of Acemoglu, Johnson and Robinson (2005) and Boix (2003), my theory argues that it is not only the prevailing level of inequality that enters into elite calculations, but also the economic cost of repression itself. This part of the theory relates closely to the work of Rosendorff (2001).

2.2 Theory

2.2.1 Defining democracy

Following Coppedge et al. (2021) I define democracy as a system of government that seeks to make rulers responsive to citizens through electoral competition for the electorate's approval under circumstances when (1) suffrage is (very close to) universal among the adult citizenry; (2) political and civil society organizations can operate freely; (3) elections are clean and not marred by fraud or systematic irregularities; (4)

elections affect the composition of the chief executive of the country; and (5) there is freedom of expression and an independent media capable of presenting alternative views on matters of political relevance.

2.2.2 Manufacturing workers' incentive to push for democracy

Prior to democratization the government is typically controlled by a small group of capitalists and/or large landowners. Both these groups tend to strongly oppose democracy for two reasons. First, being a rich and small minority makes that the one-person, one-vote rule underlying democratic elections risks significant redistribution towards the poorer majority (Acemoglu and Robinson, 2006; Boix, 2003). Second, both capitalists and large landowners tend to derive significant economic benefits from labor-repressive economic institutions that are difficult to sustain under democracy. These labor-repressive economic institutions tend to take the form of restrictions on labor migration in the case of large landowners, and little to no regulation on wages and working conditions for capitalists (Moore, 1966). While capitalists and large landowners may occasionally have other opposing political-economic interests, these are typically not decisive enough for either group to band together with the masses and push for full democracy (Przeworski, 2009; Rueschemeyer, Stephens and Stephens, 1992).

While most ordinary people are typically disadvantaged under autocracy, manufacturing workers are especially likely to have a strong interest in political change under autocracy. First, manufacturing workers are due to the nature of their work at high risk of negative life-cycle shocks (e.g., unemployment, sickness) while they have neither the traditional coping mechanisms provided by agricultural communities (e.g., substituting wage labor for subsistence farming) nor the greater wealth of economic elites

to effectively deal with such risks (Korpi, 2006). Second, factory work itself tends to generate particularly harsh socio-economic conditions for manufacturing workers (e.g., long hours, little labor autonomy, and often a highly hierarchical and labor-repressive management regime). Manufacturing workers, therefore, tend to have a particularly strong interest in gaining power over government policies with regard to such issues as labor regulation and welfare benefits, as well as over more directly redistributive issues such as progressive taxation.

Rather than bargain for selective policy concessions by the existing autocratic regime or attempt to establish their own autocratic regime, manufacturing workers in highly industrialized countries are likely to demand democracy for two reasons. First, manufacturing workers are unlikely to be satisfied with selective policy concessions of the existing autocratic regime because selective policy concessions from a government controlled by a group with opposing economic interests cannot be credible in the long term (Acemoglu and Robinson, 2006). Second, establishing a regime that solely tailors to manufacturing workers' own interests is typically not feasible. Doing so would require the structural exclusion of all higher social classes, which, due to their stronger socio-economic position, generally have sufficient resources to remain politically influential (Rueschemeyer, Stephens and Stephens, 1992).

Conditional on being unable to exclude higher classes, manufacturing workers in highly industrialized countries have an interest in supporting a regime that would enfranchise *all* classes. In highly industrialized countries before democratization approximately 25% to 30% of the workforce is typically employed in manufacturing (see Table 2.8 below). At this stage of development poorer farmers, petty service workers, and paupers together typically represent more than 25% but less than 50% of the workforce (Osamu and Shaw-Taylor, Forthcoming). This suggests that manufacturing workers in highly industrialized countries not only typically incorporate the economic

median voter position (which ensures them significant influence over government policy under the one-person, one-vote rule of democracy)⁸ *but also* that they would typically lose this position if they excluded any other social group (i.e., if they exclude the poor they would typically fall well below the median voter position).

2.2.3 Manufacturing workers' capacity to push for democracy

Industrialization not only generates the incentive among manufacturing workers to push for democracy, it also provides manufacturing workers with the organizational capacity to do so.⁹

Unlike agriculture, mining, and petty services (the predominant economic sectors in little industrialized countries) manufacturing production combines significant economies of scale with a large potential for labor specialization (through dividing the production process into separate distinct tasks). Manufacturing capitalists interested in profit maximization have a strong incentive to exploit these features because specialization tends to increase labor productivity (Smith, 1776), and increasing the size of production plants in industries with economic of scale reduces the cost per unit produced (Mankiw and Taylor, 2011). In order to exploit these features capitalists need to establish large firms, or network of firms, where many workers work together in long and complex production chains. Making this work efficiently requires instilling workers and managers

⁸Ansell and Samuels (2014) argue that industrial workers cannot be motivated to push for democracy through this channel because they typically are still a small group situated well above the economic median voter at the time of democratization. They primarily come to this conclusion based on social tables from the United Kingdom in the year 1867. Importantly, however, the Reform Act of 1867 enfranchised only about 15% of the adult population. When the United Kingdom introduced universal suffrage in 1928, it employed approximately 34.7% of its workforce in manufacturing and this group almost certainly incorporated the economic median voter (see evidence from social tables in Appendix 2.8.1).

⁹This contrasts sharply with peasants, who tend to be the economic median voter prior to widespread industrialization. Dahlum, Knutsen and Wig (2019) find that significantly fewer mass revolts focused on political change originate among the peasantry, and that revolts dominated by peasants, if anything, tend to negatively affect democracy (Tables A.1, A.2, and A.28 of Dahlum, Knutsen and Wig (2019)).

with a wide range of organizational and attitudinal capabilities that foster cooperation on a large and impersonal basis (e.g., negotiating and working together with strangers, dealing with disagreements, disciplining free riders). Many of these capabilities are transferable to mass organization in general and, therefore, can also be used to organize politically (Marx, 1867). It is no coincidence, therefore, that many significant “political” civil society organizations—for example, labor unions—traditionally originated in industry (Marks, 2016).¹⁰

In line with these economic micro-foundations Butcher and Svensson (2016), Dahlum, Knutsen and Wig (2019), and Tilly (1978) find that in more highly industrialized countries mass political protests tend to be more frequent, larger, and better coordinated. Butcher and Svensson (2016) shows that this is true regardless of the level of urbanization (which itself does not predict major nonviolent campaigns after accounting for industrialization). This suggests that the causal mechanism is likely to be about organizational capacity rather than (only) agglomeration.

2.2.4 Industrialization and the cost of suppression

Industrialization not only increases the pressure for democracy from below, it also makes it more costly for elites to repress such demands.

As compared to other economic sectors manufacturing production tends to rely on significantly more forward and backward linkages (Hirschman, 1959). These supply-chain linkages make that economic production in highly industrialized countries is much more strongly disrupted by socio-political instability than economic production in non-industrialized societies (e.g., strikes in one industry directly affect many other industries that supply or demand goods to/from the disrupted industry) (Acemoglu and Robinson, 2006). Furthermore, because mass revolts in highly industrialized countries

¹⁰This is even while industrialists and autocratic elites typically do everything they can to suppress such organizations from originating in the first place (Marks, 2016).

tend to be more frequent, larger, and better coordinated, repressing these protests is significantly more expensive in terms of police and defense costs.¹¹

Thus, industrialization not only increases manufacturing workers' incentive and organizational capacity to generate mass political revolts, these industrialization-induced revolts also tend to be more costly for autocratic elites. Once the cost of repression rises sufficiently, autocratic elites find it in their own best interest to tolerate democratization as it is cheaper than continuing political repression (Boix, 2003; Rosendorff, 2001).¹²¹³

2.2.5 Industrialization and democratic consolidation

Manufacturing employment typically remains high for many decades after democratization. The same socio-economic structure that led to the initial democratic transition therefore remains largely intact, supporting democratic consolidation. Note, for example, that even in 1970, decades after their respective democratic transitions, manufacturing employment in the United Kingdom, the United States, France, West Germany, and Sweden still stood at 31.7%, 22.2%, 25.4%, 35.8%, and 28.2% respectively (calculated based on the 10-sector database of the GGDC).

However, manufacturing employment does not remain consistently high over the course of a country's economic development. Today, only around 10% of Western

¹¹Note that the cost of repression in autocracies is significant to begin with; even when holding income levels and exposure to warfare constant, autocracies spend about 40% more on their militaries than do democracies (Brauner, 2015).

¹²Note that the cost of democracy to elites is often relatively limited as they typically find ways to remain disproportionately influential under democracy (e.g., through campaign contributions or by embedding anti-majoritarian institutions in the new democratic constitution) (Albertus and Menaldo, 2018).

¹³Note that I do not intend to claim that democratization always arises because of, or even necessarily in the presence of, significant pressure from below. Indeed, democratic transitions can also be elite-led (Albertus and Menaldo, 2018; Riedl et al., 2020) or simply due to elite-level miscalculation (Treisman, 2020). I do claim, however, that industrialization-induced transitions often arise at least in part due to the political mobilization of manufacturing workers. I provide empirical evidence for this claim in section 2.6.6.

working populations are engaged in manufacturing.¹⁴ While this is still relatively high from a cross-national perspective, this does beg the question: What stabilizes democracy in the long term?¹⁵ Why would the middle class that is employed in the modern service sector in post-industrial societies not opt for a political regime that structurally excludes the lower classes? Such a bargain was ultimately preferred by many middle classes prior to deep and widespread industrialization (Rueschemeyer, Stephens and Stephens, 1992).

I argue that middle classes in post-industrial societies chose not to (attempt to) exclude the lower classes for two reasons.

First, middle class modern service workers in post-industrial societies have little incentive to disenfranchise the poorer segments of society because they themselves tend to incorporate the economic median voter. Prior to industrialization, the middle class tends to be a small social group situated well above the median income (Ansell and Samuels, 2014). Universal suffrage for middle class actors prior to widespread industrialization thus means relinquishing significant control over socio-economic policies to the much more numerous and much poorer lower classes. After the shift from manufacturing employment to modern service employment has taken place, however, a majority of the population could reasonably be classified as middle class, and, therefore,

¹⁴The shift towards modern service employment is itself largely the result of successful industrialization. This occurs because labor productivity tends to increase significantly faster in manufacturing than services, while the relative demand for manufactured goods and services remains roughly the same over the course of development. This means that, over time, more and more labor has to shift from manufacturing to services to fulfill demand in highly industrialized countries (Rowthorn and Wells, 1987). Modern services, as opposed to the petty services that tend to dominate little industrialized countries (e.g., cab drivers, street sellers, domestic servants), tend to only arise after industrialization because many modern services are to a large extent inputs (e.g., banking, design, R&D, consultancy) or outputs (e.g., retail, marketing) of manufacturing. In the absence of a large and productive domestic manufacturing sector modern services tend to remain a small enclave of the economy enriching a small number of high-skilled service workers, while leaving most of the rest of the economy untouched (e.g., the direct and indirect jobs generated by India's "high-tech" service sector employ, at best, 2% of the total Indian workforce) (Rodrik, 2014).

¹⁵It is possible that the fact that manufacturing employment typically remains high for several decades after democratization is in itself sufficient for democratic consolidation. Note, for example, that the probability of an authoritarian reversal is close to zero after a democracy has been in existence for more than 20 years (Svolik, 2008).

the middle class always contains the economic median voter position. Under such conditions, universal suffrage relinquishes significantly less policy control to the poorer segments of society.

Second, middle class modern service workers in post-industrial societies have much less capacity to disenfranchise the poorer segments of society. Unlike the case of pre-industrial middle classes, the lower classes in post-industrial service economies are no longer poor and disorganized farmers and petty service workers. Instead, they are relatively well-organized and politically conscious construction, mining, manufacturing, and health care workers with significant disruptive capacity. Therefore, the poor and working class can no longer be excluded without significant costs to middle- and upper-class lives (through, for example, strikes in crucial economic sectors).

2.3 Relationship with existing literature on the economic roots of democracy

Much has been written about the relationship between economic development and democracy. It is useful therefore to explicitly outline how my theory on the relationship between a shift towards a large share of employment in manufacturing (i.e., industrialization) and democracy differs from closely related work on the economic roots of democracy. I here do so by discussing how my theory differs from the seminal work of Acemoglu and Robinson (2006), Ansell and Samuels (2014), Boix (2003), Inglehart and Welzel (2005), Lipset (1959), and Lizzeri and Persico (2004).

2.3.1 Acemoglu and Robinson (2006), Ansell and Samuels (2014), and Boix (2003)

There exists a large literature on the relationship between economic inequality and democracy. The seminal references for this literature are: Acemoglu and Robinson (2006), Ansell and Samuels (2014), and Boix (2003).¹⁶

This literature is highly diverse but in the aggregate differs from my theory in three fundamental ways. First, I focus on employment patterns, as opposed to output (income/wealth) patterns. Second, I focus on the *level* of industrialization, as opposed to the *distribution* of income/wealth. Third, my theory focuses on how industrialization changes both the incentive and the capacity of different social groups to push in favor or against democracy. Acemoglu and Robinson (2006), Ansell and Samuels (2014), and Boix (2003) focus only on the incentive of different groups to favor or oppose democracy (this incentive, in these theories, is affected by different degrees and structures of economic inequality).¹⁷

2.3.2 Inglehart and Welzel (2005)

With Inglehart and Welzel (2005) I share the belief that industrialization plays an important role in democratization. Crucially, however, Inglehart and Welzel (2005) argue that a shift from agricultural employment to industrial employment leads to secular-rational values which *retard* democracy (see, for example, pages 36 and 59). For Inglehart and Welzel (2005) it is only a shift *away* from industrial employment towards modern service employment that leads to democracy because, they argue,

¹⁶See also Freeman and Quinn (2012), Houle (2009), and Rosendorff (2001), among others.

¹⁷In Ansell and Samuels' (2014) theory inequality is not in itself causing democracy. Instead, rapidly increasing inequality is simply proxying for an economically rising yet politically disenfranchised social group which has an incentive to push for enfranchisement in order to protect its newly acquired wealth.

post-industrial service work tends to induce self-expressive values. I differ in two fundamental ways from this perspective.

First, my theory and empirical results directly contradict Inglehart and Welzel's (2005) expectation that a *decline* in industrial employment leads to democracy (conditional on having achieved a high level of industrialization in the past). Instead, I find an unconditionally and very large positive effect of the percentage employed in manufacturing on democracy. This result makes sense from a historic point of view. Note, for example, that the post-industrial service economy, as well as the post-material "self-expressive" values highlighted by Inglehart and Welzel (2005), only arose decades after all currently highly developed countries in the West and East Asia had already democratized (i.e., after +/- 1960 in the West, after +/- 1980 in Japan, and after +/- 2000 in South Korea and Taiwan).

Second, I differ on the mechanism through which economic development induces democracy. Inglehart and Welzel (2005) argue that economic development leads to an increased sense of economic security, which in turn leads to "self-expressive" values, which, they assume, are sufficient for democracy to be established and sustained. This implicitly assumes that the wealthiest people in society are the strongest proponent of democracy (as they are economically most secure), which is in direct contradiction with existing theory and empirical evidence (e.g., Acemoglu and Robinson (2006), Boix (2003), Rueschemeyer, Stephens and Stephens (1992)). My theory takes a political economy approach, focusing on how industrialization changes the incentive and capacity of different social groups to support/oppose democracy; which is in line with much of the existing literature and evidence on democracy.

2.3.3 Lipset (1959)

I share with Lipset (1959) the belief that economic development tends to make democratic forms of government more likely. My theory nonetheless differs from Lipset (1959) in two fundamental ways.

First, my theory differs with regard to what particular type/form of economic development matters for democracy. Lipset (1959) regarded industrialization as simply one part of a broader modernization process, including other variables such as income and education, which in turn *collectively* cause democracy (e.g., p. 80). Lipset (1959) did therefore never develop a theory of how industrialization affects democracy in particular, and if anything, Lipset's (1959) idea was that if any of the factors of income, education, urbanization, and industrialization did not "move together" this would lead to negative effects on democracy (pp. 81–83). I differ from this perspective by showing that it is industrialization that tends to induce democracy, and that many of the other structural factors highlighted by modernization theory do, first, not tend to develop at the same point in time as industrialization does, and, second, do not tend to have much effect on democracy after accounting for industrialization.

Second, my theory differs in terms of the causal mechanism that links economic development with democracy. For Lipset (1959) economic development leads to democracy through four causal mechanisms (pp. 83–85). First, economic development increases a population's standard of living and literacy, which in turn allows for the development of norms of tolerance, compromise, and gradualism, norms which Lipset (1959) assumes are necessary for democracy. Second, economic development tends to reduce the gap between the rich and the poor, which leads to an upper class that sees the lower class as more worthy of equal political rights. Third, a higher level of income increases a population's tolerance of democracy as it does not make too much difference if some redistribution does take place. Last, greater wealth increases the size

and strength of civil society, which can function as a source of countervailing power, and acts as a training ground for new political ideas/leaders. My theory differs from Lipset (1959) by explaining which agent (manufacturing workers) tends to push for democracy and why this agent has both the incentive and the organizational capability to do so. Lipset (1959) does not provide a microfounded theory of democracy. If anything, it is the middle class that Lipset (1959) sees as the primary promoter of democracy. However, Lipset (1959) does not explain why the middle class would have the organizational capacity to push for democracy, and he does not explain why the middle class would have an interest in democracy, as opposed to a regime that benefits them to the exclusion of others.

2.3.4 Lizzeri and Persico (2004)

Lizzeri and Persico (2004) provide a formal model that is closely related to that of Llavador and Oxoby (2005). In Lizzeri and Persico's (2004) model elites that benefit from public goods have an incentive to extend the franchise because increasing the number of voters reduces the fraction of the electorate that can be wooed with *ad hominem* promises and therefore, by comparison, increases the electoral value of policies with diffuse benefits. My theory differ in two important respects from Lizzeri and Persico (2004).

First, Lizzeri and Persico (2004, pp. 713-714) argue that it was urbanization, not manufacturing employment, that increased the value of public goods in 19th century Britain, which in turn lead to democratization. I show that urbanization is actually relatively weakly correlated with democracy after accounting for industrialization, and that all currently highly developed countries in the West and East Asia democratized on levels of urbanization that are low even by the standard of many of today's autocratic developing countries.

Second, my theory assumes that in rapidly industrializing countries democracy is rarely granted by elites without any pressure from below.¹⁸ This is because while some economic elites may have an interest in better public goods provision this interest is generally triumphed by the greater risk of redistribution and more pro-labor socio-economic policies under democracy (Rueschemeyer, Stephens and Stephens, 1992). This assumption is in line with a large literature on the history of democratization in all countries that ever reached high levels of industrialization (e.g., Collier (1999), Kim (2006), Przeworski (2009), and Rueschemeyer, Stephens and Stephens (1992)). Furthermore, the results in Section 5.6 of the main text also strongly suggest that mass revolts by industrial workers mediate a significant part of the effect of industrialization on democracy.

To be sure, however, I do not exclude that some of the top-down mechanisms that Lizzeri and Persico (2004) and Llavador and Oxoby (2005) highlight may also sometimes contribute to democratization. I see my theory as a complement to, not as a substitute of, Lizzeri and Persico's (2004) and Llavador and Oxoby's (2005) seminal models.

2.4 Measurement and data

Empirically testing the effect of industrialization on democracy is made difficult by the fact that existing datasources do not provide data on manufacturing employment for before 1950, and for most countries only contain data since 1970, or even later. I circumvent this data challenge by scraping census data on manufacturing employment from the hardcopy accounts of Mitchell (2013), and by combining this data with

¹⁸This is not to deny the possibility that in other cases (particularly in little industrialized countries during the Third Wave) elites may have sometimes seen it in their own economic interest to support democratization without any pressure from below. This was primarily the case because autocrats in those latter cases were less reliable partners of economic elites than government in all currently highly developed countries before democratization (Albertus and Gay, 2017).

manufacturing employment data from the UNIDO INDSTAT2 database and the GGDC 10-sector database.

Mitchell (2013), GGDC, and UNIDO all ensure, as best as possible, that their estimates on employment in the manufacturing sector remain comparable over time. Importantly, however, the UNIDO estimates for any given country-year tend to be marginally lower than the estimates from Mitchell (2013) and GGDC. This is because UNIDO classifies manufacturing workers on the basis of the nature of their job, rather than on the basis of whether they work for a company that is predominantly engaged in manufacturing. To avoid sudden jumps in the time-series due to changes in the data source, rather than real changes in a country's number of persons employed in manufacturing, I use in all cases one data source per country and add country fixed effects in all specifications. All results hold when creating the most extensive panel possible by imputing (in any order) the UNIDO, GGDC, and Mitchell (2013) data, and when restricting the analysis to the use of only one data source for the entire sample.

To choose which datasource to use for any individual country I simply select for each country the datasource that maximizes the number of years for which data for that individual country is available. Before doing so I linearly interpolate the data using the following two rules: (1) I drop country-years if the available data requires me to interpolate for more than 15 years; and (2) if (1) happens I also drop the years prior to this point to maintain a continuous time series for each country.¹⁹ All results hold with no or unrestricted interpolation.

Because UNIDO does not provide data on the total working population I account for differences in country size by calculating for each country-year the percentage of the *total* population that is employed in manufacturing. To do so I use data on population

¹⁹The only exceptions to these rules are the United Kingdom and the Netherlands. For these countries I interpolate more than 15 years from 1931 to 1951 and from 1930 to 1947, respectively. Not doing so would drop 86 and 58 years of qualified data before WW II (which interrupted the censuses in these countries). Dropping these years makes no substantive difference to the results.

size from Bolt et al. (2018), Fink-Jensen (2015), and Haber and Menaldo (2011). All results hold when using the population that is between 15 and 64 years of age as the denominator variable (but this data is not available for more than 40 countries for which data on total population is available).²⁰

To measure a country-year level of democracy I use the electoral democracy index of the Varieties of Democracy (V-dem) Project. This variable is the direct operationalization of the democracy concept described in section 2.2.1. To aid the interpretation of substantive effects I rescale this variable to range from 0 to 10 (the original variable ranges from 0 to 1). To ensure that my results are not affected by the choice for this particular democracy indicator I run robustness checks with the combined Polity IV index, the “lexical” electoral democracy index of Skaaning, Gerring and Bartusevičius (2015), and the dichotomous democracy measures of Boix, Miller and Rosato (2013) and Cheibub, Gandhi and Vreeland (2010). In addition, I use data from V-dem to generate two simple dummy variables that take the value 1 if all adults or all adult males have the right to vote, and 0 otherwise.

2.5 Empirical strategy

To examine the effect of industrialization on democracy I follow the existing literature by estimating the following equation with ordinary least squares (OLS):²¹

$$Y_{it} = \beta_0 + \beta_1 Y_{it-1} + \beta_2 X_{it-1} + \beta_3 C_{it-1} + \delta_t + \gamma_i + \epsilon_{it} \quad (2.1)$$

²⁰Note that all results holds when adding the dominator of the independent variable as a control variable.

²¹Given that the time dimension is relatively large in my panel (1845–2015) I expect the influence of the Nickell bias to be negligible. As shown in Figure 2.1 the results hold when using the Arellano and Bond (1991) “difference” GMM estimator.

where Y_{it} is the level of democracy in country i and year t . X_{it-1} is the percentage of the population engaged in manufacturing in country i 5 years prior to t .²² On the right-hand side I include one lag of democracy (i.e., Y_{it-1}) to account for serial autocorrelation and potential mean-reverting. After including one lag of democracy I find no economically or statistically significant effect of further lags of democracy and no evidence of serial autocorrelation in a Wooldridge autocorrelation test. Nonetheless all results hold with 2 to 5 lags of democracy on the right-hand side. δ in equation (2.1) are a full set of country fixed effects, γ are a full set of time fixed effects, and C_{it-1} is a vector of time-varying control variables. I report standard errors that are robust against heteroskedasticity and serial correlation on the country level.²³

I choose to focus on linear dynamic panel models with country and time fixed effects for three reasons. First, such models are easy to interpret. Second, this structure of fixed effects maximizes the amount of econometric identification that can be obtained by design, given the observational nature of the data. Last, this is the most widely used econometric specification used in the recent democratization literature (e.g., Acemoglu et al. (2008), Boix (2011), and Treisman (2015)).²⁴ Note, however, that imposing a linear functional form assumes that a decline in manufacturing employment always negatively affects democracy. In the context of my theory this is a very conservative approach as my theory suggests that the higher labor productivity in manufacturing tends to generate a structural change in employment from manufacturing to modern services in highly industrialized countries which, importantly, still sustains democracy. In Appendix 2.8.6 I find that taking this subtlety into account does indeed lead to slightly larger estimated effects. However, I judge these differences to be too small to

²²As shown in Figure 2.1 all results hold with longer and shorter time lags.

²³Following the existing literature I estimate equation (2.1) in levels. Evidence from unit root tests suggests that this is appropriate given that the data is stationary.

²⁴The results are, if anything, stronger when focusing on random effects models, which were widely used in the earlier democratization literature (e.g., Barro (1999), Epstein et al. (2006), and Przeworski and Limongi (1997)).

warrant a substantially more complex and unconventional econometric specification (the differences with the simpler linear model are generally not statistically significant and are typically around or under 10% of the total effect size).

In terms of causal identification equation (2.1) removes three important sources of potential bias. First, because of the country fixed effects equation (2.1) controls by design for all potentially confounding factors that do not vary over the time period that countries are observed. This, for example, excludes the possibility that the correlation that I find between industrialization and democracy is due to historical factors that occurred before countries enter the sample (e.g., colonialism). Second, because of the time fixed effects equation (2.1) controls by design for all potentially confounding factors that affect all countries at the same point in time. This, for example, excludes the possibility that the correlation that I find between industrialization and democracy is due to factors related to the nature of the international system. Last, equation (2.1) controls for reversed causality by lagging the industrialization variable with 5 years.²⁵ This guards against the possibility that the correlation that I find between industrialization and democracy is due to democracy inducing industrialization, rather than vice versa.

Given the lagged independent variable, country, and time fixed effects the only way how equation (2.1) can be biased is by factors that: (a) *change* over the time period that countries are observed; *and* (b) which affect *both* the level of democracy in t and the level of industrialization 5 years prior to t . The key identifying assumption underlying my approach is thus the absence of time-varying confounders.

As is typical for country-level studies on democratization I am unable to provide a design-based solution to the problem of time-varying confounders. To nonetheless minimize the probability of spurious correlation I conduct three robustness checks. First, I control for a large number of time-varying factors that may reasonably be

²⁵This does require the assumption that serial autocorrelation is sufficiently accounted for.

expected to affect both industrialization and democracy (see Appendix 2.8.2 for the theory behind these controls). Second, I use the method of Oster (2019) to estimate how large selection on unobservables has to be, relative to that of observed control variables, to assign the entire correlation between industrialization and democracy to confounding (rather than causality). Last, I run robustness checks where I estimate the effect of industrialization on democracy in relatively short time frames (e.g., post-1975) where potentially time-varying confounders are less likely to have substantially changed. Taken together I believe that this provides the best possible evidence that can currently be collected regarding the long-term country-level effect of industrialization on democracy.

2.6 Results

2.6.1 Level of industrialization at which today's highly developed countries democratized

Before turning to the econometric results I analyze whether the historical record in terms of the level and timing of democratization in the currently highly developed countries makes sense from an industrialization point of view. To do so I plot the level of industrialization²⁶, income, equality, education, and urbanization, in the year that all currently highly developed countries in the West and East Asia became stable democracies according to the ordinal electoral democracy index of V-dem.²⁷²⁸ For

²⁶For this analysis I am able to take the percentage of the workforce (rather than population) that works in manufacturing without losing much data.

²⁷With the exception of Germany I take the year before World War II in which countries democratized, even if democracy was interrupted during the 1933 to 1945 period.

²⁸In a small number of cases V-dem already codes countries as “full” democracies before they introduced universal adult suffrage. In such cases I take the first year that the country introduced universal adult suffrage *and* was coded by V-dem as a full democracy. Not imposing this additional condition (which I deem necessary to call a country fully democratic) makes no substantive difference to the results.

comparison I report in parentheses the percentage of countries that V-dem codes as non-democracies and which had an equal or higher level of industrialization, income, equality, education, and urbanization in 2015.

As can be seen in Table 2.8 most countries in North-West Europe and the British offshoots democratized in and around the 1920s. This appears very difficult to explain when one focuses on income, inequality, education, and urbanization because most of these countries still had levels of income, equality, education, and urbanization at the time that are low even by the standard of many of today's autocratic developing countries. For example, even the United Kingdom (in many ways the first "economically developed" country) had only a level of income of \$7,150, a V-dem equality score of 0.348²⁹, an average years of schooling of 5.2, and an urbanization rate of 64.0% when it democratized in 1928. More than 50.0%, 64.6%, 73.7%, and 29.3% of all autocracies in fact had an equal or higher level of income, equality, education, and urbanization in 2015.³⁰³¹

When one, however, focuses on industrialization the timing of democratization among early developers makes a lot of sense since the Second Industrial Revolution, which had completely transformed the socio-economic structure of Western societies, had just ended by the 1920s, right before most early developers democratized.³² And indeed in 1928, in the United Kingdom, approximately 34.7% of the working

²⁹I here use V-dem's equal distribution of resources index because Gini data is missing for many early developers at the time of democratization.

³⁰These disparities are even starker if one considers the United Kingdom already a "full" democracy in the 19th century.

³¹Appendix 2.8.7 shows that this also holds when one measures education levels through primary education (as in Murin and Wacziarg (2014)) and inequality through top 10% and top 1% income shares (using data from the World Inequality Database (WID)). Note that WID data on capital shares in GDP (as in Houle (2009)) is missing for all countries in Table 2.8 except for the United Kingdom, the United States, and France at the time of democratization. For the United Kingdom (1928), the United States (1965), and France (1947) the numbers are 25.7 (15.4%), 26.7 (15.4%), and 16.0 (0%), respectively. The result in France is heavily influenced by the capital destruction of WWII (see Piketty (2013)). In 1939 the capital share in France was 27.8%.

³²Of course I am not suggesting here that industrialization was, or ever is, the only cause of democracy. Another factor that clearly was important in this particular time period was the end of WWI.

Table 2.1 Level of income, equality, education, urbanization, and industrialization at the time of democratization in currently highly developed countries.

| Country | Year | GDP per capita (PPP) | Equality index (V-dem) | Average years of education | % of population living in cities | % of workforce in manufacturing |
|--|-------------------|----------------------|------------------------|----------------------------|----------------------------------|---------------------------------|
| Panel A: Major European countries | | | | | | |
| Austria | 1921 | \$2,871 (72.7%) | 0.685 (29.3%) | 5.1 (78.9%) | 22.4 (89.6%) | 33.2 [†] (0%) |
| Belgium | 1949 | \$6,585 (50.0%) | 0.816 (8.5%) | 6.7 (52.6%) | 51.0 (50.6%) | 37.7 (0%) |
| France | 1947 | \$5,413 (54.5%) | 0.894 (7.3%) | 4.3 (84.2%) | 44.4 (54.5%) | 22.9 (0%) |
| Italy | 1947 | \$2,915 (72.7%) | 0.556 (40.2%) | 4.2 (84.2%) | 37.8 (67.5%) | 22.8 (0%) |
| Netherlands | 1922 | \$5,266 (54.5%) | 0.627 (34.1%) | 4.2 (84.2%) | 43.0 (57.1%) | 25.4 (0%) |
| Portugal | 1976 | \$8,908 (42.4%) | 0.753 (15.9%) | 4.5 (84.2%) | 20.4 (93.5%) | 27.0 (0%) |
| Spain | 1978 | \$12,656 (31.8%) | 0.815 (8.5%) | 6.8 (52.6%) | 50.0 (53.2%) | 26.8 (0%) |
| Sweden | 1921 | \$4,166 (60.6%) | 0.631 (34.1%) | 4.9 (84.2%) | 17.2 (96.1%) | 30.2 [†] (0%) |
| Switzerland | 1971 | \$23,858 (12.1%) | 0.962 (0%) | 10.0 (15.8%) | 30.6 (80.5%) | 38.5 (0%) |
| West Germany | 1949 | \$4,674 (57.6%) | 0.929 (4.9%) | 7.0 (50.0%) | N/A | 38.0 (0%) |
| Panel B: UK and British offshoots | | | | | | |
| Australia | 1901 | \$5,756 (53.0%) | 0.716 (22.0%) | 3.1 (97.4%) | 36.3 (70.1%) | 25.8 [†] (0%) |
| Canada | 1921 | \$5,587 (54.5%) | 0.424 (52.4%) | 5.6 (65.8%) | 26.3 (88.3%) | 27.9 [†] (0%) |
| New Zealand | 1893 | \$5,237 (54.5%) | N/A | 4.3 (84.2%) | N/A | 28.0 [†] (0%) |
| United Kingdom | 1928 | \$7,150 (50.0%) | 0.348 (64.6%) | 5.2 (73.7%) | 64.0 (28.6%) | 34.7 (0%) |
| United States | 1920 [‡] | \$10,153 (37.9%) | 0.333 (67.1%) | 7.2 (44.7%) | 35.6 (71.4%) | 26.1 (0%) |
| United States | 1965 | \$21,390 (16.7%) | 0.582 (37.8%) | 10.4 (13.2%) | 55.9 (40.3%) | 27.9 (0%) |
| Panel C: East-Asian miracle economies | | | | | | |
| Japan | 1952 | \$3,086 (71.2%) | 0.931 (3.7%) | 7.1 (44.7%) | 38.6 (67.5%) | 20.9 (2.4%) |
| South Korea | 1988 | \$10,732 (36.4%) | 0.753 (15.9%) | 10.1 (15.7%) | 55.0 (42.9%) | 24.2 (0%) |
| Taiwan | 1997 | \$27,312 (12.1%) | 0.913 (7.3%) | 9.9 (18.4%) | 78.0 (13.0%) | 32.2 (0%) |

Notes: Entry is year that currently highly developed countries became stable democracies. This is operationalized as the first year in which countries are coded as fully democratic by the V-dem electoral democracy index, universal adult suffrage is effective, and both these variables remain unchanged in all years until 2015. Percentage of countries that are coded as nondemocracies by V-dem in 2015 with an equal or higher level of income, equality, education, urbanization, and industrialization in parentheses. See Appendix 2.8.3 for measurement of variables. If data is missing it is imputed from earlier or later years within a 5 year interval. N/A indicates that data is not available.

[†] Includes construction and mining together with manufacturing.

[‡] Includes construction together with manufacturing.

[‡] Refers to the year that universal suffrage for whites was introduced.

population was employed in manufacturing, which is about 12.4% higher than the most industrialized undemocratic country today (China).³³

Table 2.8 shows that the story for the late-developers in East Asia and Southern Europe is essentially the same. Spain and Portugal industrialized from the beginning of the 1960s until 1974 and democratized in 1975 and 1976 respectively. South Korea industrialized from 1963 to approximately the middle of the 1980s and held its first democratic election in 1988. Taiwan industrialized from the 1950s to the end of the 1980s and democratized in 1996. Furthermore, all late-developers democratized on levels of industrialization that are virtually unprecedented by any autocratic country today.³⁴ Again this story would be difficult to tell when one focuses on income, inequality, education, or urbanization. For example: when South Korea democratized in 1988 it still had an average per capita income of “only” \$11,571, which even at the time was below that of countries such as Uruguay, Mauritius, Trinidad and Tobago, Barbados, Argentina, Seychelles, and Gabon.

2.6.2 Industrialization as distinct from other economic determinants of democracy

Before turning to the main results I econometrically test the claim that industrialization is not simply a proxy for other more widely studied economic determinants of democracy. To do so I examine the “within” R-squared of two-way fixed effects regressions of the percentage of the population engaged in manufacturing on income, inequality,

³³The 2021 Chinese Statistical Yearbook suggests that approximately 22.3% of the Chinese workforce was employed in manufacturing in 2020. In the West, Taiwan, and South Korea at least 22.9%—and typically more than 25%—of the workforce was employed in manufacturing at the time of democratization (median=27.0%; mean=28.5%) (see Table 2.8). While it is certainly true that China harbors a sizable group of manufacturing workers, China is also the largest country in the world. With the exception of Japan (which arguably democratized “early” because of unrelated processes triggered by its defeat in World War II) China is actually somewhat *less* industrialized than all of today’s highly developed countries were when they democratized.

³⁴The exception is Japan which democratized in 1952 when it was still relatively little industrialized (arguably at least in part due to outside pressure after WWII).

education, and urbanization (in the same year), after purging the country and time fixed effects using Wooldridge's (1991) detrending method. This exercise suggests that changes in industrialization within countries over time only share 18.1%, 4.5%, 27.4%, and 11.1% of their variance with respective changes in income, equality, education, and urbanization *at the same point in time*. This pattern arises because industrialized countries have historically reached high shares of employment in manufacturing several decades *before* they have reached high levels of income, equality, education, and urbanization.³⁵

2.6.3 Fixed effects estimates of effect of industrialization on democracy

Table 2.2 reports the main results. Here I regress the V-dem electoral democracy index on the percentage of the total population engaged in manufacturing, together with country and time fixed effects.³⁶ To be able to compare the substantive size of effects I standardize all non-dichotomous variables using z-scores.

The coefficient in column (1) suggest that a standard deviation increase in the percentage of the population engaged in manufacturing (i.e., an increase of 4.1%) leads on average to a 0.468 scale points increase in V-dem's electoral democracy index (rescaled to range from 0 to 10) in 5 years. This effect is statistically significant at the 99% confidence level.

0.468 is only the short-run (immediate) effect, however. Because the model includes a lag of the dependent variable we have to back out the implied long-run effect (working

³⁵This result is established by visually analyzing trends over time in each country included in Table 2.8.

³⁶In my baseline sample I include country-years if they arguably have the sovereignty to decide upon their own regime type and data is available. This first condition excludes all ex-colonies during their respective periods of colonialization, countries that were occupied by Nazi Germany and Japan during WWII, Macao, Hong Kong after 1996, and the Soviet satellite states in Eastern Europe from 1945 to 1990. All results hold when including these cases. See Appendix 2.8.4 for all the country-years included in the baseline sample.

through the autoregressive process of democracy) by estimating:

$$\beta_2 X_{it-1} / (1 - \beta_1 Y_{it-1}) \quad (2.2)$$

where X is % in manufacturing, Y is democracy, and the β 's are regression coefficients (Wooldridge, 2010).

As is shown at the bottom of column (1) of Table 2.2 the long-run effect of a standard deviation increase in industrialization is estimated to be 0.907. This is an economically large, but not an implausibly large, effect. It suggests, for example, that if a country would progress from a largely agricultural society of say less than 1% of the total population engaged in the manufacturing sector (e.g., Afghanistan, Angola, Eritrea, Kenya, Kyrgyzstan, Yemen, Zambia in 2015) to a highly industrialized society of say 13% of the total population engaged in manufacturing (e.g., approximately the peak-level of industrialization reached by countries such as the France, Japan, and the United States, but lower than the peak-level reached by the United Kingdom) that its level of democracy would increase by approximately 24.1% of V'dem's full electoral democracy index. 24.1% is roughly equivalent to the difference in the level of democracy that existed between the United States and Turkey in 2015.

In column (2) I add the natural log of GDP per capita, V-dem's equality index, average years of education, and urbanization as covariates. As can be seen the effect of industrialization remains highly statistically and economically significant after accounting for these other more widely studied economic determinants of democracy. Furthermore, the size of the coefficients indicates that the effect of industrialization on democracy is significantly stronger than these alternative determinants.³⁷ This again

³⁷Note that the effect of inequality and urbanization, while smaller, is in the direction expected by most standard theories of democratization (except for Ansell and Samuels (2014)). Average years of education is not significantly different from zero even when only accounting for country and time fixed effects (a result first established by Acemoglu et al. (2005)). Importantly, however, years of primary education and literacy do remain economically and statistically significant even when accounting

highlights the importance of industrialization as a distinct economic explanation for democracy.³⁸

The effect of industrialization on democracy in columns (1) and (2) is causal to the extent that there exist no other factors that vary during the time that I observe each country and which affect both democracy at time t and the level of industrialization 5 years prior to t . To gauge how plausible this assumption is I begin by adding a large number of time-varying control variables which the existing literature highlights as potential co-determinants of industrialization and democracy (see Appendices 2.8.2 and 2.8.3 for the measurement and theoretical motivation behind these controls).

Reassuringly, the effect of industrialization on democracy remains highly statistically and economically significant, and changes very little in size, after controlling for: oil rents, the number of past transitions, economic recessions, the amount of years that the current regime is in place, private property rights security, international trade, whether a democracy functions under a constitution written by an outgoing autocratic elite, the degree of state ownership in the economy, political corruption, alignment

for industrialization and country and time fixed effects (results available on request). Interestingly, I find that GDP per capita has an economically and statistically significantly *negative* effect on democracy after accounting for industrialization. Given space limitations I do not intend to provide a fully-fledged explanation for this finding in this paper. One theoretically plausible explanation is, however, that when one regresses democracy on GDP per capita one is implicitly assuming that a 1% increase in income coming from agricultural production by big landowners, natural resource extraction, tax evasion, or tourism, is equally likely to lead to democracy as a 1% increase in income from manufacturing or modern services. Given what we know of how these different production activities have radically different effects on a country's socio-economic structure this assumption appears quite questionable. One way to interpret the fact that GDP per capita negatively affect democracy after conditioning on the percentage of the population engaged in manufacturing is that this removes more of the "positive" type of economic production activities from the equation leaving the variation in GDP per capita to be more heavily derived from production activities that are either unlikely to affect democracy (e.g., tourism, family farming, retail services), or are in fact likely to negatively affect the long-run prospects of democracy (e.g., natural resource extraction, agricultural production by large landowners, tax havens). Future research may fruitfully examine this possibility further.

³⁸An alternative explanation could be that income, education, and urbanization affect democracy through industrialization, and thus that including these variables together with industrialization in the same time period attenuates the effect of income, education, and urbanization. I believe this is unlikely as certainly income, and most probably also education and urbanization, are, if anything, more likely to be outcomes, rather than causes, of industrialization (see also section 2.6.2). In any case I find similarly economically and statistically insignificant effects of income, education, and urbanization on democracy when adding industrialization at $t - 2$.

to the West, and inter- and intra-state warfare (see columns (3) – (6)). Furthermore, interpreting the substantive size of the control variables suggest that the effect of industrialization on democracy is significantly larger than any of these alternative determinants of democracy.

The fact that the correlation between industrialization and democracy remains after including these controls suggest that the effect of industrialization on democracy is not driven by factors that we know from existing research may well be correlated with both industrialization and democracy. This of course does not proof that there are no other, perhaps currently unknown, factors that vary over time and confound the relationship between industrialization and democracy.

Excluding this possibility beyond any doubt would require a (natural) experiment, which would purge such time-varying confounders through (partial) random assignment of industrialization across countries. Unfortunately such a (natural) experiment is, to the best of my knowledge, not available.

I therefore take a second-best approach and ask: how large should selection on unobserved time-varying confounders be, relative to the observed control variables in Table 2.2, to entirely explain away the effect of industrialization on democracy? To estimate this parameter I make use of the recently developed method of Oster (2019).³⁹ In sum, Oster (2019) uses the observed correlation between the controls and industrialization to estimate how much stronger the correlation between industrialization and unobservables has to be to reduce the effect of industrialization on democracy to 0. Oster's (2019) δ for each set of controls can be found at the bottom of Table 2.2. As can be seen I find that to assign the entire correlation between industrialization and democracy to confounding (rather than causality) the effect of time-varying unobserved confounders has to be at least 1.2 times (i.e., 120%) stronger than the effect of country

³⁹This method is closely related to, and improves on, the seminal work of Altonji and Taber (2005).

Table 2.2 The effect of industrialization on democracy.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--|---------------------|----------------------|---------------------|---------------------|---------------------|---------------------|
| Democracy $t-1$ | 0.484*** (0.047) | 0.476*** (0.056) | 0.493*** (0.059) | 0.388*** (0.068) | 0.460*** (0.048) | 0.481*** (0.048) |
| % in manufacturing $t-1$ | 0.468*** (0.116) | 0.606*** (0.126) | 0.509*** (0.130) | 0.425** (0.137) | 0.481*** (0.116) | 0.471*** (0.122) |
| Equality index (V-dem) $t-1$ | | 0.299 (0.203) | | | | |
| Log GDP per capita $t-1$ | | -0.743*** (0.179) | | | | |
| Years of education $t-1$ | | -0.024 (0.282) | | | | |
| Urbanization rate $t-1$ | | 0.389* (0.187) | | | | |
| Oil rents (% of GDP) $t-1$ | | | 0.059 (0.100) | | | |
| No. of past transitions $t-1$ | | | 0.405*** (0.094) | | | |
| Economic crisis $t-1$ | | | -0.003 (0.088) | | | |
| Duration regime $t-1$ | | | | -0.318** (0.099) | | |
| Property rights security $t-1$ | | | | 0.451* (0.204) | | |
| Trade (% of GDP) $t-1$ | | | | -0.055 (0.048) | | |
| Authoritarian constitution $t-1$ | | | | | 0.018 (0.255) | |
| State control of economy $t-1$ | | | | | 0.221 (0.125) | |
| Political corruption $t-1$ | | | | | -0.266 (0.156) | |
| Aligned to West $t-1$ | | | | | | -0.086 (0.124) |
| Inter-state warfare $t-1$ | | | | | | -0.223 (0.155) |
| Intra-state warfare $t-1$ | | | | | | -0.040 (0.200) |
| Implied long-run effect of % in manufacturing $t-1$ | 0.907*** (0.229) | 1.157*** (0.256) | 1.003*** (0.252) | 0.695** (0.229) | 0.891*** (0.224) | 0.908*** (0.244) |
| Country fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Time fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Oster δ (R^2 max = 1) | — | 2.7 | 1.4 | 1.3 | 1.5 | 1.2 |
| Observations | 1294 | 948 | 1037 | 1080 | 1134 | 1294 |
| Countries | 145 | 95 | 126 | 128 | 135 | 145 |

Notes: OLS regressions. The dependent variable is the electoral democracy index of V-dem (rescaled to range from 0 to 10). See Appendix 2.8.3 for measurement of control variables. Data is observed in years ending with 0 and 5's (i.e., 5 year regular intervals). Robust standard errors clustered on the country level in parentheses.

*** p<0.001, ** p<0.01, * p<0.05.

and time fixed effects and the strongest *combination* of control variables included in Table 2.2.

While I cannot exclude the possibility that such a strong confounder exist, this does indicate that for this to be true the existing literature on democracy and industrialization must have missed an extremely important co-determinant of industrialization and democracy outside the already substantial list of controls included in Table 2.2.

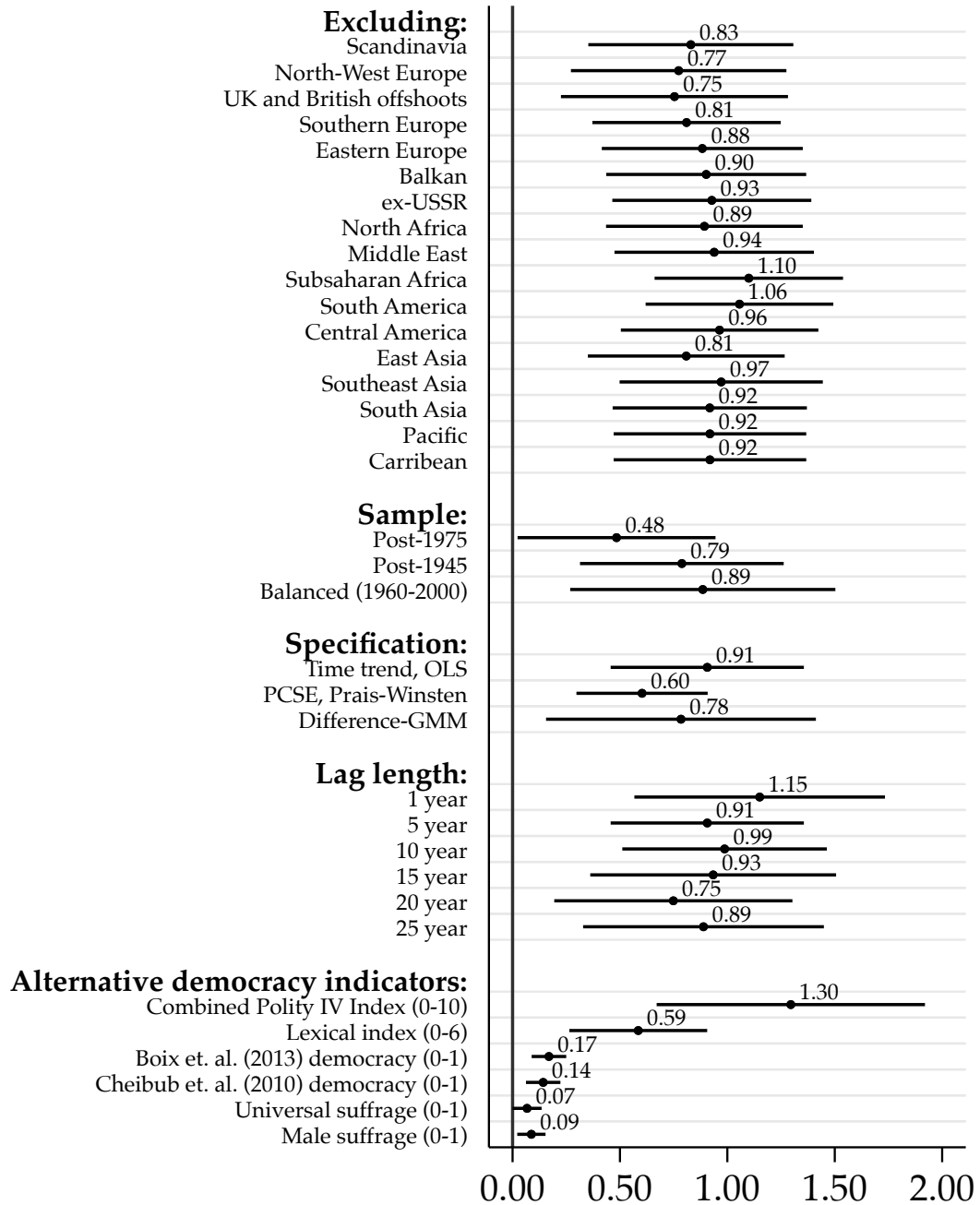
2.6.4 Robustness checks

In Figure 2.1 I find that the effect of industrialization on democracy is highly robust to (1) omitting any particular world region from the sample, (2) examining the effect only in particular time periods (e.g., third wave of democratization), (3) using a balanced sample from 1960 to 2000 (as in Acemoglu et al. (2008)), (4) using longer or shorter lag lengths, (5) using different democracy indicators (e.g., combined Polity IV index)⁴⁰, and (6) using alternative dynamic panel estimators (e.g., the “difference” GMM estimator).

In Appendix 2.8.8 I examine whether it is truly manufacturing employment that is driving the effect or whether the effect is rather the result of other factors that may be correlated with manufacturing employment. To do so I re-estimate equation (2.1) while controlling for the percentage employed in agriculture, mining, and construction, and when controlling for manufacturing output. I here find that it is indeed manufacturing employment that is driving the effect, and that these other factors, if anything, have a negative effect on democracy after accounting for manufacturing employment.

⁴⁰Note that, as we would expect, the effect of industrialization on each individual democracy indicator is relatively similar if each (continuous) democracy indicator is standardized using z-scores. More specifically, the long-run effect is estimated to be 0.298 (Std. error: 0.076) on the standardized V-dem electoral democracy index, 0.365 (Std. error: 0.090) on the standardized Polity IV index, and 0.252 (Std. error: 0.071) on the standardized Lexical index.

Fig. 2.1 Robustness checks.



Notes: Implied long-run effect of industrialization on democracy estimated using a dynamic panel model with country and time fixed effects. Circles denote regression coefficients standardized using z-scores. 95% confidence intervals are denoted by lines. Each row refers to a separate regression. Confidence intervals are robust against heteroscedasticity and autocorrelation on the country level.

2.6.5 Treatment heterogeneity

In Table 2.3 columns (1) and (2) I examine whether the effect of industrialization on democracy varies depending on whether one is focusing on transitions to or consolidations of democracy (as is suggested by Przeworski and Limongi (1997) and Houle (2009) with regard to the effect of income and inequality on democracy). To do so I regress the dichotomous version of the V-dem electoral democracy index on the level of industrialization in $t - 1$ interacted with the (dichotomous) level of democracy in $t - 1$ (together with country and time fixed effects). This approach thus examines whether the effect of industrialization on democracy depends on whether a country was a democracy or not when the change in the level of industrialization occurred. As a robustness check I run the same regression using Boix, Miller and Rosato's (2013) democracy indicator, which is especially designed to be dichotomous. As can be seen in columns (1) and (2) both interaction effects are highly statistically and economically *insignificant*, suggesting that industrialization has an equally large effect on both transitions to and consolidations of democracy.

In column (3) I examine to what extent the effect of industrialization on democracy is contingent on state dependency. Bellin (2000) and O'Donnell (1973), among others, have suggested that industrialization under conditions of late development may have very different, and decisively less benevolent, effects on democracy. In sum, the argument is that late developers sought to industrialize under conditions of much greater market integration and competition of vastly more superior producers abroad. Manufacturing workers and industrialists were therefore heavily dependent on the state for protection to survive. This dependency on the state in turn made it less likely that industrialization generated an autonomous social group with the interest and capacity to push for democracy (because nobody wants to bite the hand that feeds him).

Cross-country measures of the degree to which country' manufacturing sectors are state dependent are, to the best of my knowledge, not available. I therefore proxy this variable using V-dem expert survey data on the degree to which the state owns or otherwise controls productive capital in the economy (this variable can unfortunately not be disaggregated to the manufacturing sector in particular).⁴¹ As can be seen in column (3) at least this measure of (general) state dependency does not significantly moderate the effect of industrialization.

While better measures of a manufacturing sector' state dependency are desired one plausible explanation for this finding is that state dependency is in itself affected by successful industrialization. While import substitution industrialization in Latin America and command industrialization in the Soviet Union was heavily state dependent, so was the process of industrialization in many of the countries that did become well-functioning democracies shortly after industrialization. The governments of South Korea, Taiwan, and Japan all made widespread use of selective subsidies and tariffs protections to develop their manufacturing sector. Furthermore, many of the key industries that drove industrialization in these countries were entirely or in large part state-owned. While often underlighted, many manufacturing industries in the early developers, including in Britain and the United States, also heavily relied on selective tariff protection, state-sponsored industrial espionage, and large government subsidies (Chang, 2002). Crucially, however, in the truly successful industrializers this state dependence lessened over time precisely *because* successful industrialization made the manufacturing sector competitive enough to flourish without the help of state support. In line with this interpretation my data suggests that none of the cases that Bellin (2000) and O'Donnell (1973) study to substantiate their hypothesis in fact ever reached

⁴¹Note that employment in state-owned enterprises is regularly used to measure "state dependency" in individual-level studies on support for democratization (e.g., Rosenfeld (2021)).

Table 2.3 Heterogeneity across democratic transitions/consolidations and state dependency.

| | (1) | (2) | (3) |
|--|---------------------|---------------------|---------------------|
| % in manufacturing $t-1$ | 0.071* (0.029) | 0.110*** (0.033) | 0.492*** (0.119) |
| V-dem democracy (dichotomous) $t-1$ | 0.457*** (0.039) | | |
| V-dem democracy (dichotomous) $t-1$ * | 0.016 (0.032) | | |
| Boix et. al. (2013) democracy $t-1$ | | 0.442*** (0.046) | |
| Boix et. al. (2013) democracy $t-1$ * | | -0.025 (0.041) | |
| % in manufacturing $t-1$ | | | 0.474*** (0.049) |
| V-dem democracy (continuous) $t-1$ | | | 0.147 (0.107) |
| State ownership $t-1$ | | | -0.091 (0.075) |
| State ownership $t-1$ * | | | |
| % in manufacturing $t-1$ | | | |
| Implied long-run effect of % in manufacturing $t-1$ | 0.131* (0.053) | 0.196*** (0.057) | 0.936*** (0.229) |
| Country fixed effects | Yes | Yes | Yes |
| Time fixed effects | Yes | Yes | Yes |
| Observations | 1294 | 1251 | 1294 |
| Countries | 145 | 143 | 145 |

Notes: OLS regressions. The dependent variable in column (1) takes the value 1 if V-dem's electoral democracy index exceeds 0.8, and 0 otherwise. The dependent variable in column (2) is the dichotomous democracy variable of Boix, Miller and Rosato (2013). The dependent variable in column (3) is the (continuous) electoral democracy index of V-dem (rescaled to range from 0 to 10). Data is observed in years ending with 0 and 5's (i.e., 5 year regular intervals). Robust standard errors clustered on the country level in parentheses.

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

close to the level of industrialization that existed in the currently highly developed countries in the West and East Asia before they democratized.⁴²

⁴²The same argument is likely to apply to the USSR. While reliable data on manufacturing employment in the USSR is largely missing, the most credible data provided by Kouwenhoven (1997) suggests that approximately 21.2% of the Soviet workforce worked in manufacturing in 1987. As is shown in Table 2.8 this is still well below the level of industrialization that existed in all currently highly developed countries before they democratized (with the exception of Japan). In Germany 30.8% of the working population was employed in manufacturing in 1933. This case thus *does* contradict the theory. Germany before WWII may be a true outlier. Alternatively, it could be the case that industrialization did in fact push Germany into a pro-democratic direction, but that this effect was temporarily overwhelmed by the major anti-democratic effect of Germany's defeat in WWI (as convincingly argued by Berman (2001)).

2.6.6 Causal mechanism

A high share of employment in manufacturing can in principle positively affect democracy through many different complementary causal mechanisms. My theory emphasizes how industrialization tends to politically mobilize manufacturing workers in favor of democracy. If this is true one would expect to find: (a) that industrialization strongly increases the probability of mass revolts dominated by industrial workers; (b) that mass revolts dominated by industrial workers tend to induce democracy; and (c) that including mass revolts dominated by industrial workers on the right-hand side of a regression equation accounts for a significant part of the effect of industrialization on democracy.

I now test these expectations empirically.

To measure mass revolts by industrial workers I use Dahlum, Knutsen and Wig's (2019) data on the socio-economic make-up of mass political protests in 147 countries from 1900 to 2006. Because Dahlum, Knutsen and Wig (2019) only code very large political protests their data is relatively sparse (only 82 mass revolts dominated by industrial workers are recorded). I therefore collapse the data in 3 year simple moving averages and code a dummy that takes the value 1 if any mass political protest dominated by industrial workers has occurred in any of the past three years, and 0 otherwise.

In Table 2.4 column (1) I regress this industrial workers revolt dummy on the average level of industrialization during the same 3 year period (this is appropriate as I expect industrialization to directly lead to greater mobilization, rather than with a time lag). In line with my theory I find that industrialization strongly increases the probability of a mass political protest dominated by industrial workers. Substantially, a standard deviation increase in the fraction of the population engaged in manufacturing increases the probability of industrial worker revolts by 2.5%. This is a very large

effect given that the baseline probability of at least 1 mass political protest dominated by industrial workers is only 0.9%.

In column (2) I regress the V-dem electoral democracy index on industrial worker revolts. In line with the results of Dahlum, Knutsen and Wig (2019) I find that industrial worker revolts are very strongly correlated with democracy—at least one industrial worker revolt in the past 3 years increases the V-dem electoral democracy index with 1.6 points or approximately 0.6 standard deviations.

Running a formal mediation analysis is arguably of limited utility in this case because industrialization is likely to affect democracy through many other industrial worker revolts outside the major ones that are recorded by Dahlum, Knutsen and Wig (2019). The results in Table 2.4 nonetheless suggest that even the 82 mass industrial worker revolts coded by Dahlum, Knutsen and Wig (2019) account for approximately 15% of the direct effect of industrialization on democracy (see decline in industrialization coefficient between columns (3) and (4)). Assuming sequential ignorability (Imai et al., 2011) this suggests that mass revolts by industrial workers mediate at least some of the effect of industrialization on democracy.

Table 2.4 Industrialization, mass revolts by industrial workers, and democracy.

| Dependent variable: | Industrial worker revolt (0-1) $t-1$ | Electoral democracy index (V-dem) | | |
|--------------------------------|---|--------------------------------------|---------------------|---------------------|
| | (1) | (2) | (3) | (4) |
| Democracy $t-1$ | -0.013** (0.004) | 0.780*** (0.024) | 0.755*** (0.024) | 0.775*** (0.025) |
| Industrial worker revolt $t-2$ | 0.215* (0.088) | | | |
| Industrial worker revolt $t-1$ | | 1.630*** (0.300) | | 1.620*** (0.331) |
| % in manufacturing $t-1$ | 0.025* (0.010) | | 0.265*** (0.078) | 0.226** (0.077) |
| Country fixed effects | Yes | Yes | Yes | Yes |
| Time fixed effects | Yes | Yes | Yes | Yes |
| Observations | 1831 | 1936 | 1827 | 1827 |
| Countries | 136 | 136 | 135 | 135 |

Notes: OLS regressions. Dependent variable in column heading. The electoral democracy index of V-dem is rescaled to range from 0 to 10. Data is in 3 year simple moving averages. Robust standard errors clustered on the country level in parentheses.

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

2.7 Conclusion

Understanding why some countries are well-functioning democracies while other countries are autocracies is a major concern in comparative politics, and is of paramount importance for understanding the root causes of the major differences in human welfare across countries and over time. This paper contributes to this endeavor by showing that industrialization—defined as a large share of employment in manufacturing—is a highly robust determinant of democracy, net of other more widely studied economic determinants of democracy (e.g., income and inequality).

My empirical aim in this paper was to test whether industrialization is robustly correlated with democracy across a large number of countries and over a long period of time. Future research may fruitfully contribute by discovering ways to causally identify the effect of industrialization on democracy (if this is at all possible, given that industrialization cannot be randomly assigned by researchers themselves). Future

research is also necessary with regard to the important question of whether the average effect of industrialization on democracy that I recover varies across specific types of (de)industrialization (e.g., state versus market-led industrialization, export-oriented versus import-substitution industrialization, premature versus mature deindustrialization) and/or across different forms of manufacturing (e.g., high- versus low-tech, heavy- versus light industry).

The results have several important policy implications. This is particularly the case in light of the ongoing public policy debate on whether industrialization is still an important goal in itself, or whether poor countries may instead be able to reach Western standards of living by moving directly into a service economy (Ghani and O’Connell, 2014). Besides the economic question whether it is in fact true that countries can in the long-run reach high levels of development without industrializing my results suggest that a “post-industrial” development strategy may have large unintended political consequences because industrialization has historically been an important path towards well-functioning democratic institutions.

Related to this policy discussion is the important debate about the causes of and potential remedies against “premature deindustrialization” — i.e., the tendency of currently developing countries to start deindustrializing on much lower levels of income as compared to the experience of early industrializers (Palma, 2005; Rodrik, 2016). If it is true that industrialization tends to lead to more democratic forms of government it becomes even more crucial to understand the degree to which premature deindustrialization is driven by domestic policy choices (e.g., absence of targeted industrial policies), the rise of China as manufacturing exporter of the world, and/or automation (Haraguchi, Cheng and Smeets, 2017; Rodrik, 2016), as well as to invent policies that can help developing countries to increase employment in manufacturing.

2.8 Appendix

2.8.1 Social tables of the United Kingdom in 1928

In their seminal book Ansell and Samuels (2014) argue that the organized working class, like the urban middle class, is typically located above the economic median voter position at the time of democratization. They argue that an economic pivotal voter logic can therefore not explain why this group would have an incentive to push for free and fair elections with universal suffrage. They primarily come to this conclusion based on social tables from the United Kingdom in the year 1867.⁴³

I do not dispute Ansell and Samuels' (2014) findings with regard to Britain's social structure in 1867. Importantly, however, the Reform Act of 1867 enfranchised only about 15% of the adult population. Here I show that in 1928, when the United Kingdom introduced universal suffrage and arguably became a "full" democracy, the economic median voter was almost certainly a relatively high-skilled and plausibly well-organized manufacturing worker.

The British Census of 1921, which is the closest Census to the year 1928, suggests that the British income distribution at the time was as follows:⁴⁴

1. **Higher professionals:** 1.46% of the workforce, earning on average £582.
2. **Employers, proprietors, managers, and administrators:** 10.52% of the workforce, earning on average £480.
3. **Lower professionals:** 3.61% of the workforce, earning on average £320.

⁴³Ansell and Samuels (2014) also analyze social tables from Germany (1882), Russia (1904), Peru (1876), Chile (1861), and Java (1924). Russia, Peru, Chile, and Java were, however, hardly industrialized at these times, so we would not expect manufacturing workers to be the median voter in these cases. German industrialization only really took off after the 1871 unification but even then Ansell and Samuels's (2014) own data suggests that shy of 2.7% the median voter in Germany (1882) was a textile manufacturing worker. So this case is arguably broadly in line with my hypothesis that the median voter in highly industrialized countries tends to be a manufacturing worker. As granted in footnote 29 of the main text pre-WWII Germany is a rare outlier with regard to the effect of industrialization on democracy, but this is an unrelated issue.

⁴⁴The wages refer to males only. Employment shares include both males and females.

4. **Foremen, inspectors, and supervisors:** 2.61% of the workforce, earning on average £268.
 5. **Clerical workers:** 9.52% of the workforce, earning on average £182.
 6. **Skilled manual workers:** 25.11% of the workforce, earning on average £180.
 7. **Unskilled manual workers:** 10.46% of the workforce, earning on average £128
 8. **Semi-skilled manual workers:** 36.72% of the workforce, earning on average £126.
- (Routh, 1980, pp. 40, 120).

The median economic voter in 1921 Britain was thus a skilled manual worker. The categories for counting employees and wages within the group of skilled manual workers unfortunately do not overlap in the 1921 census, which makes it impossible to establish with certainty which group of skilled manual workers exactly held the economic median voter position at this time. We nonetheless do know that 69.1% of skilled manual workers were manufacturing workers, and that within the group of skilled manual workers approximately 11.0% and 4.4% were coalface workers and railway employees which tended to earn more on average than manufacturing workers (Routh, 1980, pp. 29, 101).⁴⁵ Taken together, this makes it very likely that manufacturing workers occupied the median economic voter position in Britain during the 1920s.

⁴⁵Coalface workers earned more because of a risk premium. Railway employees earned more because driving trains was generally more high-skilled labor.

2.8.2 Theoretical motivation behind control variables

In the paper I control for a large number of time-varying variables that could potentially affect both industrialization and democracy, and thus confound my two-way fixed effects estimates. Here I discuss the theoretical motivation behind the controls that I include.

Oil rents (% of GDP): Oil rents are a strong determinant of democracy (Tsui, 2011; Ross, 2001, 2012; Andersen and Ross, 2014; Ramsay, 2011; Ahmadov, 2014).⁴⁶ This is most probably because the production of oil allows states to receive income without relying on taxation (which may reduce demands for accountability), because oil revenues may provide autocratic governments with additional funds for repression, and/or because oil rents may reduce the incentive to invest in education and occupational specialization (Ross, 2001, 2012). Oil could also lower the incentive to industrialize (because oil extraction generates “easy” money) and/or the capacity to industrialize (because the price of manufacturing exports increases as a result of the “Dutch Disease”) (van Wijnbergen, 1984*a,b*)

No. of past transitions: The number of past (unsustained) transitions to democracy is one of the few variables that passes the extreme bounds test of Gassebner, Lamla and Vreeland (2013). It is unclear whether this is because past experience with democracy causes democracy, or whether this variable is capturing the effect of other factors that caused transitions in the past, have lasted, and cause democracy in the present as well. The result of Gassebner, Lamla and Vreeland (2013) does suggest however that the number of past transitions is a very robust correlate of democracy. The number of past transitions may also affect industrialization, for example, because more frequent shifts in regime type may negatively affect investment confidence (Alesina et al., 1996; Alesina and Perotti, 1996).

⁴⁶Nonetheless the “political resource curse” is certainly not uncontroversial (e.g., Haber and Menaldo (2011)).

Economic crisis: Economic recessions is also one of the few variables that passes Gassebner, Lamla and Vreeland's (2013) extreme bounds test—economic crises are a strong predictor of dictatorships breaking down.⁴⁷ Although the causal mechanisms behind this correlation are somewhat elusive this may be because the legitimacy of dictatorships tends to rest particularly heavily on economic performance (as the regime type itself has little inherent legitimacy) (Guriev and Treisman, 2019; Magaloni, 2006), and/or because economic recessions create a window of opportunity to mobilize based on economic grievances (Acemoglu and Robinson, 2001; Haggard and Kaufman, 1995). Economic crises also negatively affect the manufacturing sector particularly strongly. This is because unlike in many other sectors producers can postpone selling and consumers can postpone buying so that manufacturing output contracts particularly strongly during a recession, and because manufacturing is particularly capital-intensive and capital investments tend to rebound with a significant time lag after a recession (Eggers and Ioannides, 2006).

Duration regime: The probability of a regime change (in either democratic or authoritarian direction) declines strongly with the time that the current regime type is in place (Svolik, 2008). This is likely because political-economic actors make costly investments surrounding a particular regime type (Acemoglu and Robinson, 2006) and/or because once a regime type has been in place for a long time it may give the appearance that it is difficult to change (Little, 2017). Regime duration may also affect industrialization as political stability is a strong predictor of capital investment (Alesina et al., 1996; Alesina and Perotti, 1996).

Property rights security: New Institutional Economics suggests that private property rights security tends to cause economic development (Acemoglu, Johnson

⁴⁷There is also evidence that suggests that economic recessions tend to make it more likely that democracies reverse back to autocracy (Svolik, 2008).

and Robinson, 2001, 2005; North, 1990; Rodrik, Subramanian and Trebbi, 2004).⁴⁸ Private property right security could plausibly affect economic development through two key mechanisms. First, ill-defined and poorly protected private property rights might lower investment because of the higher uncertainty for potential investors as to whether they will be able to reap the benefits of their investment in the future. Second, insecure private property rights might divert resources from productive purposes toward private protection which is likely to be inefficient due to a decrease in the division of labor and due to economies of scale in securing property (Besley and Ghatak, 2009). The effect of private property rights security on democracy is more speculative, and New Institutional Economics in fact generally assumes that democracy causes private property rights security, rather than vice versa (e.g., Acemoglu, Johnson and Robinson (2005)). It could nonetheless be the case that private property rights security (also) affects democracy by reducing civil society' economic dependence on the state and/or by generating a more equal distribution of power in society by enabling the accumulation of wealth among groups that would otherwise be too weak to enforce their own property rights (Hayek, 1944).

Trade (% of GDP): López-Córdova and Meissner (2008) find that greater international trade tends to induce democracy.⁴⁹ They argue that this effect occurs because trade openness tends to strengthen the economic fortunes of the middle class, which in turn tends to push for democracy. International trade openness has also figured prominently on debates surrounding industrialization. Standard economic theory suggests that openness to international trade tends to foster industrialization. Proponents of the infant industry protection argument, however, suggest that while openness to exports are essential, developing countries may be better off by temporarily protecting their domestic manufacturing industries from imports coming from more technologically-

⁴⁸Nonetheless this theory is certainly not uncontroversial (e.g., Chang (2002, 2011), and Khan (2012)).

⁴⁹In line with this argument Aidt and Gassebner (2010) find that autocratic states trade less.

advanced economies (see Lin and Chang (2009) for an excellent discussion of these opposing views).

Authoritarian constitution: Albertus and Menaldo (2018) have shown that autocratic elites often manage to remain disproportionately powerful after a transition to democracy. One of the primary channels through which autocratic elites manage to do so is by influencing, or entirely writing, the constitution of a new democracy. If the elites of the previous autocratic regime manage to influence a democracy's constitution they typically embed anti-majoritarian institutions that structurally benefit their political-economic interests vis-à-vis the rest of society. Such "authoritarian" constitutions also typically contain rules that make it very difficult to lawfully change the constitution going forward. This in turn has large negative effects on the quality of democracy. Authoritarian constitutions are also likely to negatively affect industrialization. This is because successful industrialization typically requires inclusive economic institutions that reward anyone that manages to push the economy forward, while autocratic elites tend to prefer extractive institutions that redistribute wealth from the rest of the society to them, regardless of their own actual economic contribution (Acemoglu and Robinson, 2012).

State control of economy: Research by Bellin (2000) and Rosenfeld (2017, 2021), among others, suggests that democratization becomes less likely if the state directly controls large parts of the economy. This is because such control allows autocratic elites to use the employment dependency on the state of large parts of the population to stifle democratic opposition. Standard economic theory meanwhile suggests that allocating large parts of the economy through the government, rather than the market, is generally an inefficient way to industrialize (Mankiw and Taylor, 2011).⁵⁰

⁵⁰The relationship between state ownership and industrialization is nonetheless hardly obvious empirically. Indeed, most countries have industrialized with large parts of the manufacturing sector directly owned or otherwise strongly directed by the state. This is particularly true among the

Political corruption: While democracy is often seen as a potential cause of corruption (e.g., Fukuyama (2014), Rock (2009)), the relationship may also partly work the other way around. Politicians may, for example, reduce the pressure for democratization by using the public purse to selectively enrich their own supporters (De Mesquita et al., 2003). Corruption also appears to negatively affect economic development (Aidt, 2009; Mauro, 1995), although it could also be the case that corruption tends to decline because of economic development, rather than vice versa (Chang, 2002; Khan, 2012).

Aligned to West: Levitsky and Way (2005, 2010) have shown that social, economic, and technocratic ties to the West have had a large positive impact on democratization, particularly in the period after the Cold War. This is so, they argue, because such ties increase the cost of cracking down on democratic opposition for autocratic regimes. Ties with the West have also, at least in some cases, been important for industrialization. Amsden (1989) and Wade (1990), for example, argue that the large scale capital and technology transfers from the United States to South Korea and Taiwan, during the Cold War, played an important role in South Korea's and Taiwan's industrialization. In the post-Cold War period one could think of similar cases. The industrialization of many East-European countries was, for example, arguably helped by market access to and capital/technology transfers from more economically advanced West-European economies after the 2004 Eastern enlargement of the European Union.

Intra-state warfare: Civil wars are by definition violent breakdowns of political order and are therefore typically highly detrimental to democracy—which in the end is a political system designed to solve communal distributional issues through free and fair elections and an independent judiciary, as opposed to through violence. Wars

late-developers in East Asia (Amsden, 1989; Wade, 1990), but is also true in many of the early Western industrializers (Chang, 2002).

of all kind are also naturally very economically disruptive, hence typically negatively affecting industrialization prospects.

Inter-state warfare: International wars can clearly be fought while well-functioning democratic institutions remain in place domestically. In practice, however, international wars tend to come together with significant increases in state executive power, if not rule by decree, which does often tend to negatively affect democracy. Like civil wars inter-state warfare typically negative affects industrialization prospects. This is particularly the case if the war leads to the destruction of large amounts of (human and physical) capital and/or the interruption of important production chains. Inter-state warfare can, however, also lead to increases in industrialization. This is particularly the case when the war is mostly fought abroad and domestic industry benefits of increased demand for manufactured goods, such as weaponry (e.g., the United States during both WWI and WWII) (Block, 1977).

2.8.3 Measurement of control variables

Below I discuss the measurement and data sources used for each of the control variables included in the main text.

V-dem equality index: The equal distribution of resources index of the Varieties of Democracy (V-dem) project. Higher values mean more equality. The index is based on expert survey data regarding the equality of access to health care, education, and other public goods. See Coppedge et al. (2021, p. 56) for more information.

GDP per capita: Natural log of income per capita (PPP based). Data comes from Bolt et al. (2018).

Years of education: Average years of formal education per capita. Data comes from Lee and Lee (2016).

Urbanization rate: The percentage of the population living in urban areas (as defined by the country itself). Data comes from Vanhanen (2003), Coppedge et al. (2021), and the World Bank Development Indicators.

Oil rents (% of GDP): Petroleum production relative to total GDP. Data for both the value of petroleum production and total GDP comes from Haber and Menaldo (2011).

No. of past transitions: Number of past unsustained democratic transitions. Data comes from Boix, Miller and Rosato (2013). Democracy is defined minimally as (1) political leaders are chosen through free and fair elections, and (2) at least 50% of the male population has the right to vote.

Economic crisis: Dummy that takes the value 1 if the GDP per capita (PPP based) growth rate relative to the previous year is negative, and 0 otherwise. Data comes from Bolt et al. (2018).

Duration regime: Number of consecutive years that the current regime type is in place. Regime type is defined minimally as democracy, or not. Democracy is defined

as (1) political leaders are chosen through free and fair elections, and (2) at least 50% of the male population has the right to vote. Data comes from Boix, Miller and Rosato (2013).

Property rights security: Private property rights security index of V-dem. Variable is measured via expert surveys. Higher values mean more secure private property rights. Private property rights are defined as the right to acquire, possess, inherit, and sell private property, including land. Limits on private property rights may come from the state which may legally limit rights or fail to enforce them; customary laws and practices; or religious or social norms. This variable concerns the right to private property, not actual ownership of property. See Coppedge et al. (2021, p. 300) for more information.

Trade (% of GDP): Imports plus exports as a percentage of total GDP. Import and export data comes from Barbieri and Keshk (2016). GDP data comes from Bolt et al. (2018).

Authoritarian constitution: Dummy that takes the value 1 if a democracy has a constitution that was written by an outgoing autocratic elite, and 0 otherwise. Data comes from Albertus and Menaldo (2018).

State control of economy: The state ownership of the economy variable of V-dem. Expert survey question that gauges the degree to which the state owns and controls capital (including land) in the industrial, agricultural, and service sectors. Higher values mean less state control. See Coppedge et al. (2021, p. 161–162) for more information.

Political corruption: The political corruption index of V-dem. Expert survey measure of the pervasiveness of corruption in the executive, legislative, and judicial branches of government. See Coppedge et al. (2021, p. 227) for more information.

Aligned to West: Dummy that takes the value 1 if a country-year is aligned to the West, and 0 otherwise. This variable always takes the value 0 before 1945. From 1945 to 1990 Western alignment is defined as official alignment with the Western Bloc. This includes: Canada, United States, Suriname, French Guiana, Iceland, Norway, United Kingdom, Denmark, Netherlands, West Germany, Belgium, Luxembourg, France, Portugal, Spain, Italy, Greece, Turkey, Western Sahara, Angola, Namibia, South Africa, Zimbabwe, Mozambique, Iran, Oman, United Arab Emirates, Thailand, Philippines, South Korea, Taiwan, Japan, Papua New Guinea, Australia, and New Zealand.⁵¹ After 1990 Western alignment is defined as NATO membership, being a member state or candidate member state of the European Union, being a member state of the Schengen Agreement, being a member state of the European Economic Area, or being South Korea, Taiwan, or Japan.

Intra-state warfare: Dummy that takes the value 1 if a country was engaged in an intra-state war during any time within the past year, and 0 otherwise. Data comes from Clio Infra (clio-infra.eu), drawing on Brecke (2001).

Inter-state warfare: Dummy that takes the value 1 if a country was engaged in an inter-state war during any time within the past year, and 0 otherwise. Data comes from Clio Infra (clio-infra.eu), drawing on Brecke (2001).

⁵¹The results remain the same when also coding Pakistan as aligned (which was never officially the case).

2.8.4 Country-years included in sample

Table 2.5 Country-years included in baseline sample (N=145).

| Country | Years | Country | Years | Country | Years |
|------------------------------|-----------|-------------|-----------|--------------------------|-----------|
| Afghanistan | 1973–2000 | Greece | 1951–2008 | Papua New Guinea | 1975–2001 |
| Albania | 1988–2015 | Guatemala | 1950–2006 | Paraguay | 1950–2008 |
| Algeria | 1962–2004 | Guyana | 1966–2002 | Peru | 1940–2008 |
| Angola | 1975–2014 | Haiti | 1950–1991 | Philippines | 1948–2008 |
| Argentina | 1950–2011 | Honduras | 1950–2007 | Poland | 1918–2008 |
| Armenia | 1991–2014 | Hungary | 1869–2008 | Portugal | 1940–2008 |
| Australia | 1931–2008 | India | 1950–1991 | Qatar | 1986–2014 |
| Austria | 1919–2008 | Indonesia | 1961–2012 | Romania | 1930–2008 |
| Azerbaijan | 1991–2014 | Iran | 1963–2015 | Russia | 1897–2008 |
| Bahrain | 1992–2005 | Iraq | 1957–2002 | Rwanda | 1978–1989 |
| Bangladesh | 1972–2003 | Ireland | 1936–2008 | Saudi Arabia | 1976–2014 |
| Belarus | 2005–2015 | Israel | 1963–2015 | Senegal | 1974–2014 |
| Belgium | 1846–2008 | Italy | 1881–2008 | Sierra Leone | 1963–2004 |
| Benin | 1975–1981 | Ivory Coast | 1966–1997 | Singapore | 1965–2015 |
| Bolivia | 1950–2010 | Jamaica | 1959–2008 | Slovakia | 1993–2015 |
| Botswana | 1966–2010 | Japan | 1872–2008 | Slovenia | 1991–2014 |
| Brazil | 1950–2011 | Jordan | 1963–2015 | Somalia | 1967–1986 |
| Bulgaria | 1963–2015 | Kazakhstan | 1998–2014 | South Africa | 1946–2008 |
| Burkina Faso | 1974–1998 | Kenya | 1963–2015 | South Korea | 1963–2015 |
| Burundi | 1969–2013 | Kuwait | 1967–2014 | Spain | 1940–2008 |
| Cambodia | 1988–2000 | Kyrgyzstan | 1991–2014 | Sri Lanka | 1953–2008 |
| Cameroon | 1970–2008 | Laos | 1975–1999 | Sudan | 1972–2001 |
| Canada | 1891–2008 | Latvia | 1991–2015 | Suriname | 1975–2004 |
| Central African Republic | 1973–1993 | Lesotho | 1982–2009 | Swaziland | 1968–2011 |
| Chile | 1940–2008 | Liberia | 1962–1984 | Sweden | 1945–2008 |
| China | 1952–2011 | Libya | 1964–1996 | Switzerland | 1941–1980 |
| Colombia | 1938–2004 | Lithuania | 1992–2015 | Syria | 1963–2010 |
| Costa Rica | 1950–2011 | Luxembourg | 1985–2014 | Taiwan | 1949–2000 |
| Croatia | 1991–2015 | Macedonia | 1991–2011 | Tajikistan | 1991–2013 |
| Cuba | 1943–2008 | Madagascar | 1967–2006 | Tanzania | 1961–2011 |
| Cyprus | 1960–2008 | Malawi | 1964–2012 | Thailand | 1937–2008 |
| Czech Republic | 1987–2015 | Malaysia | 1968–2015 | The Gambia | 1975–2004 |
| Democratic Republic of Congo | 1968–1988 | Mali | 1976–2004 | Trinidad and Tobago | 1962–2008 |
| Denmark | 1945–2008 | Mauritius | 1968–2008 | Tunisia | 1963–2014 |
| Dominican Republic | 1950–2007 | Mexico | 1930–2008 | Turkey | 1935–2008 |
| Ecuador | 1950–2006 | Moldova | 1991–2014 | Uganda | 1963–2000 |
| Egypt | 1937–2008 | Mongolia | 1990–2014 | Ukraine | 1992–2015 |
| El Salvador | 1950–2007 | Morocco | 1960–2012 | United Arab Emirates | 1971–2010 |
| Eritrea | 1993–2011 | Mozambique | 1975–2000 | United Kingdom | 1841–2008 |
| Estonia | 1993–2015 | Myanmar | 1983–1997 | United States of America | 1870–2008 |
| Ethiopia | 1961–1993 | Nepal | 1961–1999 | Uruguay | 1968–2012 |
| Fiji | 1970–2013 | Netherlands | 1849–2008 | Venezuela | 1941–2008 |
| Finland | 1917–2008 | New Zealand | 1906–2008 | Yemen | 1988–2013 |
| France | 1856–2008 | Nicaragua | 1940–2006 | Zambia | 1965–2010 |
| Gabon | 1963–1995 | Niger | 1960–1977 | Zimbabwe | 1970–1996 |
| Georgia | 1998–2015 | Nigeria | 1960–2011 | | |
| Germany (Post-1990) | 1991–2015 | Norway | 1930–2008 | | |
| Germany (Pre-1945) | 1882–1939 | Oman | 1993–2015 | | |
| West Germany (1945–1990) | 1950–1990 | Pakistan | 1972–2008 | | |
| Ghana | 1960–2011 | Panama | 1940–2008 | | |

2.8.5 Descriptive statistics

Table 2.6 Summary statistics.

| Variable | N | Mean | Overall Std. Dev. | Within Std. Dev. | Min | Max |
|-----------------------------------|-------|--------|----------------------|---------------------|--------|---------|
| V-dem electoral democracy index | 1,320 | 6.325 | 2.772 | 1.577 | 0.297 | 9.725 |
| Combined Polity IV index | 1,248 | 6.217 | 3.664 | 1.997 | 0 | 10 |
| Lexical electoral democracy index | 1,281 | 3.859 | 2.332 | 1.468 | 0 | 6 |
| Boix et.al. (2013) democracy | 1,275 | 0.536 | 0.499 | 0.300 | 0 | 1 |
| Cheibub et.al. (2010) democracy | 1,072 | 0.517 | 0.500 | 0.279 | 0 | 1 |
| Universal suffrage | 1,292 | 0.760 | 0.427 | 0.337 | 0 | 1 |
| Male suffrage | 1,292 | 0.802 | 0.399 | 0.307 | 0 | 1 |
| % in manufacturing | 1,367 | 5.320 | 4.023 | 1.518 | 0.016 | 19.041 |
| V-dem equality index | 1,292 | 0.589 | 0.285 | 0.114 | 0.034 | 0.986 |
| Average years of education | 1,073 | 5.957 | 3.094 | 1.944 | 0.359 | 13.105 |
| Natural log of GDP per capita | 1,200 | 8.695 | 1.074 | 0.531 | 5.922 | 11.720 |
| Urbanization rate | 1,299 | 43.826 | 24.632 | 13.669 | 1.600 | 100 |
| Oil rents (% of GDP) | 1,126 | 3.644 | 11.200 | 6.081 | 0 | 128.066 |
| No. of past transitions | 1,256 | 0.391 | 0.738 | 0.318 | 0 | 4 |
| Economic crisis | 1,200 | 0.274 | 0.446 | 0.415 | 0 | 1 |
| Duration regime | 1,275 | 45.655 | 49.078 | 24.009 | 1 | 216 |
| Property rights security | 1,321 | 6.397 | 2.485 | 1.057 | 0.032 | 9.525 |
| Trade (% of GDP) | 1,094 | 2.181 | 8.421 | 5.570 | 0.008 | 172.893 |
| Authoritarian constitution | 1,150 | 0.118 | 0.323 | 0.229 | 0 | 1 |
| State control of economy | 1,321 | 0.332 | 1.198 | 0.673 | -4.018 | 2.657 |
| Political corruption | 1,311 | 0.434 | 0.301 | 0.092 | 0.006 | 0.969 |
| Aligned to West | 1,367 | 0.249 | 0.433 | 0.258 | 0 | 1 |
| Inter-state warfare | 1,367 | 0.072 | 0.258 | 0.210 | 0 | 1 |
| Intra-state warfare | 1,367 | 0.092 | 0.289 | 0.226 | 0 | 1 |

Note: Data is observed in years ending with 0 and 5's (i.e., 5 year regular intervals). Sample is subsetting to country-years when % in manufacturing is non-missing.

2.8.6 Accounting for mature deindustrialization

In the main text I test the effect of industrialization on democracy using a linear dynamic panel model with country and time fixed effects. This has three advantages. First, such models are easy to interpret. Second, this structure of fixed effects maximizes the amount of econometric identification that can be obtained by design, given the observational nature of the data. Last, this is the most widely used econometric specification used in the recent democratization literature (e.g., Acemoglu et al. (2008), Boix (2011), and Treisman (2015)).

In the context of my theory linear dynamic panel models with country and time fixed effects are, however, likely to underestimate the effect of industrialization on democracy. This is because my theory suggests that the higher labor productivity in manufacturing tends to generate a structural change in employment from manufacturing to modern services in highly industrialized countries which, importantly, still supports democracy (see Section 2.5 in the main text).

In Table 2.7 I take this into account by interacting my key % in manufacturing variable with a dummy that takes the value 1 if the country-year is a highly industrialized country in the post-1945 period *and* if this country-year has seen a decline in its level of manufacturing employment in the previous 5 years, and 0 otherwise.⁵²

As can be seen in Table 2.7 mature deindustrialization has a positive (but statistically insignificant) effect on democracy. This is in line with my theory that the shift towards modern service employment after widespread industrialization still supports democracy. The implied long-run effect of industrialization on democracy is estimated to be 0.814 within the null category of the mature deindustrialization dummy, which are country-years *without* mature deindustrialization. In line with the logic outlined above this is

⁵²Highly industrialized countries are coded as: Canada, United States, Norway, Sweden, Finland, Denmark, Ireland, United Kingdom, Netherlands, Germany, Belgium, Luxembourg, Austria, France, Switzerland, Portugal, Spain, Italy, Greece, Australia, New Zealand, Japan, South Korea, Taiwan, Singapore, and Hong Kong. Results are robust to using the OECD as the group identifier.

marginally larger than the baseline estimate of 0.907, which is obtained when simply pooling mature deindustrialization together with all other forms of (de)industrialization.

Table 2.7 Accounting for mature deindustrialization.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--|---------------------|----------------------|---------------------|---------------------|---------------------|---------------------|
| Democracy $t-1$ | 0.483*** (0.049) | 0.474*** (0.058) | 0.492*** (0.060) | 0.387*** (0.068) | 0.456*** (0.050) | 0.480*** (0.049) |
| % in manufacturing $t-1$ | 0.421** (0.134) | 0.571*** (0.153) | 0.492** (0.156) | 0.391* (0.163) | 0.402** (0.146) | 0.423** (0.140) |
| Mature deindustrialization $t-1$ | -0.244 (0.330) | -0.140 (0.363) | -0.054 (0.374) | -0.155 (0.356) | -0.348 (0.335) | -0.255 (0.331) |
| % in manufacturing $t-1$ * | 0.176 (0.179) | 0.174 (0.214) | 0.112 (0.200) | 0.149 (0.173) | 0.244 (0.189) | 0.174 (0.181) |
| Mature deindustrialization $t-1$ | | | | | | |
| Equality index (V-dem) $t-1$ | | 0.289 (0.205) | | | | |
| Log GDP per capita $t-1$ | | -0.729*** (0.183) | | | | |
| Years of education $t-1$ | | -0.037 (0.284) | | | | |
| Urbanization rate $t-1$ | | 0.400* (0.190) | | | | |
| Oil rents (% of GDP) $t-1$ | | | 0.061 (0.100) | | | |
| No. of past transitions $t-1$ | | | 0.405*** (0.094) | | | |
| Economic crisis $t-1$ | | | -0.004 (0.089) | | | |
| Duration regime $t-1$ | | | | -0.318** (0.097) | | |
| Property rights security $t-1$ | | | | 0.158* (0.072) | | |
| Trade (% of GDP) $t-1$ | | | | -0.055 (0.048) | | |
| Authoritarian constitution $t-1$ | | | | | 0.020 (0.253) | |
| State control of economy $t-1$ | | | | | 0.227 (0.124) | |
| Political corruption $t-1$ | | | | | -0.293 (0.151) | |
| Aligned to West $t-1$ | | | | | | -0.078 (0.120) |
| Inter-state warfare $t-1$ | | | | | | -0.226 (0.155) |
| Intra-state warfare $t-1$ | | | | | | -0.047 (0.201) |
| Implied long-run effect of % in manufacturing $t-1$ | 0.814** (0.280) | 1.087** (0.331) | 0.968** (0.328) | 0.639* (0.282) | 0.739* (0.289) | 0.813** (0.292) |
| Country fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Time fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1294 | 948 | 1037 | 1080 | 1134 | 1294 |
| Countries | 145 | 95 | 126 | 128 | 135 | 145 |

Notes: OLS regressions. The dependent variable is the electoral democracy index of V-dem. See Appendix 2.8.3 for measurement of control variables. Data is observed in years ending with 0 and 5's (i.e., 5 year regular intervals). Robust standard errors clustered on the country level in parentheses.

*** p<0.001, ** p<0.01, * p<0.05.

2.8.7 Alternative measures of education and inequality

Table 2.8 Alternative measures of education and inequality at the time of democratization in currently highly developed countries.

| Country | Year | Avg. years of primary edu | Avg. years of secondary edu | Avg. years of tertiary edu | % of GDP to richest 10% | % of GDP to richest 1% |
|--|-------------------|------------------------------|--------------------------------|-------------------------------|----------------------------|---------------------------|
| Panel A: Major European countries | | | | | | |
| Austria | 1921 | 4.6 (42.1%) | 0.4 (97.4%) | 0.05 (86.8%) | N/A | N/A |
| Belgium | 1949 | 5.3 (28.9%) | 1.2 (84.2%) | 0.12 (68.4%) | N/A | N/A |
| France | 1947 | 3.8 (65.8%) | 0.5 (97.4%) | 0.05 (86.8%) | 36.0 (5.2%) | 10.8 (7.8%) |
| Italy | 1947 | 3.6 (65.8%) | 0.6 (97.4%) | 0.04 (86.8%) | 37.2 (5.2%) | 13.2 (14.3%) |
| Netherlands | 1922 | 4.1 (57.9%) | 0.1 (100%) | 0.01 (100%) | 39.3 (10.4%) | 16.5 (50.6%) |
| Portugal | 1976 | 3.3 (76.3%) | 1.1 (86.8%) | 0.08 (81.6%) | N/A | N/A |
| Spain | 1978 | 4.7 (42.1%) | 1.9 (60.5%) | 0.24 (47.4%) | 35.6 (5.2%) | 12.0 (7.8%) |
| Sweden | 1921 | 4.3 (55.3%) | 0.5 (97.4%) | 0.03 (92.1%) | 39.0 (9.1%) | 21.7 (90.9%) |
| Switzerland | 1971 | 6.7 (0%) | 3.0 (26.3%) | 0.32 (44.7%) | 31.9 (1.3%) | 12.3 (9.1%) |
| West Germany | 1949 | N/A | N/A | N/A | N/A | N/A |
| Panel B: UK and British offshoots | | | | | | |
| Australia | 1901 | 2.9 (81.6%) | 0.2 (100%) | 0.01 (100%) | 50.6 (70.1%) | 16.9 (55.8%) |
| Canada | 1921 | 4.3 (55.3%) | 1.2 (84.2%) | 0.14 (60.5%) | 56.3 (92.2%) | 16.0 (48.1%) |
| New Zealand | 1893 | 4.1 (57.9%) | 0.1 (100%) | 0.01 (100%) | 47.7 (45.5%) | 15.9 (48.1%) |
| United Kingdom | 1928 | 4.3 (55.3%) | 0.8 (94.7%) | 0.03 (92.1%) | 39.8 (11.7%) | 22.1 (90.9%) |
| United States | 1920 [‡] | 5.0 (31.6%) | 2.0 (55.2%) | 0.20 (55.3%) | 44.3 (27.3%) | 19.2 (75.3%) |
| United States | 1965 | 5.7 (13.2%) | 4.2 (13.2%) | 0.59 (13.2%) | 36.7 (5.2%) | 13.0 (14.3%) |
| Panel C: East-Asian miracle economies | | | | | | |
| Japan | 1952 | 5.2 (31.6%) | 1.7 (68.4%) | 0.15 (57.9%) | 38.3 (7.8%) | 11.1 (7.8%) |
| South Korea | 1988 | 5.6 (18.4%) | 4.0 (15.8%) | 0.49 (23.7%) | 34.1 (3.9%) | 9.8 (5.2%) |
| Taiwan | 1997 | 5.3 (28.9%) | 4.0 (15.8%) | 0.57 (13.2%) | 30.0 (0%) | 7.7 (1.3%) |

Notes: Entry is year that currently highly developed countries became stable democracies. This is operationalized as the first year in which countries are coded as fully democratic by the V-dem electoral democracy index, universal adult suffrage is effective, and both these variables remain unchanged until 2015. Percentage of countries that are coded as nondemocracies by V-dem in 2015 with an equal or higher level of education and equality (i.e., lower level of top 1% and top 10% income share) in parentheses. Years of primary, secondary, and tertiary education data comes from Lee and Lee (2016). Top 10% and top 1% income share data comes from the World Inequality Database. If data is missing it is imputed from earlier or later years within a 5 year interval. N/A indicates that data is not available.

[†] Includes construction and mining together with manufacturing.

[‡] Includes construction together with manufacturing.

[‡] Refers to the year that universal suffrage for whites was introduced.

2.8.8 Controls for mining, farming, construction and manufacturing output

My theory suggests that a shift towards a large share of employment in manufacturing tends to increase the likelihood that a country becomes a stable democracy. In line with this hypothesis I find that the percentage of the population employed in manufacturing is strongly correlated with democracy, even after accounting for country and time fixed effects and a large number of time-varying control variables.

Here I examine whether this effect is truly driven by changes in manufacturing employment, as opposed to other structural economic changes that tend to correlate with changes in manufacturing employment. To do so I use data from GGDC (2015) and Mitchell (2013) to control for the share of the population employed in agriculture, construction, and mining, and data from GGDC (2015), UNIDO (2015), and the World Bank Development Indicators on manufacturing value added.

As can be seen in Table 2.9 manufacturing employment remains strongly positively correlated with democracy after accounting for employment agriculture, construction, and mining, and after accounting for manufacturing value added. Furthermore, agricultural, construction, and mining employment, as well as manufacturing output, are themselves negatively associated with democracy after accounting for manufacturing employment. This increases my confidence that it really is manufacturing employment that is driving the effect.

Table 2.9 Accounting for employment in agriculture, construction, and mining, and manufacturing output.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| Democracy $t-1$ | 0.484*** (0.047) | 0.508*** (0.056) | 0.511*** (0.056) | 0.505*** (0.068) | 0.417*** (0.056) | 0.414*** (0.055) |
| % in manufacturing $t-1$ | 0.468*** (0.116) | 0.447** (0.137) | 0.463*** (0.132) | 0.580*** (0.134) | 0.270+ (0.156) | 0.329* (0.158) |
| % in agriculture $t-1$ | | -0.239 (0.269) | | | | |
| % in construction $t-1$ | | | -0.126 (0.161) | | | |
| % in mining $t-1$ | | | | -0.153 (0.161) | | |
| MVA per capita $t-1$ | | | | | -0.287* (0.145) | |
| MVA (% of GDP) $t-1$ | | | | | | -0.535** (0.177) |
| Implied long-run effect of % in manufacturing $t-1$ | 0.907*** (0.229) | 0.908** (0.280) | 0.947*** (0.272) | 1.171*** (0.275) | 0.464+ (0.269) | 0.561* (0.269) |
| Country fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Time fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1294 | 1014 | 1014 | 794 | 923 | 924 |
| Countries | 145 | 100 | 100 | 80 | 124 | 124 |

Notes: OLS regressions. The dependent variable is the liberal democracy index of V-dem. Data is observed in years ending with 0 and 5's (i.e., 5 year regular intervals). Robust standard errors clustered on the country level in parentheses.

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.10$.

Chapter 3

Paper II: Does Industrialization Cause Democratization? Quasi-Experimental Evidence from the Norwegian Industrial Revolution

Abstract

Recent work has shown that a large share of employment in manufacturing (i.e., industrialization) is strongly correlated with democracy, even after accounting for country and time fixed effects, time trends, and a wide range of plausible control variables. In this paper we exploit a unique quasi-experiment in 19th- and early 20th-century Norway that allows us to examine whether this correlation between industrialization and democratization is causal. Using novel roll-call data from the Norwegian national parliament we study whether MPs that represented more rapidly industrializing districts were more likely to vote for suffrage extensions over the 1891 to 1906 period. For causal identification we exploit that Norwegian districts with a greater geographical potential for hydropower generation were significantly more likely to industrialize after the nationwide introduction of hydroelectricity in 1892. Preliminary results suggest that industrialization did cause democratization in Norway.¹²

¹This is a preliminary version of a paper that I am currently working on together with professors Magnus Rasmussen and Tore Wig (University of Oslo). I came up with the idea of this paper independently. Given that Magnus Rasmussen and Tore Wig happened to work on a similar project we decided to join forces. The text and data analysis included in this dissertation are written/generated by me alone. The eventual paper that is to be submitted to a peer-reviewed journal will be written by all three authors. Letters from professors Rasmussen and Wig attesting to these facts are attached in Appendices 3.8.1 and 3.8.2.

²I am grateful to Michael Albertus and seminar participants at the 2021 Annual Meeting of the American Political Science Association for helpful comments and suggestions.

3.1 Introduction

van Noort (2021) argues that a large share of employment in manufacturing (i.e., industrialization) tends to make democratic forms of government more likely. In line with this hypothesis van Noort (2021) shows that industrialization is strongly correlated with democracy, even after accounting for country and time fixed effects, time trends, and a wide range of plausible control variables.

While suggestive of an important effect of industrialization on democracy van Noort’s (2021) two-way fixed effects regressions can ultimately not proof beyond a reasonable doubt that industrialization causes (rather than merely correlates with) democracy. This is because it is possible that there are other, perhaps currently unknown, variables that vary over time *and* cause both industrialization and democracy, inducing spurious correlation in two-way fixed effects regressions.

In this paper we circumvent this causal identification issue by analyzing the results of a unique quasi-experiment in 19th- and early 20th-century Norway. More specifically, we use novel roll-call data from the Norwegian national parliament to examine whether Norwegian members of parliament (MPs) that represented more rapidly industrializing districts were more likely to vote in favor of suffrage extensions during the 1891 to 1906 period.³ For causal identification we follow Leknes and Modalsli (2020) and exploit that Norwegian districts with a greater geographical potential for hydropower generation were significantly more likely to industrialize after the nationwide introduction of hydroelectricity in 1892.

In line with van Noort (2021) we find evidence that suggests that industrialization caused democratization in Norway. More specifically, instrumenting the percentage of the workforce employed in manufacturing with the interaction between the geographical potential to generate hydropower (which is determined by the slope of the landscape

³Note that Norway introduced universal male suffrage in 1898 and universal adult suffrage in 1913. We are currently collecting the data necessary to extend the analysis to 1913.

and the volume of river ways) and years since the introduction of hydroelectricity, suggests that a 1% increase in manufacturing employment increased the percentage of votes in favor of extending the suffrage with about 8.7% on average (mean of votes in favor of suffrage extensions = 22.6%). This effect is robust to district fixed effects and parametrically accounting for a number of potential violations of the exclusion restriction.

This result has two important implications. First, the result suggests that industrialization is not only strongly correlated with democracy but also that at least in one case industrialization appears to have caused democratization. Second, the result suggests that the relationship between industrialization and democracy occurs not only on the national level but that at least in one case the effect of industrialization on democracy is also detectable on the subnational level.

3.2 Background of Norwegian case

Norway was in 1891 still relatively little industrialized, particularly as compared to Britain, Belgium, and many other North-West European countries. This was largely the result of the fact that Norway has very little coal deposits, and coal was at the time very expensive to trade across countries (particularly to Norway where major waterways tended to be frozen for significant parts of the year).⁴ Norway did have a large latent capacity to generate hydropower, but this was for most of the 19th century not sufficient to generate and store enough energy to imitate the energy-intensive coal-based manufacturing technologies from Britain (Vries, 2001).

⁴Coal was limitedly internationally tradable until far into the 20th century because of its exceptionally heavy weight/large volume relative to its per kg/m³ monetary value (Bairoch, 1995; Hummels, 2007; Wårell, 2006). Even today less than 18% of all coal is traded internationally (WCA, 2019), and even today transportation costs *within* the United States are typically higher than the value of the transported coal itself (EIA, 2018).

This fundamental energy bottleneck was radically alleviated with the invention and widespread introduction of hydroelectricity—a technology that was developed in the United States and first introduced in Norway in 1892 via a letter to parliament by Norwegian parliamentarian, and later prime minister, Gunnar Knutsen. Hydroelectricity greatly spurred industrialization in Norway because it allowed Norway to tap into the large amount of electricity-based manufacturing technologies that had already been developed in more advanced economies (Hodne and Grytten, 2002; Thomson, 1938).

Crucially, hydropower could not be generated in all parts of Norway. Instead, large-scale hydropower plants could only be fruitfully developed in areas that happened to have water falls with sufficient water volume and velocity (Crafts and Wolf, 2014; Borge, Parmer and Torvik, 2015; Leknes and Modalsli, 2020). Because a cross-regional electricity grid was only built in 1922 manufacturing firms that wanted to exploit the electricity generated by hydropower had to produce in very close proximity to the hydropower plants themselves. Localities with abundant hydropower potential—almost all of which were (until the hydropower revolution) agricultural economies—suddenly became rapid industrializers after 1892.

We exploit these peculiarities of Norwegian economic history by studying whether MPs who represented constituencies that more rapidly industrialized after 1892 *because* they happened to have the geographical potential to generate hydropower, were significantly more likely to vote for suffrage extensions.

This causal identification strategy is credible because (1) the geographical characteristics that allow for hydropower generation are plausibly exogenous to industrialization and democratization, and (2) because it is unlikely that the capacity to generate hydropower could have affected MPs support for suffrage extensions over the 1891 to

1906 period through channels other than industrialization (particularly conditional on district fixed effects).⁵

3.3 Data

3.3.1 Measurement of manufacturing employment

We generate a district-year level dataset on the percentage of the workforce employed in manufacturing using data from the Norwegian censuses of 1891, 1900, and 1910. Given that many roll-call votes on suffrage extensions occurred in the years in between 1891, 1900, and 1910 we use linear interpolation to avoid gaps in the time series. If the boundaries of a district change we include the new district as a unique observation from the year of the redistricting onwards. This does not generally drop much data because we have the manufacturing employment data on the municipality level (which' boundaries rarely change during the 1892 to 1906 time period) so that we can derive the level of industrialization in a new electoral district even if the redistricting did not occur in a census year (municipalities are smaller units that perfectly map into districts).

3.3.2 Measurement of support for democracy

Formal political institutions do not vary within Norway. We therefore create a novel subnational measure of support for democracy by analyzing the voting pattern of

⁵Note that Norway was in a union with Sweden from 1814 until 1905. The two states kept separate constitutions, laws, legislatures, administrations, state churches, armed forces, and currencies, but shared the same king and foreign policy. The Union was forced upon Norway by Sweden in the Treaty of Kiel. Throughout the union Sweden tended to be the dominant party, with the king predominantly residing in Stockholm. Crucially for our purpose, however, Norwegian MPs had the power to extend the franchise independently from Sweden. This is evidenced by the fact that many major franchise extensions (including the granting of universal male suffrage in 1898) occurred before 1905, even though Sweden had a much more restricted franchise at the time (e.g., Sweden only introduced universal male suffrage in 1909).

Norwegian MP's on all 30 suffrage bills that were put up to a vote in the Norwegian national parliament from 1891 to 1906. As our outcome variable we take the percentage of suffrage extending bills a particular MP in a particular district-year voted in favor off.⁶⁷

3.3.3 Measurement of hydropower potential

We take our instrument from Leknes and Modalsli (2020) who have collected data on the geographical potential to generate hydropower in Norwegian subnational regions.⁸⁹ Leknes and Modalsli (2020) measure the geographical potential to generate hydropower by the combination of the slope of the landscape, the water flow, and river length. More specifically, the instrument is generated by solving the following equation:

$$HydroPotential_m = Z_m = \frac{\sum_{v=750}^{v=10} (River4_{vm} \times v)}{Area_m} \quad (3.1)$$

where v is the water volume class used by the Norwegian Water and Energy Directorate and m indexes municipalities.¹⁰ Leknes and Modalsli (2020) estimate the gradient of each stretch of river using a GIS terrain model with 50×50 -meter grids based on data from Norway Digital. They focus on river stretches with a gradient of 4 degrees or more. $River4_{vm}$ in equation (3.1) is therefore meters of river with water volume class

⁶If a district-year has more than one MP the outcome variable is simply the average of all MPs for that particular district-year.

⁷In the future we intend to collect data that would allow us to create a variable capturing whether an MP voted in favor of universal adult suffrage, or not. Our current outcome variable does not have an absolute reference point (i.e., our current outcome variable simply captures whether an MP supported a bill that would *extent* the number of people with the right to vote from what it was at that point in time).

⁸Leknes and Modalsli's (2020) approach to measuring the geographical potential to generate hydropower is standard in the economic geography literature (e.g., Crafts and Wolf (2014), Borge, Parmer and Torvik (2015)).

⁹Note that Leknes and Modalsli (2020) drop several urban municipalities with essentially zero potential to generate hydropower.

¹⁰The Norwegian Water and Energy Directorate classifies rivers into ten categories based on cubic meters per second. The categories are: 1-10, 10-50, 50-100, 100-150, 150-200, 200-250, 250-300, 300-400, 400-600, and 600-750.

v , in terrain with a slope of at least 4 degrees, in municipality m . Leknes and Modalsli (2020) subsequently multiply for each river class the meters of rivers by the maximum water flow in that class. Last, to create a scale-independent measure of geographical hydropower potential, Leknes and Modalsli (2020) take the sum of these products and divide by total area (km^2).

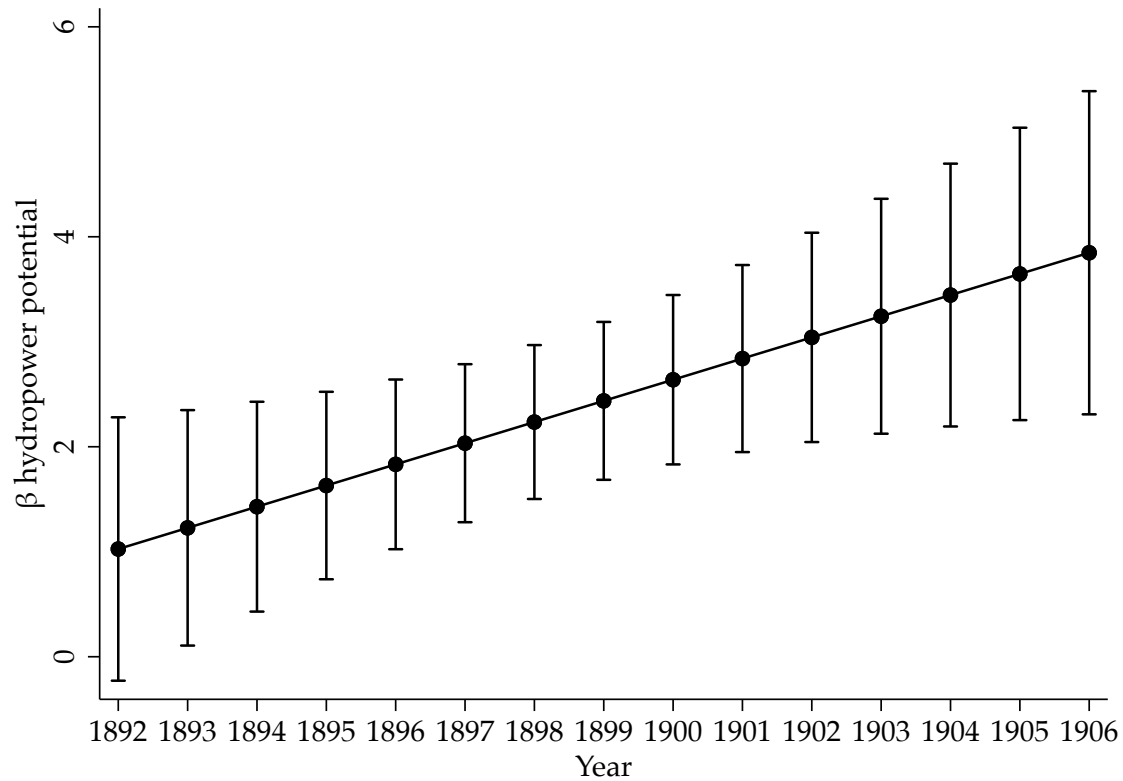
Because our measure of MPs support for suffrage extensions varies on the district-year level we create our measure of geographical hydropower potential by simply taking the average hydropower potential of all municipalities in a particular district-year.

3.4 Identification strategy

3.4.1 Estimation technique

Hydroelectricity was first introduced in Norway in 1892. We expect that from that year onwards areas with a greater geographical potential to generate hydropower were significantly more likely to industrialize, holding everything else constant. In practice, however, it took some time to adapt to the hydroelectricity shock (hydropower plants had to be built, workers had to relocate, supply chains had to be restructured, etc.). In line with this assertion districts with more capacity to generate hydropower experienced a larger increase in the percentage of the workforce employed in manufacturing from 1892 onwards, but the industrialization-inducing effect of hydropower potential increased significantly with time since 1892, up until at least 1906 (see figure 3.1). More specifically, an OLS regression of the percentage of the workforce employed in manufacturing on hydropower potential interacted with years since 1892 suggests that hydropower potential increased industrialization about 3.8 times more in 1906 as compared to 1892 (see difference in point estimate at either end of figure 3.1).

Fig. 3.1 Effect of hydropower potential on industrialization moderated by years since 1892.



Note: Plot of how the effect of geographical hydropower potential on the percentage of the workforce employed in manufacturing changes with years since 1892. Dots represent point estimates. Bars represent 95% confidence intervals. Results are derived from an OLS regression of the percentage of the workforce employed in manufacturing on hydropower potential interacted with years since 1892.

We take this lagged process into account by instrumenting industrialization with the *interaction* between geographical hydropower potential and a count variable capturing years since 1892. Besides increasing instrument strength this has the additional advantage of inducing within-district variation, which creates the possibility of including district fixed effects.¹¹ Including district fixed effects is important in this case in order to account for time-invariant factors—such as better access to the outside world through

¹¹Note that hydropower potential itself is time-invariant because it is generated by geographical features that do not vary over the 1891 to 1906 period.

waterways—which may have affected MPs support for suffrage extensions through channels besides industrialization.

Taking this time dimension into account we estimate the following system of equations with two-stage least squares (2SLS):

$$X_{it-1} = \alpha_0 + \alpha_1 Z_i + \alpha_2 Year - 1892_{t-1} + \alpha_3 Z_i \times Year - 1892_{t-1} + \delta_i + \varepsilon_{it} \quad (3.2)$$

$$Y_{it} = \beta_0 + \beta_1 \hat{X}_{it-1} + \delta_i + \epsilon_{it} \quad (3.3)$$

where X_{it} is the percentage of the workforce employed in manufacturing and Y_{it} is the percentage of franchise extending bills supported by the MP of district i in year t . Z_i is district i 's geographical hydropower potential which is time-invariant and interacted with a count variable capturing years since 1892. δ are a full set of district fixed effects. Given the district fixed effects the time-invariant variable Z drops out of the equation so that the predicted variation in X that is used to estimate the effect in the second stage (i.e., equation (3.3)) is solely derived from the *interaction* between a district' geographical hydropower potential and time since the availability of hydroelectricity.

3.4.2 Instrument validity

For causal identification of equations (3.2) and (3.3) five conditions must hold.¹² First, the instrument has to be *relevant*, meaning that Norwegian districts with a greater geographical potential to generate hydropower should have industrialized faster after the introduction of hydroelectricity in 1892. Second, the instrument has to be *exogenous*, meaning that the interaction between the geographical potential to generate hydropower and years since the nationwide introduction of hydroelectricity should not itself be caused by MPs support for suffrage extensions, should not itself

¹²See Angrist and Pischke (2008, chapter 4) and Wooldridge (2013, chapter 15) for a detailed treatment of the assumptions underlying instrumental variable analysis.

be caused by a district-year' level of industrialization, and should not itself be caused by other factors that also simultaneously affect industrialization and MPs support for suffrage extensions. Third, the instrument has to be *excludable*, meaning that the interaction between the geographical potential to generate hydropower and years since the nationwide introduction of hydroelectricity should not have affected MPs support for suffrage extensions through any other channel besides industrialization. Fourth, the instrument has to be *monotonic*, meaning that a greater geographical potential to generate hydropower should not have caused any district to *deindustrialize* after the nationwide introduction of hydroelectricity. Last, the *stable unit treatment value* assumption (SUTVA) has to hold, meaning that a district' geographical potential to generate hydropower interacted with years since 1892 should not have affected industrialization patterns in other districts.

Below we explain why we believe that these preconditions are likely to hold.

Instrument relevance

Instrument relevance is the only assumption underlying instrumental variable analysis that is directly testable. We do so by examining the Shea (1997) partial R^2 (a measure of economic significance) and the Kleibergen and Paap (2006) F-statistic (a measure of statistical significance) of the first-stage regression of industrialization on the interaction between geographical hydropower potential and years since 1892.

It is nonetheless noteworthy that there are two important theoretical reasons to expect that the instrument will be strong. First, and as previously mentioned, because of a lack of coal Norwegian manufacturing over the 1891 to 1906 period relied almost entirely on hydropower, which can only be generated when certain geographical preconditions are met (Thomson, 1938). Second, until 1917 the vast majority of hydropower plants in Norway were financed by foreign investors. This is important because foreign investors had a strong economic incentive to build hydropower plants

in locations where the geographical features were such that hydropower could be most easily generated (as opposed to governments, which may, for political reasons, allocate hydropower investments to regions with lower geographical potential) (Leknes and Modalsli, 2020).

Instrument exogeneity

Exogeneity in this case requires that the fact that the effect of hydropower potential on industrialization increases with years after 1892 (see figure 3.1) is not due to a district' pre-existing level of industrialization, is not due a district' MPs pre-existing level of support for suffrage extensions, and is not due to other time-varying factors that may determine both industrialization and MPs support for suffrage extensions.¹³

This assumption is plausible in this case because both the introduction of hydroelectricity in 1892 and the geographical potential to generate hydropower are plausibly orthogonal to industrialization and MPs support for suffrage extensions within Norwegian districts.

Leknes and Modalsli (2020) generate the hydropower potential instrument from a region' average elevation, water flow, and river length (see equation (3.1))—all factors that cannot plausibly be affected by MPs support for suffrage extensions, pre-existing levels of industrialization, and/or other time-varying factors that could simultaneously determine both industrialization and MPs support for suffrage extensions.

Hydroelectricity was meanwhile invented in the United States, without any apparent relationship to the political-economic conditions in Norwegian districts. After the first hydroelectricity plant opened along the Fox River in Appleton, Wisconsin, the then MP, and later prime minister of Norway, Gunnar Knutsen, wrote a letter to parliament in 1892, urging parliament to adopt hydroelectricity as the nation's core industrial energy source. There is, to the best of our knowledge, no evidence that suggests that Knutsen

¹³Note that time-invariant confounders are accounted for by district fixed effects.

laid out the industrial potential of hydroelectricity to parliament for reasons that are endogenous to district-level differences in industrialization and/or regional differences in MPs support for suffrage extensions. Instead, Knutsen's letter and the introduction of hydroelectricity in Norway more generally was simply part of a nationwide effort to catch-up industrially to more advanced neighboring countries in Western Europe (Thomson, 1938).

Instrument excludability

Excludability in this case requires that the interaction between the geographical potential to generate hydropower and years since 1892 has not affected MPs support for suffrage extensions through any other channel besides industrialization. We regard this assumption as plausible because we are not aware of any other factor that could be correlated with the geographical potential to generate hydropower interacted with years since 1892 *and* which may affect MPs support for suffrage extensions.¹⁴

One possibility is nonetheless that the interaction between the geographical potential to generate hydropower and time since the introduction of hydroelectricity increased manufacturing *output*, which in turn may have had an independent effect on MPs support for suffrage extensions besides manufacturing *employment* (which is the key variable in van Noort's (2021) theory). For this reason we also study the reduced form effect of our instrument on MPs support for suffrage extensions, which does not suffer from this potential problem but instead leaves undefined if the result is driven by manufacturing employment, manufacturing output, or both.

¹⁴To be sure, it is theoretically possible that a greater geographical potential for hydropower spurs industrialization after the introduction of hydroelectricity, and that this in turn triggers other socio-economic processes (e.g., urbanization), which may in turn affect MPs support for suffrage extensions. This would, however, not be a violation of the exclusion restriction, but rather an alternative causal mechanism linking industrialization with MPs support for suffrage extensions.

Instrument monotonicity

Instrumental variable analysis can at best only identify the local average treatment effect—i.e., the effect of industrialization on MPs support for suffrage extensions among those district-years that have seen an increase in industrialization due to the combination of a pre-existing geographical suitability for hydropower generation and time since the introduction of hydroelectricity. For the the local average treatment effect to be identified one has to assume monotonicity. Monotonicity implies, in this case, that the interaction between a district’ geographical potential to generate hydropower and years since 1892 never actually caused a district’ percentage of the workforce engaged in manufacturing to *decline*.

We regard this monotonicity assumption as plausible in this case. While it is certainly true that the relationship between hydropower potential and manufacturing employment is far from perfect (because industrialization is also determined by many other factors—e.g., public policy—and because the most geographically suitable waterfalls may occasionally have been left unexploited for other reasons—e.g., remoteness to markets), we are not aware of any Norwegian district that actually *deindustrialized* after 1892 *because* it had a large geographical potential to generate hydropower (which is what is necessary to violate monotonicity).

Instrument SUTVA

For the local average treatment effect to be identified one must assume that the level of industrialization in any particular district-year is unaffected by the interaction between the geographical potential to generate hydropower and years since 1892 in all other district-years.

This SUTVA assumption is plausible in this case because the first major transmission line that could be used to transport electricity across Norwegian districts only came into operation in 1922. This means that over the entire period of interest

manufacturing production had to essentially take place in very close proximity to where the hydroelectricity was generated (Vogt, 1971; Hughes, 1993). It is unlikely therefore that any district-year' level of industrialization was affected by the interaction between another district-year' geographical potential to generate hydropower and time since the nationwide introduction of hydroelectricity.

3.4.3 Generalizability from local average treatment effect to average treatment effect

Assuming exogeneity, excludability, monotonicity, and SUTVA the instrumental variable estimates obtained from estimating equations (3.2) and (3.3) identify the causal effect of a unit increase in the percentage of the workforce employed in manufacturing on MPs support for suffrage extensions among district-years that have seen their level of industrialization increase *because* of the combination of a pre-existing geographical potential to generate hydropower and years since the nationwide introduction of hydroelectricity (i.e., the effect is estimated among the “compliers” – Imbens and Angrist (1994)).

Generalization from the local average treatment effect to the average treatment effect (i.e., the causal effect of a unit increase in manufacturing employment across all Norwegian districts over the 1891 to 1906 period) requires that a 1% increase in manufacturing employment induced by the exploitation of hydroelectricity has the same effect on MPs support for suffrage extensions as a 1% increase in manufacturing employment due to other reasons.

It is possible that in Norway hydropower-induced industrialization may have had different effects on MPs support for suffrage extensions, as compared to non-hydropower-induced industrialization. This is because in Norway most of the useful waterfalls, and thus most of the places where hydroplants and manufacturing facilities were built over the 1891 to 1906 period, turned out to be located in very remote and rural areas

(Leknes and Modalsli, 2020). If one follows van Noort (2021) in assuming that industrialization tends to lead to democracy through increasing the mobilization capacity of disenfranchised groups, and that mass revolts in urban areas are particularly disruptive and particularly threatening to political elites (Aidt and Franck, 2019; Campante, Do and Guimaraes, 2019), one should expect that the local average treatment effect that we recover, if anything, *understates* the average treatment effect of industrialization on MPs support for suffrage extensions.

3.5 Results

Table 3.1 reports the main results. In column (1) I use ordinary least squares (OLS) to regress the percentage of suffrage extending bills supported by an MP on the percentage of the workforce employed in manufacturing, together with district fixed effects. The point estimate of 3.622 suggests that a 1% increase in the workforce employed in manufacturing is estimated to lead on average to a 3.6% increase in the percentage of suffrage extending bills supported by a district-year' MP. This is a substantively large effect given that the mean of the percentage of suffrage extending bills supported by an MP is only 22.6%. Standardizing using z-scores suggests that each standard deviation increase in industrialization is estimated to lead on average to a 1.025 standard deviation increase in the percentage of suffrage extending bills supported by an MP.

Importantly, the OLS estimate in column (1) may be biased because of endogeneity and/or measurement error. More specifically, random measurement error in the

Table 3.1 Effect of industrialization on MPs support for suffrage extensions in Norway (1892–1906).

| | (1) Baseline (OLS) | (2) First stage (2SLS) | (3) Second stage (2SLS) | (4) Reduced form (OLS) |
|----------------------------------|--------------------------|------------------------------|-------------------------------|------------------------------|
| % in manufacturing $t-1$ | 3.622*** (1.060) | | 8.708*** (2.808) | |
| Hydropotential X Year–1892 $t-1$ | | 0.262*** (0.035) | | 1.934* (1.122) |
| Kleibergen-Paap F statistic | – | 74.2 | – | – |
| Shea partial R ² | – | 0.519 | – | – |
| District FE | Yes | Yes | Yes | Yes |
| Districts | 270 | 268 | 268 | 270 |
| Observations | 43 | 41 | 41 | 43 |

Notes: OLS and 2SLS regressions. The dependent variable is the percentage of suffrage extending bills supported by a district-year’ MP. Robust standard errors clustered on the district level in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

creation of the industrialization variable would bias the OLS estimate towards zero,¹⁵¹⁶ unobserved variables that increase (decrease) both industrialization and MPs support for suffrage extensions would cause OLS to overstate the effect of industrialization, and unobserved variables that increase (decrease) industrialization and decrease (increase) MPs support for suffrage extensions would cause OLS to understate the effect.

As it can fundamentally not be known whether any of these biases are present we instrument industrialization with the geographical potential to generate hydropower interacted with a count variable capturing the number of years since 1892 (i.e., the year that hydroelectricity first became available in Norway). This more credibly identifies the effect of industrialization by only focusing on district-years that experienced

¹⁵Note that systematic measurement error in the creation of the industrialization variable would bias the OLS estimate in an unknown direction (Wooldridge, 2013). For this to happen the Norwegian censuses of 1891, 1900, and 1910 should have over/underestimated the level of industrialization in particular districts *because* of their MPs level of support for suffrage extensions. We have no indication that this occurred.

¹⁶2SLS removes random measurement error in the industrialization variable by including it in the error term of equation (3.2), together with all other within-district variance in industrialization that cannot be explained by the interaction between the geographical potential to generate hydropower and time since the introduction of hydroelectricity.

an increase in their level of industrialization because they happened to have the geographical capacity to generate hydropower and happened to exist at a time when they had had enough years to economically exploit the availability of hydroelectricity technology.

Column (2) shows the first-stage results. As can be seen the instrument strongly predicts industrialization in Norway over the 1891 to 1906 period. While the point estimate does not have an intuitive interpretation in this regression (because of the units in which hydropower potential is measured) the effect is statistically significant on the 99% confidence level and the first-stage F statistic of 74.2 far exceeds Staiger and Stock's (1997) commonly used threshold of 10. The Shea (1997) partial R^2 suggests that the interaction between the geographical potential to generate hydropower and years since the introduction of hydroelectricity explains approximately 51.9% of the variation in industrialization within districts over time. This suggests that the local average treatment effect that is recovered by 2SLS is derived from a relatively broad subset of the data.

Column (3) shows the second-stage results. The 2SLS estimate suggests that a 1% increase in the workforce employed in manufacturing increases MPs votes in favor of suffrage extensions with about 8.7% on average. This effect is somewhat larger than the OLS estimate in column (1) but not statistically significantly so. Assuming exogeneity, excludability, monotonicity, and SUTVA this result implies that industrialization caused MPs to increase their support for suffrage extensions in Norway (1892-1906).

In column (4) of Table 3.1 I directly regress MPs support for suffrage extensions on the interaction between the geographical potential to generate hydropower and years since the introduction of hydroelectricity. While reduced form estimates have the disadvantage that they cannot be interpreted in substantive units they are useful in this case because they do not rely on a correctly specified model in the first-stage

and allow for the possibility that the interaction between the geographical potential to generate hydropower and years since the introduction of hydroelectricity (partly) affects MPs support for suffrage extensions through manufacturing output (rather than manufacturing employment). As can be seen in column (4) the reduced form estimates also suggest that industrialization caused MPs to increase their support for suffrage extensions.

3.6 Accounting for potential violations of the exclusion restriction

In Table 4.3 I examine the robustness of these results to controlling for a number of other factors that could theoretically have been affected by the hydropower revolution and could theoretically have had an independent effect on MPs support for suffrage extensions (and hence could potentially violate the crucial exclusion restriction underlying equations (3.2) and (3.3)).

In columns (1) and (2) I include the number of hydropower plants. As can be seen this leaves the results unchanged, suggesting that the effects found in Table 3.1 are not simply due to something particular around the production of hydroelectricity (rather than manufacturing).

In columns (3) and (4) I include the percentage of the workforce employed in agriculture. As can be seen this leaves the results unchanged, suggesting that it is indeed industrialization, and not simply a move out of agriculture induced by the hydropower revolution, that is driving the effect.

In columns (5) and (6) I include the natural log of the total workforce. Leknes and Modalsli (2020) find that places that had a greater geographical potential to generate hydropower experienced a significantly greater increase in the size of the labor force after the introduction of hydroelectricity. Controlling for this is important because our

Table 3.2 Effect of industrialization on MPs support for suffrage extensions after accounting for alternative channels.

| | (1) Baseline (OLS) | (2) Second stage (2SLS) | (3) Baseline (OLS) | (4) Second stage (2SLS) | (5) Baseline (OLS) | (6) Second stage (2SLS) |
|------------------------------|--------------------------|-------------------------------|--------------------------|-------------------------------|--------------------------|-------------------------------|
| % in manufacturing t_{-1} | 3.078*** (1.031) | 10.718*** (3.218) | 1.317 (0.944) | 7.104** (3.340) | 2.535 (1.699) | 11.898*** (3.840) |
| No. of hydroplants t_{-1} | 6.671 (7.715) | -12.742 (10.015) | | | | |
| % in agriculture t_{-1} | | | -2.579 (1.952) | -0.090 (2.039) | | |
| Log total workforce t_{-1} | | | | | 17.441 (24.478) | -68.934** (27.355) |
| Kleibergen-Paap F stat | — | 58.7 | — | 45.5 | — | 88.1 |
| Shea partial R ² | — | 0.394 | — | 0.515 | — | 0.462 |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Districts | 270 | 268 | 270 | 268 | 270 | 268 |
| Observations | 43 | 41 | 43 | 41 | 43 | 41 |

Notes: OLS and 2SLS regressions. The dependent variable is the percentage of suffrage extending bills supported by a district-year' MP. Robust standard errors clustered on the district level in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

industrialization variable is normalized using the size of the labor force, opening up the theoretical possibility that the results are affected by changes in the denominator rather than the numerator. As can be seen in columns (5) and (6) this is, however, not the case as the results remain unchanged when holding the size of the labor force constant.

3.7 Discussion

In this paper we test van Noort's (2021) hypothesis that a shift towards a large share of employment in manufacturing makes democratic forms of government more likely. To do so we exploit that Norwegian districts with a greater geographical potential for hydropower generation were significantly more likely to industrialize after the nationwide introduction of hydroelectricity in 1892. The admittedly preliminary results

suggest that industrialization did cause democratization in Norway. This provides complimentary evidence to van Noort's (2021) correlational results.

A crucial question for future research is how these results in Norway (1891-1906) generalize to other countries and other times.

On the one hand, one could reasonably conjecture that Norway (1891-1906) may be a relatively "easy" case for the hypothesis that industrialization causes democratization. This is because 19th-century Norway did not have a large landowning class that was as powerful as in many other early democratizers (Holmsen, 1958). This may be important for generalizability because large landowners have historically been one of the most consistent and most successful opponents of democracy. This is because large landowners, being a numerically small group that holds immobile assets that can be easily expropriated, have much to fear from the one-person, one-vote rule underlying democratic elections (Boix, 2003), and because the profits of large landowners have historically often relied on labor-repressive economic institutions that are difficult to sustain under democracy (Moore, 1966). Having said that, large landowners appear to have become significantly less opposed to democracy in recent decades (Albertus, 2017). This may be because mechanization has made large landowners significantly less dependent on labor-repressive institutions (Samuels and Thomson, 2020) and/or because financial globalization has provided large landowners with the opportunity to internationally diversify their asset portfolios (Freeman and Quinn, 2012). Following this logic suggests that the results of this paper may generalize more easily to more recent democratizers and to cases without a powerful landowners class (e.g., South Korea and Taiwan), but may not necessarily generalize to early democratizers with a powerful landowners class (e.g., Britain and France).

On the other hand, one could reasonably conjecture that the estimates obtained in this paper are likely to be relatively conservative and may well understate the effect

of industrialization on democracy in other countries and other times. As discussed in section 3.4.3, in Norway most of the places with useful waterfalls, and thus most of the places where hydroplants and manufacturing facilities were built, turned out to be located in very remote and rural areas. Given that in most other countries and times industrialization has tended to take place in more urbanized areas—which arguably increases the organizational and disruptive capacity of manufacturing workers (Aidt and Franck, 2019; Campante, Do and Guimaraes, 2019)—it could be possible that relatively much industrialization had to take place in Norway for a given unit change in democracy.

Delineating between these two possibilities ultimately requires the discovery of more quasi/natural experiments in other countries and times, which would allow us to empirically examine the extent to which the results reported in this paper generalize. We intend to conduct some of this research in the near future.

3.8 Appendix

3.8.1 Permission letter Professor Magnus Rasmussen (co-author)

UiO : University of Oslo

Date: 12 June 2022

To whom it may concern, My name is Magnus Bergli Rasmussen, I am an Assistant Professor in the Department of Political Science at the University of Oslo and an Associate Professor at the Oslo New University College. I am writing to you regarding my co-authored paper with Cambridge University PhD candidate Sam van Noort. The paper is preliminarily titled "Does Industrialization Cause Democratization? QuasiExperimental Evidence from the Norwegian Industrial Revolution". I hereby agree to Sam including this paper in his PhD dissertation. I hereby also declare that Sam made an independent contribution to this project. Sam has been a driving force in bringing about this paper, both on the theoretical and empirical part of the project. Sam came up with the idea of this paper independently and has done most of the current work on the paper. Given that me and Tore Wig happened to work on a similar paper we decided to all join forces. The text that will be included in Sam's PhD dissertation is written by Sam alone. The eventual paper that is to be submitted to a peer-reviewed journal will be written by all three authors.

Signature redacted for privacy
reasons

Magnus Bergli Rasmussen
Assistant Professor, Department of Political Science, University of Oslo
Associate Professor, Peace Research Institute Oslo



E-mail: m.b.rasmussen@stv.uio.no
www.uio.no

3.8.2 Permission letter Professor Tore Wig (co-author)

UiO : University of Oslo

Faculty/Institute/Department/Name

Date: 30 May 2022

To whom it may concern,

My name is Tore Wig, I am a Professor in the Department of Political Science at the University of Oslo and a Research Professor at the Peace Research Institute Oslo, PRIO.

I am writing to you regarding my co-authored paper with Cambridge University PhD candidate Sam van Noort. The paper is preliminarily titled "Does Industrialization Cause Democratization? Quasi-Experimental Evidence from the Norwegian Industrial Revolution".

I hereby agree to Sam including this paper in his PhD dissertation. I hereby also declare that Sam made very important and independent contributions to this project, and will be listed as a first author, with myself and Magnus Bergli Rasmussen as co-authors, in case this paper ever gets published in a peer-reviewed journal. Sam has made very important contributions to the overall idea, the theory, the data and the empirical analyses on the project.

Sam came up with the idea of this paper independently, and has done most of the work on the paper. Given that me and Magnus Rasmussen happened to work on a similar paper we decided to all join forces. The text that will be included in Sam's PhD dissertation is written by Sam alone. The eventual paper that is to be submitted to a peer-reviewed journal will be written by all three authors.

With best regards,

Tore Wig

Signature redacted for privacy
reasons

Tore Wig
Professor, Department of Political Science, University of Oslo
Research Professor, Peace Research Institute Oslo



Postal address:
E-mail:
www.uio.no

Chapter 4

Paper III: Does American Public Opinion Punish Overt Anti-Democratic Behavior by Politicians? Natural Experimental Evidence from the January 6 Insurrection

Abstract

Do American politicians that clearly violate democratic norms lose significant public support, or does the American public form little constraint on democratic backsliding? Existing studies have examined this fundamental question using hypothetical survey experiments which suffer from limited ecological validity and potential weak treatment bias. I overcome these problems by studying a novel natural experiment created by the fact that Donald Trump's incitement of the January 6 insurrection unexpectedly occurred while Gallup was conducting a nationally representative public opinion survey using random digit dialing. Comparing party identification among respondents that were interviewed just before, and just after, January 6, 2021 suggests that the Republican Party retained 78% of its pre-insurrection support base during the first 1.5 weeks. Even this modest loss was short-lived—in February 2021 the Republican Party already stood at 93% of its pre-insurrection support level. While not zero, the public constraint on democratic backsliding is remarkably limited.¹

¹I would like to thank Andy Eggers, Alex Gazmararian, Tanushree Goyal, Roza Khoban, Daniel Lyng, Dawn Teele, Duy Trinh, James Vreeland, and seminar participants at Princeton University and the 2021 Annual Meeting of the American Political Science Association for helpful comments and suggestions. I thank Bobray Bordelon, Jeremy Darrington, and Kris Hodgins for providing access to the Gallup data.

“The U.S. Constitution has proved binding in practice partly because citizens are willing to defend it by reacting against proposed violations. Anticipating that reaction, political leaders rarely attempt violations.”
(Weingast, 1997, p. 254)

“Democracy is “self-enforcing” when politicians anticipate that, were they to behave undemocratically, their own supporters would punish them by voting for a competitor in large enough numbers to bring about their defeat.”
(Graham and Svolik, 2020, 394)

4.1 Introduction

Politicians that cannot win through free and fair elections self-evidently have an incentive to rig the electoral system for political gain. While democratic norms among politicians could in principle self-constrain them from doing so, this mechanism, even if such norms exist, becomes more and more brittle once the stake of elections increases. Political scientists have therefore long understood that for democracy to be truly self-enforcing it is necessary that politicians that display anti-democratic behavior lose so much popular support that their further political career is effectively doomed to fail. Anticipating this reaction, political leaders would rarely attempt to violate key democratic norms in the first place (Almond and Verba, 1963; Diamond, 1999; Maravall and Przeworski, 2003; Svolik, 2020; Weingast, 1997).

Given the importance of the public as the ultimate check against undemocratic politicians it is very disconcerting that a number of recent studies find that overt undemocratic behavior by politicians is only to a very limited extent punished by contemporary American voters. In their seminal paper Graham and Svolik (2020) find survey experimental evidence that suggests that a mere 3.5% of the U.S. electorate is willing to punish clear undemocratic behavior by politicians if this means that they are forced to vote for a politician that is further away from their own policy preferences. McCoy, Littvay and Simonovits (2022) and Albertus and Grossman

(2021) meanwhile find survey experimental evidence that suggests that Americans are remarkably hypocritical when it comes to democratic backsliding, supporting the same anti-democratic state executive behavior when their own party is in power, while condemning it if the opposing party is in power.²

This important existing evidence notwithstanding significant uncertainty remains as to whether the American public does indeed not strongly react to clear undemocratic behavior by politicians. First, several studies employing very similar survey experimental methods conclude that American voters do in fact strongly punish undemocratic behavior by politicians (e.g., Carey et al. (2020) and Touchton, Klostad and Uscinski (2020)). This suggests that relatively minor changes in framing and survey experimental set-up may make large differences for the results in this particular research area. Second, all existing evidence is survey experimental in nature. It remains unclear therefore whether voters would not more strongly react to *real-life* democratic transgressions that would directly affect their own life.³ Third, all existing studies examine important but nevertheless not truly fundamental and decisive forms of democratic backsliding (e.g., whether a candidate supported a proposal to reduce the number of polling places in areas that largely support the opposing party). It is unclear therefore whether American voters will not react more strongly to more extreme forms of democratic backsliding that would truly change the rules of the game (e.g., a president refusing to leave office after losing an election).

To address these issues I analyze the results of a novel natural experiment generated by the fact that Donald Trump's incitement of the insurrection at the U.S. Capitol,

²Another rapidly growing literature builds upon this survey experimental evidence and seeks to explain *why* voters may not always punish overt undemocratic behavior by politicians (e.g., Chiopris, Nalepa and Vanberg (2021), Grillo and Prato (Forthcoming), Grossman et al. (2022), and Svolik (2020)).

³Graham and Svolik (2020) are an exception to this. They also analyze the electoral effect of Republican Greg Gianforte hitting a journalist in the face. I differ from Graham and Svolik (2020) by focusing on a more extreme form of undemocratic behavior that had a more structural effect on American democracy.

on January 6, 2021, unexpectedly occurred while Gallup was conducting a nationally representative public opinion survey among 1,023 Americans. Given that Gallup recruits respondents using random digit dialing the probability of any particular individual to be interviewed before or after the January 6 insurrection is plausibly exogenous to any pre-treatment characteristics that may otherwise affect political party preference. Furthermore, given that there were no other events on January 6, 2021 that could plausibly have caused significant changes in political preferences I can recover the average treatment effect of Donald Trump's incitement of the January 6 insurrection (and the Republican Party's support for his acquittal thereafter) by simply comparing support for the Republican Party among respondents that happened to be interviewed just before, and just after, January 6, 2021.

Using this natural experiment I find that support for the Republican Party went down with approximately 11% in the 1.5 weeks after the January 6 insurrection. I interpret this effect as relatively modest in this context. A 11% reduction in Republican Party support means that 77.9% of Republicans did *not* move away from the Party, *even though its leader incited a violent insurrection to overturn the results of a free and fair election*. Note here that potential sources of bias (e.g., social desirability bias, undersampling of die-hard Trump supporters), if at all present, are likely to bias *in favor* of finding stronger negative effects. Furthermore, I find that even this modest effect diminished relatively quickly. For example, comparing Republican Party support in the days before the January 6 insurrection with Republican Party support during a follow-up survey that Gallup fielded from February 7 to February 15, 2021 reduces the effect to only 3.7%. I find similar results when analyzing the effect on opinions towards Donald Trump in particular. More specifically, I find that Donald Trump's favorability rating experienced a modest drop from 42.5% to 37.9% in the weeks following the

January 6 insurrection, and that even this modest drop disappeared entirely within less than five months.

Taken together the natural experiment suggests that for the far majority of Republican Party supporters even a Republican president inciting an insurrection to overturn the results of a free and fair election—arguably a most-likely case—is insufficient to *say* in an anonymous phone survey that they no longer support the Party (let alone take costly action to protect democracy). This uncomfortable fact may explain why the Republican Party, while initially relatively condemning of Donald Trump, has not generally tried to distance itself from Trump after the January 6 insurrection. More generally, it may explain why many less severe, but nonetheless very serious, forms of democratic norm transgressions are relatively common in American politics (e.g., gerrymandering). For the stability of American democracy more generally the results suggest that a significant fraction of the American electorate is unlikely to chose democracy over partisanship when it really comes down to it.

The results have important broader implications for the literature on democratic backsliding in general, and the literature on the role of voters in enabling/avoiding democratic backsliding in particular.⁴ While coup d'états were the primary channel of autocratization in the past, in recent years democratic backsliding has typically occurred by elected state executives that slowly undermine the checks and balances meant to constrain their power *while reasonably free and fair elections remain at least initially in place* (e.g., Brazil, Poland, India) (Bermeo, 2016; Haggard and Kaufman, 2021). This implies that voters could in principle stop the process of democratic backsliding if they would be willing to vote for an opposing political candidate in

⁴See Bermeo (2016), Cleary and Öztürk (2022), Diamond (2021), Ding and Slater (2021), Grillo and Prato (Forthcoming), Grossman et al. (2022), Haggard and Kaufman (2021), Laebens and Lührmann (2021), Lührmann (2021), and Waldner and Lust (2018) on democratic backsliding in general. See Acemoglu, Robinson and Torvik (2013), Ahlquist et al. (2018), Cho and Hwang (2021), Şaşmaz, Yagci and Ziblatt (2022), Fossati, Muhtadi and Warburton (2022), Magaloni (2006), Saikkonen and Christensen (2022), Svolik (2019, 2020), and Wuttke, Gavras and Schoen (2022) on the role of voters in tolerating democratic backsliding in particular.

sufficiently large numbers to vote the state executive that is engaging in democratic backsliding out of office (Graham and Svolik, 2020; Svolik, 2020). The results of this paper suggests that at least in the United States—one of the oldest and richest democracies in the world—many voters are unlikely to play this role in practice.

I am not the first to study the effect of the January 6 insurrection on American public opinion. Agosta and Lightbourn (2021) show that the January 6 insurrection led to a net gain for the Democratic Party of 61,000 registered voters, or approximately 0.1% of all registered voters, in the 25 states that publish voter registration data. Eady, Hjorth and Dinesen (2021) show that expressions of identification with the Republican Party on Twitter dropped with 7% in the weeks after January 6, 2021. Keeter (2021) meanwhile uses data from the American Trends Panel and finds that 25% of those who approved of Donald Trump’s job performance in July 27-August 2, 2020 changed their answer to disapproval in January 8-12, 2021.

My contributions to the study of the January 6 insurrection is two-fold. First, I provide a more credible estimate of the causal effect of the January 6 insurrection on American public opinion. Keeter’s (2021) data forces us to assign the entire change in Donald Trump’s approval rating between July/August 2020 and January 2021 to the January 6 insurrection, while many other events occurred during this time period that could also have affected Trump’s presidential approval rating (e.g., the second wave of the Covid-19 virus, the 2020 presidential election, etc.). Agosta and Lightbourn (2021) are likely to underestimate the effect of the January 6 insurrection because most voters in the U.S. do not change their party registration several years before the next election (even when their actual party preference has in fact changed). The changes in Twitter profiles documented by Eady, Hjorth and Dinesen (2021) are meanwhile hard to interpret as population-level changes in political attitudes because Twitter users are not a representative sample of the American population, and because changes in Twitter

profiles do not necessarily correspond to actual changes in underlying voter preferences. Second, and in contrast to Agosta and Lightbourn (2021), Eady, Hjorth and Dinesen (2021), and Keeter (2021), I show that the effect of the January 6 insurrection on political party preferences, while quite modest to begin with, diminished quickly.

4.2 The January 6 insurrection

On November 3, 2020 Joseph Biden (Democrat) won the 2020 U.S. presidential election of sitting U.S. president Donald Trump (Republican) with a difference of a little more than 7 million popular votes (and 74 Electoral College votes). Despite this large difference in the number of votes, and the absence of any convincing evidence of widespread voting fraud,⁵ Donald Trump claimed that the election was fraudulent and that he was the true winner of the election.

After several months of constant misinformation regarding the election, and a wide range of highly publicized court cases (which were all essentially dismissed for a lack of evidence), Donald Trump held a speech at the “Stop the Steal” rally on January 6, 2021. In the speech Trump said, among other things, that the election was stolen, that he and his supporter will never concede, that they will fight like hell, and that they are going to walk down to the Capitol because “you’ll never take your country back with weakness.”⁶

Directly after this speech a large mob of Trump supporters stormed the U.S. Capitol building, leading to the death of 5 people, the injury of at least 138 police officers, physical property damages in excess of 30 million dollars, the abrupt halt of an ongoing congressional debate, and the immediate evacuation of all members of Congress.

⁵See Eggers, Haritz and Grimmer (2021) on the absence of convincing evidence for widespread voting fraud in the 2020 U.S. presidential election.

⁶See Appendix 5.8.2 for direct quotes from the speech.

Luckily for American democracy, the insurrection was ultimately unsuccessful in overturning the results of the 2020 presidential election. From January 13 to February 13, 2021 a trial was held in the U.S. Congress to impeach president Trump. In this trial only 10 of the 207 Republican members of the U.S. House of Representatives and only 7 of the 50 Republican members of the U.S. Senate voted to impeach president Trump, leading to his acquittal (all Democrats in both chambers voted in favor of impeachment).

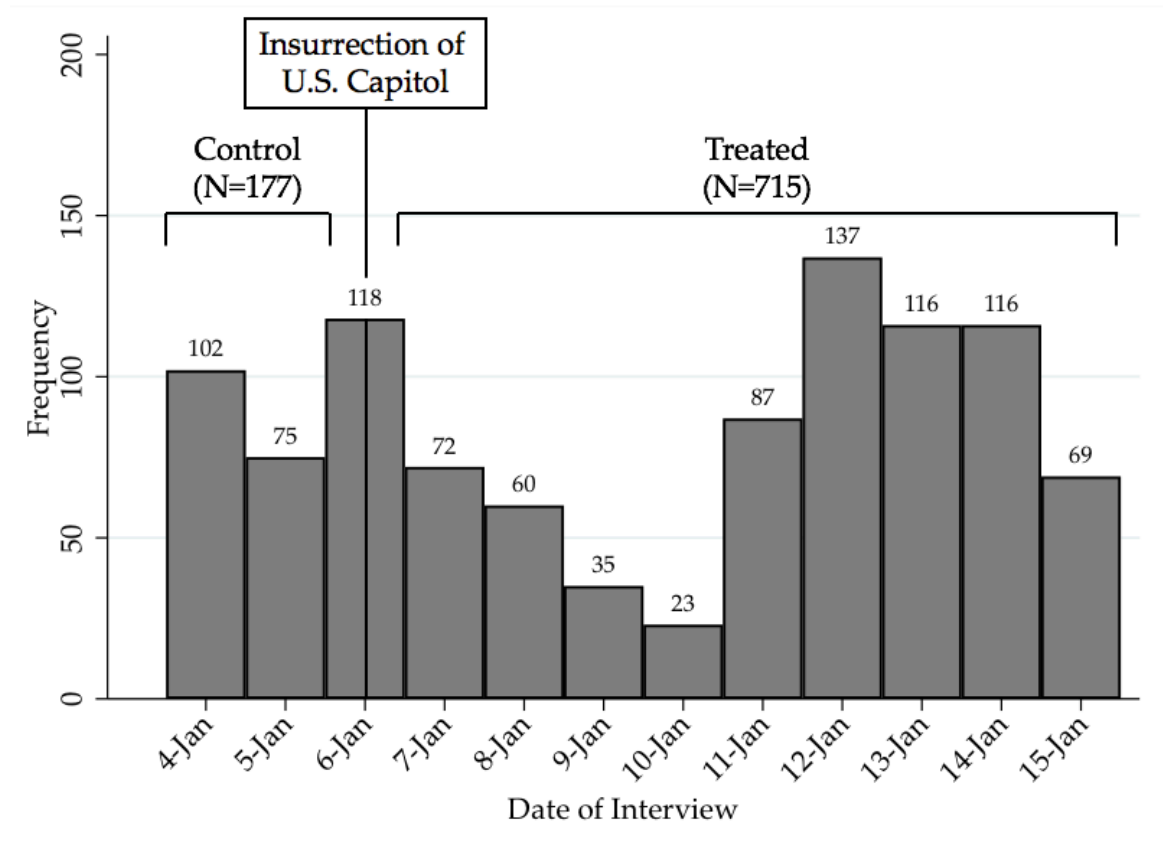
I regard this event as a clear case of democratic backsliding that should be recognized and sanctioned as such *if* the American public is to function as an effective check against overt anti-democratic behavior by politicians. I regard the acquittal of President Trump by other Republican members of Congress as a part of the treatment—i.e., a clear and overt undemocratic act by the Republican Party at large. I therefore examine effects on the Republican Party as a whole, not only on support for president Donald Trump himself. As shown in section 4.7 results are very similar when focusing on public opinion towards Donald Trump in particular.

4.3 Data

To study how much support the Republican Party lost due to Donald Trump’s incitement of the January 6 insurrection I exploit that the insurrection unexpectedly occurred while Gallup was conducting its January 2021 “Mood of the Nation” survey.

As the treatment variable I use a dummy that takes the value 1 if a respondent was interviewed after January 6, 2021, and 0 if a respondent was interviewed before this date. Figure 4.1 shows the number of respondents that were interviewed by Gallup before, on, and after January 6, 2021. As can be seen 177 people were interviewed

Fig. 4.1 Histogram of dates of interviews relative to treatment.



Note: Data refers to the year 2021. Data comes from Gallup’s January “Mood of the Nation” survey. Respondents with missing data on the party identification variable are omitted (N=13).

before January 6, 2021 and 715 people were interviewed after January 6, 2021.⁷ In the main results I drop all 118 respondents that were interviewed on the day of the January 6 insurrection itself. All results remain substantively unchanged when assigning all respondents that were interviewed on January 6, 2021 to either control or treatment.

As the dependent variable I use the variable: “In politics, as of today, do you consider yourself: a Republican, a Democrat, or an Independent?”. If a respondent

⁷The number of observations in the control group (177) far exceeds the $N \geq 30$ threshold that is sufficient for the central limit theorem to hold true (which is necessary for valid statistical significance tests). Figure 5.1 furthermore shows that the control group is not unbalanced on observables by random chance. Taken together this provides me with confidence that the results cannot be explained by small sample bias.

selects “Independent” the survey asks: “As of today, do you lean more to the Democratic Party, more to the Republican Party, or neither?”. I analyze this variable as a nominal variable with 5 categories (Republican, lean Republican, Independent, lean Democratic, and Democrat). In addition, I analyze results using a dummy that takes the value 1 if a respondent considers, or leans towards considering, him/herself a Republican, and 0 otherwise.

4.4 Identification strategy

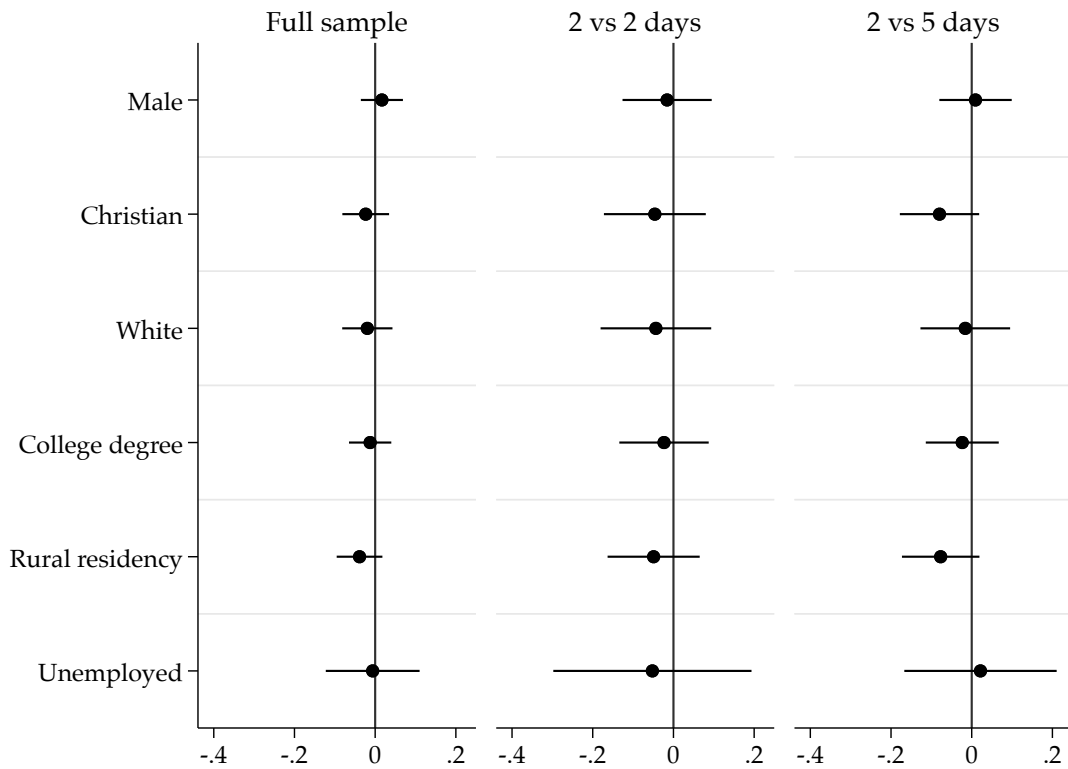
For identification I employ an “unexpected event during survey” design. This design relies on a simple comparison between the average support for the Republican Party among respondents surveyed just before, and just after, January 6, 2021. Causal identification relies on two assumptions. First, *temporal ignorability*, meaning that whether any particular individual is interviewed before or after January 6, 2021 should be orthogonal to any other individual-level characteristic that may also affect party identification. Second, *excludability*, meaning that whether any individual respondent is interviewed before or after January 6, 2021 should affect party identification only through the insurrection event, not through any other channel (Muñoz, Falcó-Gimeno and Hernández, 2020).

4.4.1 Temporal ignorability

Temporal ignorability is likely to hold for four reasons.

First, Gallup interviews over the phone and samples respondents using random digit dialing. This ensures that any individual, regardless of where he/she lives, and regardless of his/her own background characteristics, has an equal probability to be contacted for an interview on any particular day. In line with this I find that the control and treatment groups are balanced on common pre-treatment determinants of Republican Party support (see Figure 5.1).

Fig. 4.2 Balance on observables.



Note: Each dot represents an individual OLS regression. Dependent variable is treatment status. Point estimates and 95% confidence intervals are reported. Confidence intervals are robust against heteroscedasticity.

Second, there is no evidence of endogenous reachability bias (i.e., supporters of the Republican Party being more/less difficult to reach for an interview and therefore being more/less likely to be included in the treatment group). First, the number of tries before an interview was successfully completed is uncorrelated with supporting the Republican Party (OLS coefficient: -0.022; P-value: 0.199).⁸ Second, estimates of comparable magnitude are obtained when dropping all respondents that were not successfully interviewed on the first try (OLS coefficient: -0.141; P-value: 0.006).

⁸This result is generated by regressing a dummy that takes the value 1 if a respondent considers, or leans towards considering, him/herself a Republican, and 0 otherwise, on the number of tries before an interview was successfully completed.

Third, there is no indication of endogenous social desirability bias (i.e., supporters of the Republican Party being disproportionately more likely to lie about their true party alliance after the January 6 insurrection). If it were true that Republicans were disproportionately more likely to conceal their true political party preference as a result of the January 6 insurrection one would reasonably expect (1) non-response on the party identification question to go up after January 6, and (2) support for Independents, rather than Democrats, to go up after January 6, 2021 (i.e., assuming that selecting Independents would still be socially “acceptable” while preferable over Democrats for “Republicans in disguise”). This is not what the data suggests. Instead, there is no economically or statistically significant difference in non-response on the party identification question before and after January 6, 2021 (OLS coefficient: 0.004; P-value: 0.661), and support tends to predominantly shift to the Democratic Party, rather than Independent, after January 6, 2021 (see Table 4.2 below). Crucially, if social desirability bias is nonetheless present in the survey this would mean that the “true” voter reaction to the January 6 insurrection has been even weaker than what I report (i.e., assuming that social desirability bias will induce more Republicans than Democrats/Independents to disguise their true political party preference as a result of the January 6 insurrection).

Last, there is no evidence of endogenous survey non-response (i.e., supporters of the Republican Party being altogether less likely to participate in the Gallup survey after the January 6 insurrection). First, notice that passing the balance on observables test in Figure 5.1 in the presence of endogenous survey non-response requires Republicans that disproportionately choose to opt out of the survey after the January 6 insurrection to be similar to the voter profile of Democrats on average. Second, a placebo test using a dummy that takes the value 1 if a respondent is in favor of further restrictions on abortion, and 0 otherwise, suggests that the treatment

group is, if anything, overrepresenting a priori Republicans (OLS coefficient: 0.068; P-value: 0.190). Importantly, if endogenous survey non-response is nonetheless present in the survey this would mean that the “true” voter reaction to the January 6 insurrection has been even weaker than what I report (i.e., assuming that more Republicans than Democrats/Independents would refuse to be interviewed as a result of the January 6 insurrection).

4.4.2 Excludability

Excludability is likely to hold for two reasons.

First, a content analysis of the New York Times and the USA Today suggests that there were no other events that occurred on January 6, 2021 that could plausibly have had a significant effect on political party identification (see Appendix 5.8.5).

Second, while I naturally cannot exclude the possibility that the January 6 insurrection triggered some other, perhaps currently unknown, voter reaction that has also affected Republican Party support besides democratic backsliding concerns, the data does suggest that this is unlikely to be the case. This is because respondents in the treatment group are not more likely to express dissatisfaction towards other issues that could in theory be linked to the January 6 insurrection, such as crime and violence (OLS coefficient: 0.000; P-value: 0.994), moral and ethical climate (OLS coefficient: 0.014; P-value: 0.683), respect for others (OLS coefficient: -0.003; P-value: 0.876), and race relations (OLS coefficient: 0.021; P-value: 0.686).⁹¹⁰

⁹Regrettably, Gallup did not ask respondents about their satisfaction with the state of American democracy. This prohibits me from assessing whether respondents interpreted the January 6 insurrection as an attack on democracy.

¹⁰Note that if any collateral events nonetheless did take place my identification strategy would still identify the causal effect of the events of January 6, 2021, but this would represent a bundled treatment of democratic backsliding *plus* other closely related treatments. Like with other potential sources of bias, it is likely that many potential bundled treatments would bias in favor of finding stronger negative effects (e.g., when voters punish the Republican Party not for reasons related to democracy but because they perceive the January 6 insurrection as a reprehensible desacralization of a national monument).

4.5 Generalizability

Gallup samples by randomly selecting respondents from a list of all telephone numbers that have recently been used within the continental United States (see Gallup (2022) for more information). As discussed in the previous section this random digit dialing sampling strategy allows me to causally identify the effect of the January 6 insurrection. Being a phone survey does come at a potential cost, however, as phone surveys generally suffer from more survey non-response than in-person surveys.

To assess representativeness I benchmark Gallup's January 2021 "Mood of the Nation" survey against data from the 2020 American National Election Study (ANES), which was conducted from August 18, 2020 until January 4, 2021 (N=8,280). I use the ANES for three reasons. First, the ANES is one of the most rigorously sampled in-person surveys in American politics. Second, data from the Census and the Current Population Survey, which are even more rigorous in-person surveys, were, at the time of writing, only available for the year 2019. Last, the Census and the Current Population Survey do not collect data on religious affiliation, while religious affiliation is an important determinant of American political behavior, and is therefore an important variable to assess the survey's representativeness on.

As can be seen in Table 4.1 the January 2021 Gallup survey has approximately the same share of Christian, White, and rural individuals as the ANES indicated existed in the U.S. electorate at the end of 2020. Importantly, the Gallup sample does contain on average slightly more males, college graduates, and unemployed people than ANES indicated existed in the U.S. electorate at the end of 2020. To correct for this I devise inverse probability weights (see last two columns in Table 4.1).¹¹ As shown below the

¹¹I create these weights using Deville and Särndal's (1992) distance function. In terms of calibration I employ the principle of minimizing the distance between the smallest and the largest weight. This leads to a weight range of 0.65 to 1.35 in this case.

Table 4.1 Congruence with American National Election Survey (ANES) on observables.

| | Gallup | | ANES | | Weighted Gallup | |
|-----------------|--------|-----------|-------|-----------|-----------------|-----------|
| | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. |
| Male | 0.520 | 0.500 | 0.479 | 0.500 | 0.479 | 0.500 |
| Christian | 0.719 | 0.450 | 0.722 | 0.448 | 0.722 | 0.448 |
| White | 0.790 | 0.407 | 0.792 | 0.405 | 0.795 | 0.404 |
| College degree | 0.481 | 0.500 | 0.355 | 0.479 | 0.356 | 0.479 |
| Rural residency | 0.349 | 0.477 | 0.398 | 0.490 | 0.396 | 0.489 |
| Unemployed | 0.055 | 0.228 | 0.049 | 0.217 | 0.050 | 0.217 |

Notes: ANES includes both the pre- and post-election data. ANES is weighted using the probability weights provided in the data itself. Inverse probability weights for the Gallup survey are generated using the “sreweight” Stata package.

results remain substantively unchanged when estimating the effect in this weighted sample.¹²

4.6 Results

Table 4.2 reports the main results. As can be seen the percentage of respondents that indicated to identify as Republican reduced from 31.6% on January 4 and 5, 2021, to 24.6% on January 7 to 15, 2021; a reduction of 7%. The percentage that indicated to lean Republican meanwhile reduced from 18.1% to 14.3%; a reduction of 3.8%. Taken together this suggests that the total (likely) support group of the Republican Party went down with 10.8% in the 1.5 weeks after the January 6 insurrection.¹³ This reduction of 10.8% is the equivalent of a 21.8% decline in total support relative to the Republican Party’ pre-insurrection support level. The last three columns of Table 4.2

¹²With regard to the direction of potential bias generated by survey non-response I would again suggest that my estimates are likely upperbound. This is because die-hard Donald Trump supporters—which are presumably significantly *less* likely to change their party alliance as a result of the January 6 insurrection—will, if anything, be likely to be *undersampled* in Gallup’s January 2021 “Mood of the Nation” survey (e.g., like all other public opinion surveys Gallup polls have tended to underestimate Donald Trump’s vote share in the 2016 and 2020 presidential elections (Clinton et al., 2021)).

¹³As can also be seen in Table 4.2 8.4% of this 10.8% shifted to the Democratic Party, the rest went to Independent.

Table 4.2 Party identification before and after the January 6 insurrection.

| | Unweighted | | | Weighted | | |
|--------------------|------------|----------|----------|----------|----------|----------|
| | Jan 4–5 | Jan 7–15 | Δ | Jan 4–5 | Jan 7–15 | Δ |
| Republican | 31.6% | 24.6% | -7.0% | 33.9% | 26.4% | -7.5% |
| Leaning Republican | 18.1% | 14.3% | -3.8% | 18.1% | 14.5% | -3.6% |
| Independent | 7.3% | 9.8% | +2.5% | 6.4% | 9.3% | +2.9% |
| Leaning Democrat | 17.5% | 21.1% | +3.6% | 17.8% | 20.5% | +2.7% |
| Democrat | 25.4% | 30.2% | +4.8% | 23.7% | 29.4% | +5.7% |

Notes: Data comes from Gallup’s January 2021 “Mood of the Nation” survey. Weights are generated using American National Election Study data on the percentage of male, Christian, White, college educated, rural, and unemployed individuals that existed in the U.S. electorate at the end of 2020.

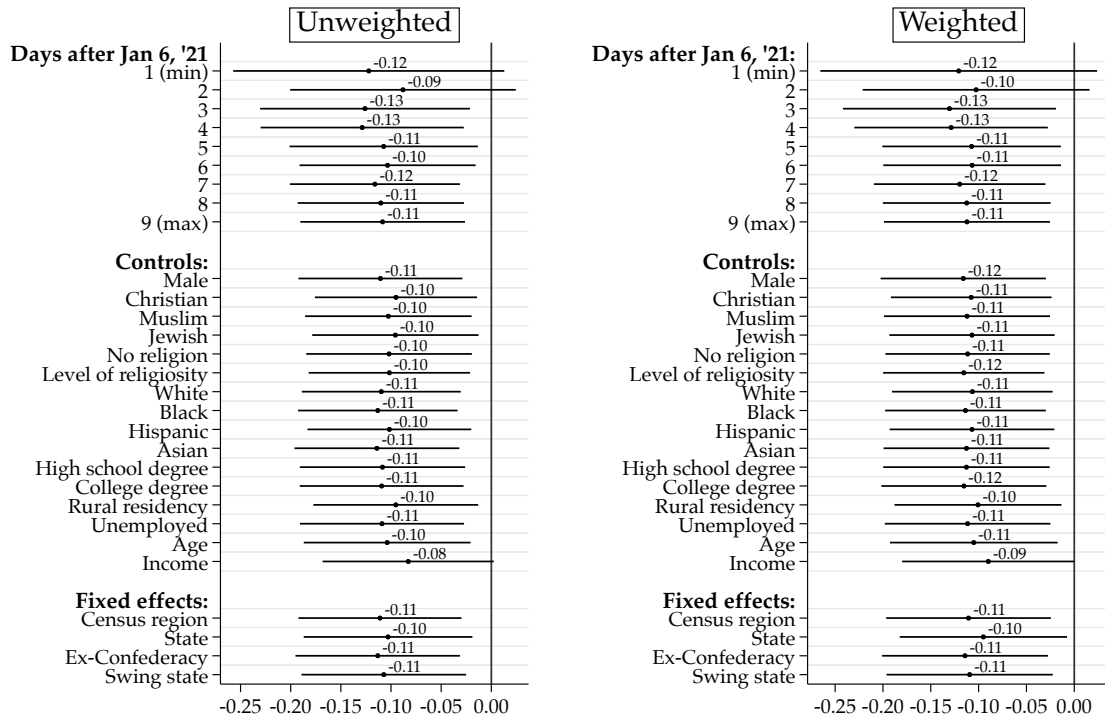
show that these results remain substantively unchanged when using the probability weights devised in the previous section.

In Figure 4.3 I examine the robustness of these results to (1) limiting the sample to only include respondents interviewed directly before and directly after January 6, 2021, and (2) adding covariates. To do so I use OLS to regress a dummy that takes the value 1 if a respondent self-identifies, or leans towards self-identifying, as a Republican, and 0 otherwise, on a dummy that takes the value 1 if a respondent was surveyed after January 6, 2021, and 0 if he/she was surveyed before this date. I use a linear probability model for ease of interpretation. The results remain substantively unchanged when using a probit model (Appendix 4.9.3).

As shown in Figure 4.3 the effect on Republican Party support remains approximately 11% when controlling for gender, religion, race/ethnicity, education, rural/urban residency, unemployment, age, and income, and when only comparing respondents that were surveyed directly before and directly after January 6, 2021. Figure 4.3 also reveals that the treatment effect is typically statistically significant on the 95% confidence level.

Is an approximately 11% decline in overall support for the Republican Party a large or small effect? Answering this question remains to some extent subjective, as

Fig. 4.3 Robustness checks.



Note: Point estimates and 95% confidence intervals of (separate) OLS regressions of a dummy that takes the value 1 if a respondent considers, or leans towards considering, him/herself a Republican, and 0 otherwise, on a dummy that takes the value 1 if a respondent is interviewed after January 6, 2021, and 0 if a respondents is interviewed before January 6, 2021. Confidence intervals are robust against heteroscedasticity. Weights are generated using American National Election Study data on the percentage of male, Christian, White, college educated, rural, and unemployed individuals that existed in the U.S. electorate at the end of 2020.

the quantity of what a large effect is, and what effect is large enough to incentivize politicians from refraining from undemocratic behavior (if such an inflection point in fact exists), cannot be clearly defined. While acknowledging this fact there are good reasons to consider a 11% decline as relatively modest in this context. A reduction of 11% suggests that 77.9% of the Republican Party' pre-insurrection support group remained loyal to the Party, *even through its leader incited a violent insurrection to overturn the results of a free and fair election*. Furthermore, as discussed above, all estimates in Table 4.2 and Figure 4.3 are arguably upperbound—i.e., the major potential sources of bias, if anything, appear to bias in favor of finding *stronger*

negative effects in this context. For example, if, even given the design checks discussed in Section 4.4, it is the case that some Republicans in the direct aftermath of the January 6 insurrection felt inclined to lie about their support for the Republican Party, or refused to be surveyed altogether, the “true” effect of Donald Trump’s incitement of the January 6 insurrection on Republican Party support was even weaker than the estimates reported in Table 4.2 and Figure 4.3.

4.7 Long run effect

My identification strategy is particularly suitable to examine the immediate (short run) effect of the January 6 insurrection on political party identification.¹⁴ Examining how the treatment effect has developed over longer periods of time is significantly more challenging. This is because many other things happened in the months after January 6, 2021 that could also have affected political party preferences—creating a bundled treatment problem.

To illustrate: if identification with the Republican Party today is the same as what it was in the days before the January 6 insurrection this could be because the negative effect of the January 6 insurrection has disappeared entirely, *or* it could be that this rebound in Republican Party support was due to other factors (e.g., the chaotic military withdrawal from Afghanistan under president Biden), so that support for the Republican Party would have been even higher today, were it not for the January 6 insurrection.

I am not aware of an available natural experiment that could seamlessly separate these two scenario’s by design. To nonetheless provide suggestive evidence on the long run effect of the January 6 insurrection I employ two complementary analyses. First, I examine the effect of the January 6 insurrection on political party identification after

¹⁴The immediate (short run) effect is also the quantity that is identified by existing survey experiments on the effect of overt undemocratic behavior by politicians on voter preferences.

only one month, when few other events that may cause a bundled treatment problem had the time to take place. Second, I analyze the effect of the January 6 insurrection on Donald Trump's favorability rating, which is less likely to be affected by other electorally important events that occurred after January 6, 2021.¹⁵

Table 4.3 compares party identification on January 4 and 5, 2021 with political party identification in a follow-up survey that Gallup fielded from February 7 to February 15, 2021 (N=1,007). As can be seen the percentage of respondents that indicated to support or lean towards supporting the Republican Party was only 3.7% lower in February 7-15, 2021 as compared to January 4 and 5, 2021. While it is possible that this reduction in effect size from 10.8% to 3.7% within one month was (partly) due to other events that occurred between January 15 and February 7, 2021, this does appear unlikely.¹⁶ During this 23 day period the news was still very much dominated by the January 6 insurrection, and while it is true that the current U.S. president, Joseph Biden, is relatively unpopular, which could in general explain a Republican Party' re-emergence, this unpopularity only arose six months later, at the beginning of August 2021 (Rakich and Wiederkehr, 2021).¹⁷

Rather than unrelated events, it appears more likely that the almost two-thirds reduction in effect size within one month was due to a significant shift in position-taking and messaging by members of the Republican Party itself. While many Republican Party politicians were moderately condemning of Donald Trump's behavior in the 1.5 weeks after the January 6 insurrection, this radically changed in the second half of January 2021. Since then there has been a well-organized campaign by the Republican

¹⁵Note that it is not possible to assess the effect on Donald Trump's presidential approval rating, as this data is not available after January 20, 2021 (when Trump left office).

¹⁶Note that there is no sign that the treatment effect already diminished in the first 1.5 weeks after the January 6 insurrection—i.e., the number of days a respondent is interviewed after January 6, 2021 is uncorrelated with identifying with the Republican Party in the January 7-15 sample analyzed in Table 4.2 and Figure 4.3 (OLS coefficient: 0.002; P-value: 0.803).

¹⁷This drop in Biden's presidential approval rating in the beginning of August 2021 coincided with the chaotic military withdrawal from Afghanistan and the spread of the Delta variant of the Covid-19 virus, and was thus plausibly unrelated to the January 6 insurrection.

Table 4.3 Effect of the January 6 insurrection on party identification after one month.

| | Unweighted | | | Weighted | | |
|--------------------|------------|----------|----------|----------|----------|----------|
| | Jan 4–5 | Feb 7–15 | Δ | Jan 4–5 | Feb 7–15 | Δ |
| Republican | 31.6% | 29.6% | -2.0% | 33.9% | 30.7% | -3.2% |
| Leaning Republican | 18.1% | 16.4% | -1.7% | 18.1% | 16.4% | -1.7% |
| Independent | 7.3% | 6.9% | -0.4% | 6.4% | 6.5% | +0.1% |
| Leaning Democrat | 17.5% | 16.3% | -1.2% | 17.8% | 16.0% | -1.8% |
| Democrat | 25.4% | 30.9% | +5.5% | 23.7% | 30.4% | +6.7% |

Notes: Data comes from Gallup’s January 2021 “Mood of the Nation” survey and Gallup’s February 2021 “World Affairs” survey. Weights are generated using American National Election Study data on the percentage of male, Christian, White, college educated, rural, and unemployed individuals in the U.S. electorate at the end of 2020.

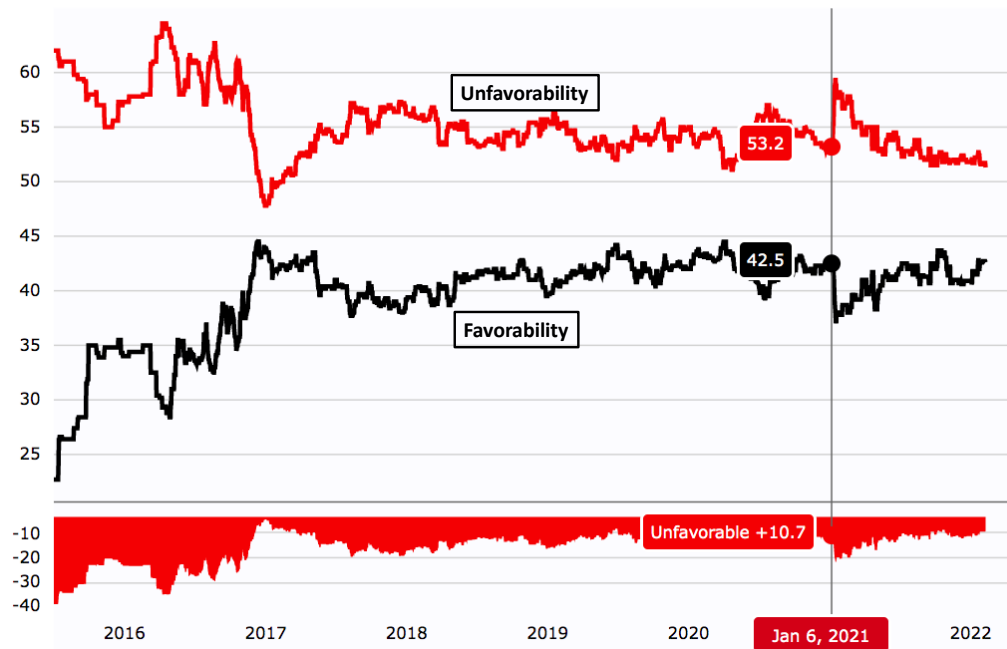
Party to downplay the severity of the January 6 insurrection, trivialize Donald Trump’s role in it, and silence all Republican members of Congress that contradict this framing (New York Times, 2022).¹⁸ Given what we know about the influence of elite messaging on co-partisan political preferences it appears likely that this at least partly explains the observed rebound in Republican Party support within the first month of the January 6 insurrection (e.g., Agadjanian (2021), Barber and Pope (2019), and Broockman and Butler (2017)).

Figure 4.4 plots the percentage of people that perceive Donald Trump favorably (bottom line) and the percentage of people that perceive Donald Trump unfavorably (top line) from the time when this data first came available (July 2, 2015) until the time of writing (February 9, 2022).¹⁹ As can be seen Donald Trump’s favorability (unfavorability) decreased (increased) from 42.5% (53.2%) on the eve of the January 6 insurrection to 37.9% (59.5%) on January 16, 2021. Given the actual severity of the January 6 insurrection this decrease (increase) in favorability (unfavorability) of 4.6% (6.3%) is again arguably quite modest. More importantly, however, Donald Trump’s

¹⁸This has gone so far that the Republican National Committee has censured two of its own representatives—Liz Cheney and Adam Kinzinger—for participating in the National Commission to Investigate the January 6 Attack on the United States Capitol Complex, and has instead declared the January 6 insurrection “legitimate political discourse” (New York Times, 2022).

¹⁹This data is collected by Real Clear Politics, and is generated by pooling survey data from YouGov, Politico, Harris, NBC News, Rasmussen, CNBC, and the Wall Street Journal.

Fig. 4.4 Donald Trump (un)favorability rating from July 2, 2015 until February 9, 2022.



Note: The top line is the percentage of respondents that view Donald Trump unfavorably. The bottom line is the percentage of respondents that view Donald Trump favorably. The bargraphs in the bottom plot the difference between these two variables. Data comes from Real Clear Politics, which has generated this data by pooling survey data from YouGov, Politico, Harris, NBC News, Rasmussen, CNBC, and the Wall Street Journal.

favorability (unfavorability) improved from January 16, 2021 onwards, leading his favorability rating to be essentially back to pre-insurrection levels on July 8, 2021 (42.5% favorability; 52.3% unfavorability). Since then Trump's favorability rating has been relatively stable.

While, as discussed above, I cannot be 100 percent certain what Trump's current favorability rating would have been in the absence of the January 6 insurrection, Figure 4.4 does, at the minimum, show that any negative effect of the January 6 insurrection was not sufficient to permanently reduce Trump's favorability rating below what it was prior to January 6, 2021. This in itself is quite remarkable considering the fact that it was Trump himself that incited his supporters to storm the center of American democracy in an attempt to overturn the results of a free and fair election.

Taken together the results from Table 4.3 and Figure 4.4 suggest that even the relatively modest immediate (short run) effect of the January 6 insurrection found in Table 4.2 and Figure 4.3 was most probably not long-lasting.

4.8 Conclusion

I natural experimentally examine whether the American public tends to retract its support from politicians that clearly violate key democratic norms. To do so I exploit that Donald Trump's incitement of the January 6 insurrection (and the Republican Party's support for his acquittal thereafter) unexpectedly occurred while Gallup was conducting a nationally representative public opinion survey using random digit dialing. I find that the far majority of current supporters of the Republican Party is so weakly committed to democracy that even a violent insurrection to overturn the results of a free and fair election is insufficient for them to *say* in an anonymous phone survey, directly following the insurrection, that they no longer support the Republican Party. Similar results are found when analyzing the effect on opinions towards Donald Trump in particular.

My novel natural experimental design allows me to state with relative certainty that the January 6 insurrection did not cause a major and long-lasting public opinion penalty for the Republican Party in general and/or Donald Trump in particular. This is a crucial finding because one would expect to find such a reaction after an insurrection aimed at overturning the results of a free and fair election *if* the American public is indeed to function as an effective constraint on overt anti-democratic behavior by politicians.

Focusing on one major case where a plausible natural experiment is available also comes with limitations, however. Future research is necessary to establish how strongly the American electorate reacts to other types of undemocratic behavior by politicians

(e.g., vote suppression) and to other politicians acting undemocratically (e.g., politicians from the Democratic rather than the Republican Party).

Looking further beyond the results highlight the urgent need for more research on how to increase the likelihood that Americans voters will strongly oppose politicians that display clearly undemocratic behavior. Future research could examine ways to increase genuine commitment to democracy among the general public and/or could study how to reduce political polarization, so that the “cost” of defecting from one’s own preferred political party/candidate, in case it acts undemocratically, is lowered (Graham and Svolik, 2020; Svolik, 2020).

4.9 Appendix

4.9.1 Quotes from Trump's speech on January 6, 2021

My identification strategy relies on the claim that Trump's speech on the January 6 "Stop the Steal" rally incited the insurrection at the U.S. Capitol, and should therefore be interpreted by the American public as a clear case of democratic backsliding.

Below, I provide a number of direct quotes from Donald Trump's speech to substantiate this assertion:

"All of us here today do not want to see our election victory stolen by emboldened radical-left Democrats, which is what they're doing. And stolen by the fake news media. That's what they've done and what they're doing. We will never give up, we will never concede. It doesn't happen. You don't concede when there's theft involved."

"Our country has had enough. We will not take it anymore and that's what this is all about. And to use a favorite term that all of you people really came up with: We will Stop the Steal."

"Because if Mike Pence does the right thing, we win the election. [...] All Vice President Pence has to do is send it back to the states to recertify and we become president and you are the happiest people."

"[...] we're going to walk down to the Capitol, and we're going to cheer on our brave senators and congressmen and women, and we're probably not going to be cheering so much for some of them."

"Because you'll never take back our country with weakness. You have to show strength and you have to be strong. "

"We fight like hell. And if you don't fight like hell, you're not going to have a country anymore."

"So let's walk down Pennsylvania Avenue."

Not only I but also virtually all reputable news outlets characterized the words of Trump as inciting the insurrection. The New York Times, for example, opened on January 7, 2021 with "Trump Incited Mob", the Washington Post opened with "President incites crowd to acts of insurrection", and the USA Today opened with "Trump incited crowd to march to Capitol Hill."

4.9.2 Content analysis of potential simultaneous events

My identification strategy relies on the assumption that there were no other events on January 6, or in the days surrounding January 6, that could also have induced a significant shift in political party identification.

To test whether this assumption is likely to hold I read the New York Times, the Washington Post, and the USA Today cover to cover on January 5, 6, and 7, 2021. I find that the most salient event in this period, besides the January 6 insurrection, was the Georgia runoff elections for the U.S. Senate, which were held on January 5, 2021.

After the general 2020 elections the Republican Party held 50 Senate seats, and the Democratic Party held 48 seats. As a result, the two runoff races in Georgia would determine which Party would control the Senate under the incoming Biden administration. In the early hours of January 6, 2021 both elections were called for the Democratic Party, giving the Democratic Party an effective majority in the Senate with Democratic Vice President Kamala Harris having the right to cast a tie-breaking vote. The extraordinary high political stakes of this election caused the race to attract significant nationwide attention.

I am not aware of research that suggests that the mere existence of salient elections or Republican Party defeats in crucial elections in itself causes major shifts in political party identification (which is necessary for this event to confound my results).

To test this null hypothesis more rigorously I run a placebo test analyzing the effect of the occurrence and results of the 2006 United States elections on Republican Party support. The 2006 elections were in two important respects comparable to the Republican Party' defeat in the 2021 Georgia runoff elections. First, the Republican Party suffered a historic defeat in 2006, losing control of both houses of Congress in one election cycle (which was the first time either party did so since the 1994 elections). Second, like the 2021 Georgia runoff elections, the bad performance of the Republican

Party in 2006 was largely blamed on an unpopular Republican president (George W. Bush in the 2006 case).

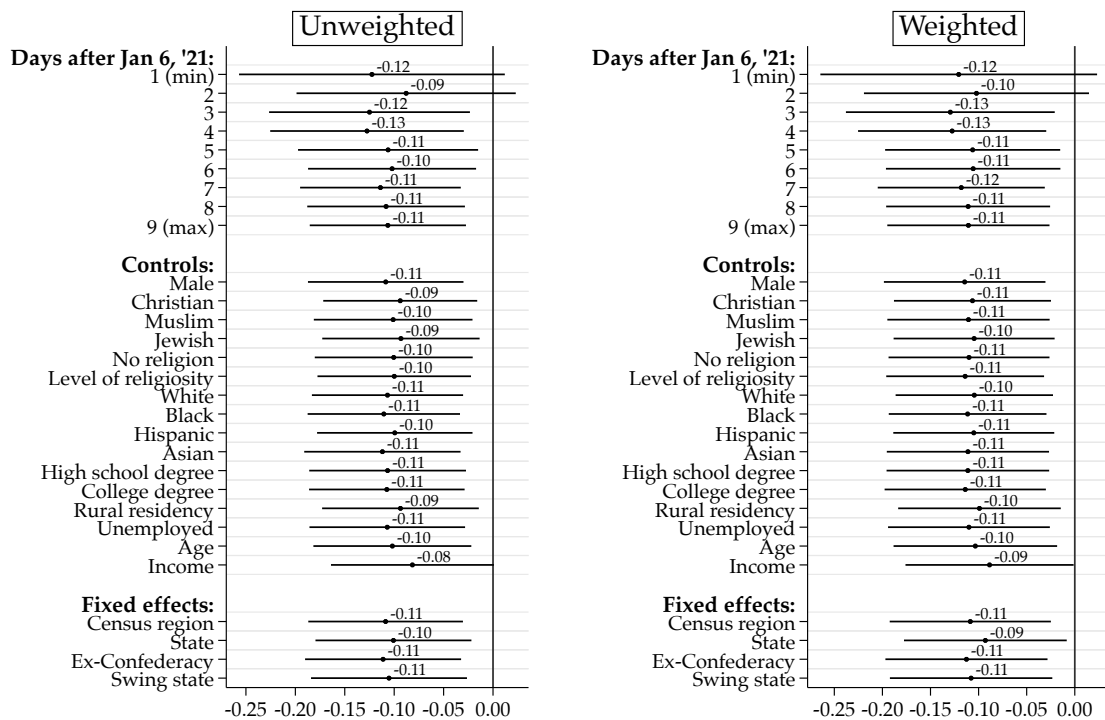
The 2006 elections occurred on November 7. To the best of my knowledge no public opinion survey was going on before and after November 7, 2006. Importantly, however, CBS News and the New York Times conducted a call-back poll, in which they surveyed 715 respondents on October 27–31 and then again on November 11–14. This allows me to estimate the effect of the election (result) by regressing a dummy capturing whether a respondent would vote for the Republican Party if there was an election today on a pre/post-election dummy, while controlling for individual-level fixed effects. Given that potential confounders are unlikely to have changed over such a short period of time this credibly identifies the causal effect of the election event.

In support of the null hypothesis I find that the 2006 electoral defeat of the Republican Party had no effect on Republican support in the CBS/NYT call-back poll (OLS coefficient: 0.000; P-value: 0.869).

4.9.3 Probit estimates

In the main text I use linear probability models for ease of interpretation. Figure 5.5 shows that the results remain essentially unchanged when using probit estimation.

Fig. 4.5 Results when using probit models.



Note: Point estimates and 95% confidence intervals of (separate) probit regressions of a dummy that takes the value 1 if a respondent considers, or leans towards considering, him/herself a Republican, and 0 otherwise, on a dummy that takes the value 1 if a respondent is interviewed after January 6, 2021, and 0 if a respondents is interviewed before January 6, 2021. Coefficients are average marginal effects. Weights are generated using American National Election Study data on the percentage of male, Christian, White, college educated, rural, and unemployed individuals that existed in the U.S. electorate at the end of 2020.

Chapter 5

Paper IV: The January 6 Insurrection and America's Standing Abroad: Natural Experimental Evidence from Five Unexpectedly Interrupted Surveys

Abstract

American geopolitical power partly relies on foreign public support for its leadership. Pundits worry that this support is evaporating now that the United States—which claims to be the world's beacon of democracy—has itself experienced democratic backsliding. I provide the first natural experimental test of this hypothesis by exploiting that the January 6 insurrection at the U.S. Capitol unexpectedly occurred while Gallup was conducting nationally representative surveys in India, Indonesia, Malaysia, Romania, and Vietnam. Because Gallup recruits respondents using random digit dialing I can identify the effect of the January 6 insurrection by comparing U.S. leadership approval among respondents that were interviewed just before, and just after, January 6, 2021. Surprisingly, I find that the insurrection had *no* effect on U.S. approval abroad. This suggests that American soft power may rely significantly less on America actually living up to its “beacon of democracy” mantra than typically presumed.¹

¹I would like to thank Andy Eggers, Tanushree Goyal, Layna Mosley, James Vreeland, and seminar participants at Princeton University for helpful comments and suggestions.

“[But] in inciting violence against the very government he leads and turning the country into an object of pity and scorn all over the world, Trump has depleted the resources that fuel America’s soft power.” (Axe, 2021)

“The allure of democracy was the nation’s best asset abroad, but the president squandered it by inciting political violence.” (Applebaum, 2021)

5.1 Introduction

The United States (U.S.) has long held a special place in the world in terms of global public support for its leadership. This global public support for U.S. leadership has been important. It has increased support for U.S. military missions (Goldsmith and Horiuchi, 2012), it has helped to delegitimize terrorist attacks against American citizens (Chiozza, 2015), it has enabled the U.S. to establish military bases in more than 80 countries (Bitar, 2016), and it has allowed the U.S. to have an outsized influence in the United Nations (Datta, 2009). Recognizing the importance of foreign public opinion the U.S. State Department invests around \$2 billion annually in public diplomacy efforts designed to increase foreign public support for U.S. leadership (Hannah, 2020).² Support for U.S. leadership has not only been important for the achievement of U.S. foreign policy goals. Levitsky and Way (2010), for example, find that since the end of the Cold War democratic transitions have been more likely to occur in countries where the U.S. has had more legitimacy and leverage.

Much work in International Relations suggests that the U.S.’ unusual level of legitimacy as a global superpower partly depends on its image as a beacon of democracy (e.g., Nye (2004)). By extension many scholars now worry that the January 6 insurrection at the U.S. Capitol—a clear and highly salient case of democratic backsliding within the

²Recognizing the importance of soft power China has also rapidly expanded funding for its soft-power projects and institutions (Glaser and Murphy, 2019; Green-Riley, Forthcoming).

US³—has significantly harmed America’s standing abroad.⁴ Nye (2021), for example, argues that:

“President Donald Trump’s term in office was not kind to American soft power. [...] But even more damaging to U.S. soft power was Trump’s effort to disrupt the orderly transition of political power after he lost the 2020 election. And on January 6, 2021, as Republican Senator Ben Sasse described the invasion of the U.S. Capitol, “the world’s greatest symbol of self-government was ransacked while the leader of the free world cowered behind his keyboard tweeting against his Vice President for fulfilling the duties of his oath to the Constitution.” America’s allies and other countries were shocked, and America’s attractiveness was diminished.”

But did the January 6 insurrection indeed *cause* support for U.S. leadership to decline? Or is foreign support for U.S. leadership perhaps not so strongly reliant on the U.S. actually living up to its mantra of a beacon of democracy after all?

I use a novel multi-country natural experiment to provide the, to the best of my knowledge, first causally identified answer to this crucial question. To do so I exploit that the January 6 insurrection at the U.S. Capitol unexpectedly occurred while the World Gallup Poll was conducting nationally representative public opinion surveys in India, Indonesia, Malaysia, Romania, and Vietnam. Because the insurrection was unexpected to foreign publics, did not coincide with other events that could plausibly affect U.S. leadership approval, and because Gallup samples respondents using random digit dialing I can identify the effect of the insurrection by comparing U.S. leadership approval among respondents that happened to be interviewed just before, and just after, January 6, 2021.

Surprisingly, the results suggest that the January 6 insurrection did *not* affect support for U.S. leadership abroad. This is true regardless of whether one compares

³“Democratic backsliding” is typically an incremental process whereby the political institutions that sustain an existing democracy are slowly hollowed out (Bermeo, 2016; Waldner and Lust, 2018). The January 6 insurrection—which was a violent attempt to stop the certification of the results of a free and fair election—can be seen as a particularly rapid and discrete acceleration of this debilitation process in a short period of time.

⁴See Appendix 5.8.1 for an overview of this literature.

respondents that were interviewed on January 5 and January 7, or when one compares respondents that were interviewed in the weeks before and after the January 6 insurrection. The result also remains unchanged when using different sets of controls, estimators, probability weights, and coding rules. Importantly, the large sample size ($N=4,317$) allows me to exclude with a high degree of certainty that this result is driven by a lack of statistical power.

The data allows me to exclude three plausible channels that could in theory explain *why* the January 6 insurrection did not depress support for U.S. leadership. First, I analyze google trends and news media data and find that it is unlikely that the null result is driven by a lack of information regarding the January 6 insurrection. Second, I analyze support for U.S. leadership before January 6 and find that the result is unlikely to be driven by support for U.S. leadership already being so low before January 6, 2021 that there was little room for further negative adjustment. Last, I analyze additional survey data and find that the result is unlikely to be driven by the U.S. before January 6, 2021 not being seen as a beacon of democracy to begin with.

I interpret these results as suggesting that the null effect of the January 6 insurrection on U.S. foreign leadership approval is simply driven by foreign publics conditioning their support for U.S. leadership solely on other factors outside the domestic state of American democracy. This may be explained by the fact that anti-democratic events within the U.S. arguably have little direct influence on the lives of foreign publics. If this is true one would expect that events in American politics that are more likely to directly affect foreign publics—such as leadership changes—*do* affect U.S. leadership approval. I find strong evidence that this is indeed the case. More specifically, I find that approval of U.S. leadership increased with approximately 21.5% (or, approximately 0.43 standard deviations) after the inauguration of Joe Biden, which

occurred on January 20, 2021 when World Gallup Polls were still ongoing in India, Malaysia, and Romania.

It is possible that January 6 insurrection had significantly greater effects on public opinion in other countries outside of India, Indonesia, Malaysia, Romania, and Vietnam (which themselves house approximately 1.8 billion people—or, 23.1% of the world’s population).⁵ However, even if this is indeed the case the results do suggest that existing theory that predicts that foreign approval of U.S. leadership significantly depends on the U.S. living up to its democratic ideals should, at the minimum, be understood in considerably more conditional terms than it currently is.

The paper contributes to the literature on the causes of soft power. The core idea of “soft” power is that rather than coercing others to do what you want (which would be “hard” power) you get others to “want what you want”, through appeal and attraction, so that coercion becomes unnecessary (Nye, 1990, 2004). Nye (2004, p. 11) argues that a country’s soft power capability primarily relies on (1) its culture (in places where it is attractive to others), (2) its political values (when it lives up to them at home and abroad), and (3) its foreign policies (when they are seen as legitimate and having moral authority). Empirically, it has been very challenging to test Nye’s (2004) theory because it is very rare to be able to exploit a random shock to (1), (2), or (3). This paper contributes by the, to the best of my knowledge, first natural experimental test of Nye’s (2004) theory. It does so by exploiting a random shock to (2)—i.e., the degree to which a country lives up to the political values that it itself proclaims. Further research is necessary to examine whether the results that I find generalize to culture, foreign policy, and other types of political value treatments.⁶

⁵Note that especially India and Romania are crucial strategic partners for the U.S. in the ongoing global power competition with China and Russia, respectively.

⁶Note that Nye (2004) himself specifically argues that while the type of public opinion polls used in this paper are not without limitations they are the best way to measure soft power in practice (e.g., p. 8)

5.2 The January 6 insurrection

On November 3, 2020 Joe Biden (Democrat) won the 2020 U.S. presidential election of sitting U.S. president Donald Trump (Republican) with a difference of a little more than 7 million popular votes (and 74 Electoral College votes). Despite this enormous difference in the number of votes, and the absence of any convincing evidence of widespread voting fraud, Donald Trump claimed that the election was fraudulent and that he was the true winner of the election.

After several months of constant misinformation regarding the election, and a wide range of highly publicized court cases (which were all essentially dismissed for a lack of evidence), Donald Trump held a speech at the “Stop the Steal” rally on January 6, 2021. In the speech Trump said, among other things, that the election was stolen, that he and his supporter will never concede, that they will fight like hell, and that they are going to walk down to the Capitol because “you’ll never take your country back with weakness.”⁷

Directly after this speech a large mob of Trump supporters stormed the U.S. Capitol building, leading to the death of 5 people, the injury of at least 138 police officers, physical property damages in excess of 30 million dollars, the abrupt halt of an ongoing congressional debate, and the immediate evacuation of all members of Congress.

I regard this event as a clear case of democratic backsliding that should be noticed and sanctioned as such *if* foreign publics are indeed conditioning their support for U.S. leadership on domestic events related to American democracy.

⁷See Appendix 5.8.2 for direct quotes from the speech.

5.3 Data

To test the effect of the January 6 insurrection on foreign public support for U.S. leadership I use data from the World Gallup Poll. I make use of World Gallup Poll data because this is the only public opinion survey that regularly conducts surveys in virtually all countries in the world (making overlap with the insurrection event likely) while using random digit dialing for the recruitment of respondents (ensuring that assignment to treatment is random). In five countries—India, Indonesia, Malaysia, Romania, and Vietnam—World Gallup Polls were ongoing when the insurrection at the U.S. Capitol occurred on January 6, 2021.⁸ Appendix 5.8.3 shows the number of respondents that were interviewed in the days before and after January 6, 2021 in each individual country.

As the dependent variable I use a dummy that takes the value 1 if a respondent approves of the job performance of the leadership of the US, and 0 if a respondent disapproves. This variable is regularly used to measure America’s standing in the world (e.g., Goldsmith, Horiuchi and Matush (2021)). I drop respondents that answer that they don’t know whether they approve or disapprove of U.S. leadership. Results remain the same when including “don’t know” responses in the 0 category.

As the treatment variable I use a dummy that takes the value 1 if a respondent was interviewed after January 6, 2021, and 0 if a respondent was interviewed before this date. I drop respondents that were interviewed on January 6 itself, as I cannot be certain whether these respondents are treated or untreated (the World Gallup Poll does not provide data on the exact time of interview). Including these respondents in either the control or treatment group makes no substantive difference to the results.

⁸The January 6 insurrection also overlapped with a World Gallup Poll in Pakistan. I exclude Pakistan because only 48 respondents were interviewed in Pakistan prior to January 6, 2021, and these respondents were interviewed on January 2 and 3 (no respondents were interviewed on January 4 and 5). Including Pakistan makes no substantive difference to the results.

5.4 Identification strategy

To identify the causal effect of the insurrection I employ a so-called “unexpected event during survey” design. Causal identification in these designs relies on two assumptions. First, *temporal ignorability*, meaning that whether any particular individual is interviewed before or after January 6, 2021 should be orthogonal to any other individual-level characteristic that also affects support for U.S. leadership. Second, *excludability*, meaning that the timing of interview should affect support for U.S. leadership only through the insurrection event, not through any other channel (Muñoz, Falcó-Gimeno and Hernández, 2020).

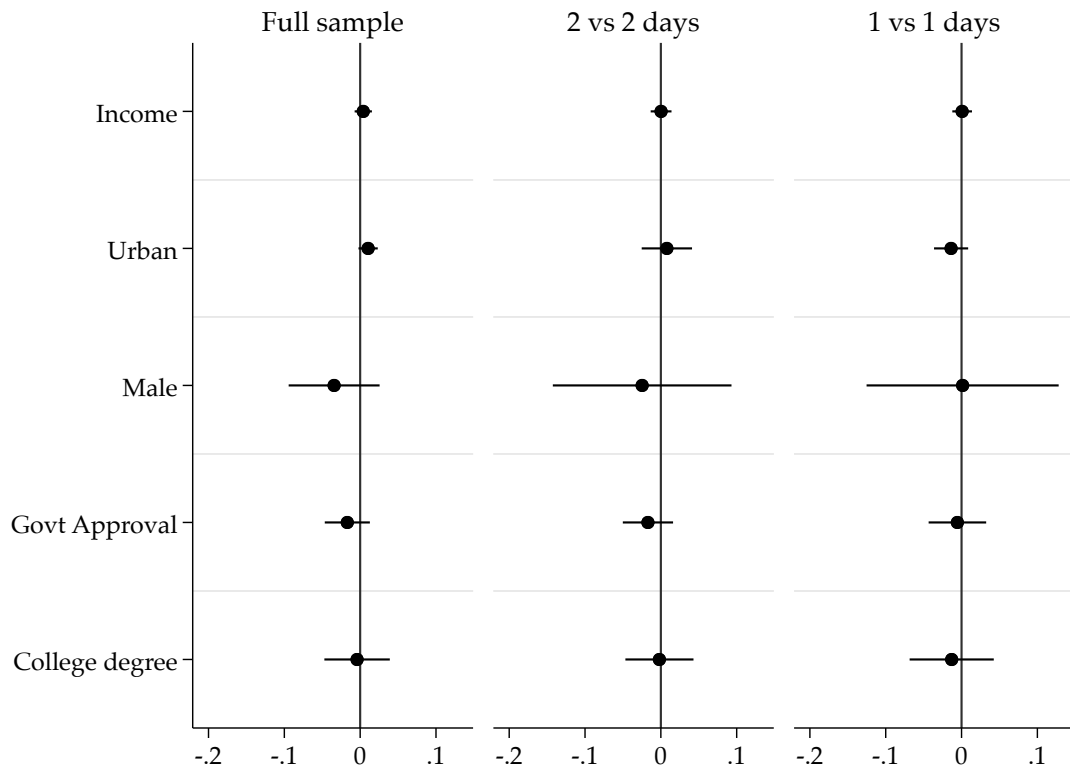
These identifying assumptions are likely to hold in this case. First, the World Gallup Poll samples respondents using random digit dialing. This ensures that any individual, regardless of his/her background characteristics, and regardless of where he/she lives, is equally likely to be interviewed on any particular day. In line with this I find that the treatment and control groups are balanced on a number of pre-treatment variables that may also affect support for U.S. leadership (see figure 5.1).⁹

Second, analysis of news media suggests that there were no other events on January 6, 2021 that could plausibly have affected support for U.S. leadership in foreign countries (see Appendix 5.8.5).

Last, the insurrection at the U.S. Capitol was plausibly unexpected to foreign publics. This makes it unlikely that respondents that were interviewed prior to January 6, 2021 had already changed their approval of U.S. leadership in expectation of the January 6 insurrection.

⁹See Appendix 5.8.4 for the measurement of these covariates.

Fig. 5.1 Balance on observables.



Note: Point estimates and 95% confidence intervals of (separate) OLS regressions. The dependent variable is a dummy that takes the value 1 if a respondent is surveyed after January 6, 2021, and 0 if a respondent is surveyed before this day. Confidence intervals are robust to heteroscedasticity and corrected for autocorrelation at the country-level. Countries included are: India, Indonesia, Malaysia, Romania, and Vietnam. Country fixed effects are included but omitted to save space. Data on government approval is missing for Vietnam.

5.5 Results

In table 5.1 I use OLS to regress the above-described U.S. leadership approval dummy on a dummy capturing whether a respondent was interviewed before (category 0) or after (category 1) January 6, 2021. All regressions include country fixed effects. Omitting these makes no substantive difference to the results.

Column (1) shows that respondents that were interviewed prior to January 6, 2021 were on average 1% less supportive of U.S. leadership, as compared to respondents that

were interviewed after January 6.¹⁰ This effect is only 1.9% of the pre-treatment mean of U.S. leadership approval, and is highly statistically insignificant (P-value: 0.711). Columns (2), (3), and (4) show that similar negligibly small estimates are obtained when restricting the sample to include only respondents that were interviewed in the 7, 2, and 1 days before and after January 6, 2021.

This null result remains unchanged (1) when using the probability weights provided by Gallup to increase the population-wide representativeness of the data (Panel B of table 5.1), (2) when adding the covariates from figure 5.1 (Appendix 5.8.6), (3) when using logit or probit estimation instead of OLS (Appendix 5.8.7), and (4) when analyzing the effect for each country individually (Appendix 5.8.8).¹¹

Taken together these results suggests that the January 6 insurrection had on average *no* effect on U.S. leadership approval in India, Indonesia, Malaysia, Romania, and Vietnam. Crucially, this null effect is *not* due to a lack of statistical power. For example, the baseline estimate in panel A column (1) suggests that two standard error bands exclude effects greater than -6.2%. Even this effect is only 11.8% of the pre-treatment mean, and within all countries insufficient to change the relative order of approval between the US, China, and Russia.

5.6 Why had the January 6 insurrection no effect on U.S. leadership approval abroad?

A natural question to ask is *why* the peoples of India, Indonesia, Malaysia, Romania, and Vietnam did not lower their support for U.S. leadership as a result of the January

¹⁰The sample in column (1) of table 5.1 is restricted to respondents that were interviewed before January 20, 2021, in order to avoid conflating the results with the effect of the inauguration of president Joseph Biden (see next section).

¹¹The exception is Romania. Here I do find a significant negative effect of the January 6 insurrection. This effect is, however, entirely offset by the positive effect of Donald Trump's replacement by Joe Biden on January 20, 2021 (see next section).

Table 5.1 Effect of January 6 insurrection at the U.S. Capitol.

| | (1) | (2) | (3) | (4) |
|--|-------------------|-------------------|-------------------|-------------------|
| Panel A: Sample average treatment effect (SATE) | | | | |
| 6 January 2021 | -0.010 (0.026) | -0.005 (0.032) | -0.009 (0.048) | -0.046 (0.053) |
| Mean of DV | 0.527 | 0.570 | 0.558 | 0.563 |
| Days pre/post | All | 7 | 2 | 1 |
| Country FE | Yes | Yes | Yes | Yes |
| N | 4317 | 2476 | 923 | 472 |
| Panel B: Population average treatment effect (PATE) | | | | |
| 6 January 2021 | -0.035 (0.021) | -0.035 (0.026) | -0.034 (0.039) | -0.084 (0.054) |
| Mean of DV | 0.527 | 0.578 | 0.570 | 0.581 |
| Days pre/post | All | 7 | 2 | 1 |
| Country FE | Yes | Yes | Yes | Yes |
| N | 4317 | 2476 | 923 | 472 |

Notes: Each column within panel A and B is a separate OLS regression. The dependent variable is a dummy that takes the value 1 if a respondent approves of the job performance of the leadership of the US, and 0 if a respondent disapproves. Countries included are: India, Indonesia, Malaysia, Romania, and Vietnam. Robust standard errors clustered on the country-level are reported in parentheses. Panel B uses the household probability weights provided by Gallup.

*** p<0.001, ** p<0.01, * p<0.05.

6 insurrection, even through standard soft power theory (e.g., Nye (2004)) strongly suggests that they would. I here examine four possible explanations.

Lack of information: One possible explanation is that people in India, Indonesia, Malaysia, Romania, and Vietnam were simply insufficiently aware of the January 6 insurrection.¹² Appendix 5.8.9 suggests that this is unlikely to be the case. The insurrection at the U.S. Capitol was prominently covered in each country's news media in the days following January 6, 2021, and Google Trends data suggests that searches with the terms "Trump" and "Capitol" were among the most frequent internet searches in all five countries right after January 6, 2021.

Floor effects: The null effect is also unlikely to be driven by support for the U.S. already being so low before January 6, 2021 that there was little scope for further decline. Approval of U.S. leadership in the week before January 6 was 58.7%, 40.9%, 35.8%, 54.2%, and 81.5% in India, Indonesia, Malaysia, Romania, and Vietnam, respectively. Support for China in these same countries was meanwhile only 14.8%, 35.9%, 50.0%, 39.0%, and 16.2%, respectively.

The U.S. not seen as a beacon of democracy to begin with: Appendix 5.8.10 suggests that the null effect is also unlikely to be explained by the U.S. not being seen as a beacon of democracy before January 6, 2021 to begin with. Data from Hannah (2020) suggests, for example, that 84% of Indians held positive opinions of "American-style" democracy as late as 2020. Data for the other countries is somewhat more sporadic, but does consistently point in the same direction.

Foreign publics condition their support for U.S. leadership on other factors: Taken together the above appears to leave room for only one last explanation—

¹²Note that the possibility of a lack of information does not invalidate my identification strategy. This is because knowledge of the democratic backsliding event is itself part of the process of theoretical interest. If even something as extreme as an attempt to violently overturn the results of a free and fair election is insufficient for foreign peoples to be informed about the state of American democracy, it is safe to assume that no democratic backsliding event will noticeably affect foreign support for U.S. leadership.

people in India, Indonesia, Malaysia, Romania, and Vietnam simply condition their support for U.S. leadership on other variables than the domestic state of American democracy. This may be explained by the fact that democratic backsliding events within the U.S. are arguably highly unlikely to directly affect the lives of people living in India, Indonesia, Malaysia, Romania, and Vietnam. If so, one would expect to find strong effects of events that are more likely to affect the lives of foreign publics—such as changes in U.S. leadership and foreign policy. In line with this conjecture I find that the replacement of Donald Trump by Joe Biden on January 20, 2021, which occurred while World Gallup Polls were still ongoing in India, Malaysia, and Romania, and which represented a sea-shift in U.S. leadership and foreign policy, *did* strongly affect support for U.S. leadership (see table 5.2).¹³ This result is in line with the work of Agadjanian and Horiuchi (2020), Bateson and Weintraub (Forthcoming), and Goldsmith and Horiuchi (2012), who have shown that support for U.S. leadership is strongly affected by the content of foreign policy and the identity of the U.S. president.¹⁴

¹³Note that support went up in all countries, and this result is highly robust to controls and probit/logit estimation (see Appendices 5.8.6, 5.8.7, and 5.8.8).

¹⁴In this case the identity of the president and the content of foreign policy arguably both changed with the replacement of Donald Trump by Joe Biden, making it difficult to cleanly separate these two mechanisms. Agadjanian and Horiuchi (2020) attempt to decompose the effect of the content of foreign policy and the identity of the U.S. president using a survey experiment. They find that both matter but that the content of foreign policy matters more.

Table 5.2 Effect of January 20 inauguration of president Biden.

| | (1) | (2) | (3) | (4) |
|--|---------------------|---------------------|---------------------|---------------------|
| Panel A: Sample average treatment effect (SATE) | | | | |
| 20 January 2021 | 0.215*** (0.022) | 0.212** (0.034) | 0.237** (0.016) | 0.316* (0.043) |
| Mean of DV | 0.537 | 0.528 | 0.551 | 0.526 |
| Days pre/post | All | 7 | 2 | 1 |
| Country FE | Yes | Yes | Yes | Yes |
| N | 2778 | 1393 | 466 | 245 |
| Panel B: Population average treatment effect (PATE) | | | | |
| 20 January 2021 | 0.182*** (0.036) | 0.166*** (0.039) | 0.190*** (0.055) | 0.235*** (0.069) |
| Mean of DV | 0.522 | 0.527 | 0.552 | 0.565 |
| Days pre/post | All | 7 | 2 | 1 |
| Country FE | Yes | Yes | Yes | Yes |
| N | 2778 | 1393 | 466 | 245 |

Notes: Each column within panel A and B is a separate OLS regression. The dependent variable is a dummy that takes the value 1 if a respondent approves of the job performance of the leadership of the US, and 0 if a respondent disapproves. Countries included are: India, Malaysia, and Romania. Robust standard errors clustered on the country-level are reported in parentheses. Panel B uses the household probability weights provided by Gallup.

*** p<0.001, ** p<0.01, * p<0.05.

5.7 Discussion

The novel multi-country natural experiment analyzed in this paper allows me to state with a high degree of certainty that the January 6 insurrection did *not* have a significant lasting effect on U.S. leadership approval in India, Indonesia, Malaysia, Romania, and Vietnam. An important question for future research is whether this result also generalizes to other countries and times. Another important question for future research is how the results of this paper generalize to other “type” of democratic backsliding treatments. On the one hand, one could argue that a violent attempt to overturn the results of a free and fair election is close to a most-likely case for the hypothesis that democratic backsliding within the U.S. causes foreign support for U.S.

leadership to decline. On the other hand, one could imagine that the *accumulation* of many different democratic backsliding events within the U.S. could eventually erode global support for its leadership.

5.8 Appendix

5.8.1 Overview of existing literature

Below I provide quotes that provide an overview of the literature on the role of the image of the U.S. as a beacon of democracy in the generation of soft power in general, and the impact of the January 6 insurrection on America's soft power in particular:

"Political values like democracy and human rights can be powerful sources of attraction, but it is not enough just to proclaim them. [...] Perceived hypocrisy is particularly corrosive of power that is based on proclaimed values. Those who scorn or despise us for hypocrisy are less likely to want to help us achieve our policy objectives." (Nye, 2004, p. 55)

"[Moreover,] we [the U.S. government, SvN] maintain strengths of openness, civil liberties, and democracy that appeal to others. Problems arise for our soft power when we do not live up to our own standards." (Nye, 2004, p. 143)

"US soft power rests partly on American culture and foreign policies when they are attractive to others; but it also rests on our values and how we practice democracy at home." (Nye, 2021)

"[But] in inciting violence against the very government he leads and turning the country into an object of pity and scorn all over the world, Trump has depleted the resources that fuel America's soft power." (Axe, 2021)

"The allure of democracy was the nation's best asset abroad, but the president squandered it by inciting political violence." (Applebaum, 2021)

"[...] the violent storming of the US Capitol by pro-Trump insurrectionist-wannabees has damaged the United States' image badly." [...] Photojournalist Eddie Adam's iconic shot of a Saigon police chief executing a Vietcong prisoner during the Tet Offensive in 1968 captured indelibly the sadness, violence, and futility of the Vietnam War. [...] Historians may someday view the violent footage of a mob storming the Capitol waving Confederate and MAGA flags as no less damaging [...]." (Blanchette, 2021)

"Beyond the domestic concerns faced in the aftermath of the breach of one of America's most hallowed buildings, the Capitol siege was a win for China. US soft power, one of its comparative advantages in the great power competition, has taken a huge hit." (Kokas and Mastro, 2021)

5.8.2 Quotes from Trump's speech on January 6, 2021

My identification strategy relies on the claim that Donald Trump's speech on the January 6 "Stop the Steal" rally incited the insurrection at the U.S. Capitol, and should therefore be interpreted by foreign publics as a clear case of democratic backsliding within the US.

Below, I provide a number of direct quotes from Donald Trump's speech to substantiate this assertion:

"All of us here today do not want to see our election victory stolen by emboldened radical-left Democrats, which is what they're doing. And stolen by the fake news media. That's what they've done and what they're doing. We will never give up, we will never concede. It doesn't happen. You don't concede when there's theft involved."

"Our country has had enough. We will not take it anymore and that's what this is all about. And to use a favorite term that all of you people really came up with: We will Stop the Steal."

"Because if Mike Pence does the right thing, we win the election. [...] All Vice President Pence has to do is send it back to the states to recertify and we become president and you are the happiest people."

"[...] we're going to walk down to the Capitol, and we're going to cheer on our brave senators and congressmen and women, and we're probably not going to be cheering so much for some of them."

"Because you'll never take back our country with weakness. You have to show strength and you have to be strong. "

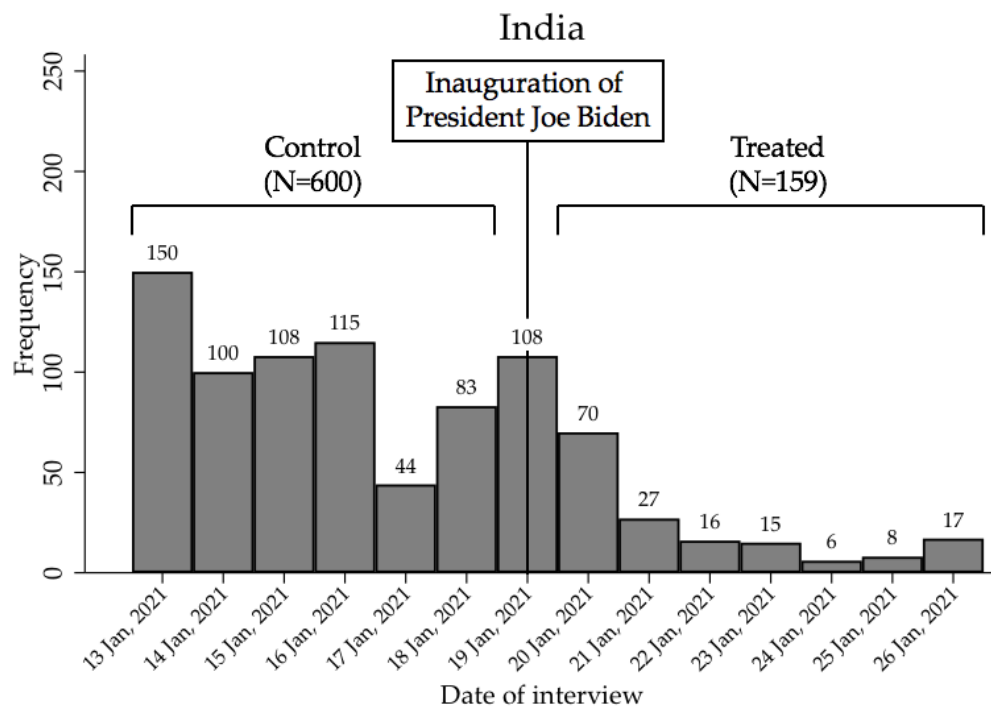
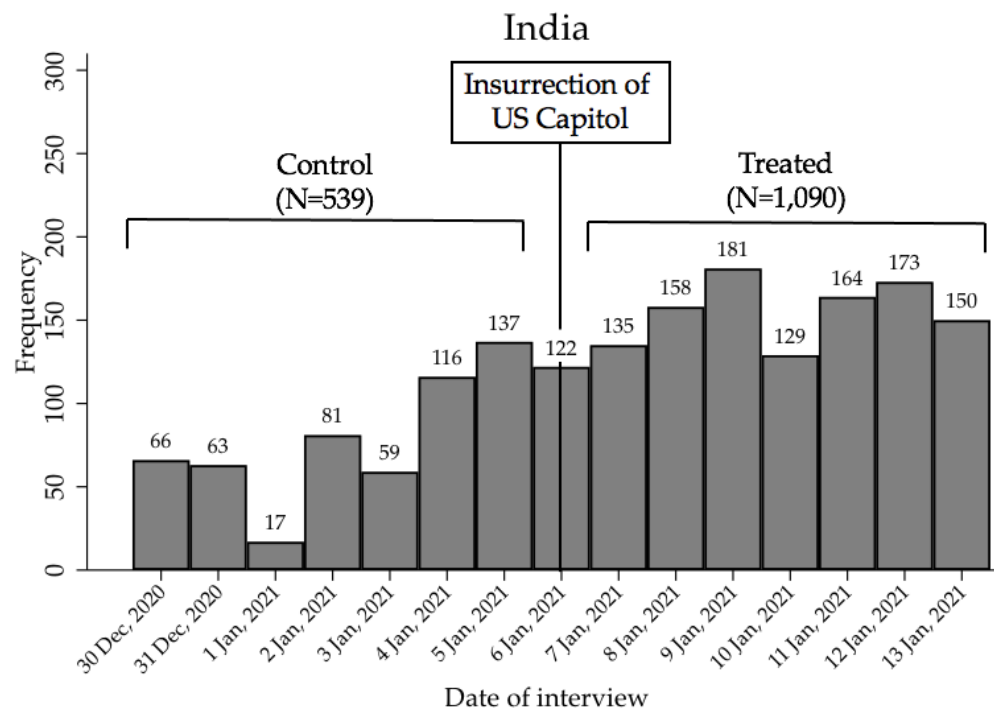
"We fight like hell. And if you don't fight like hell, you're not going to have a country anymore."

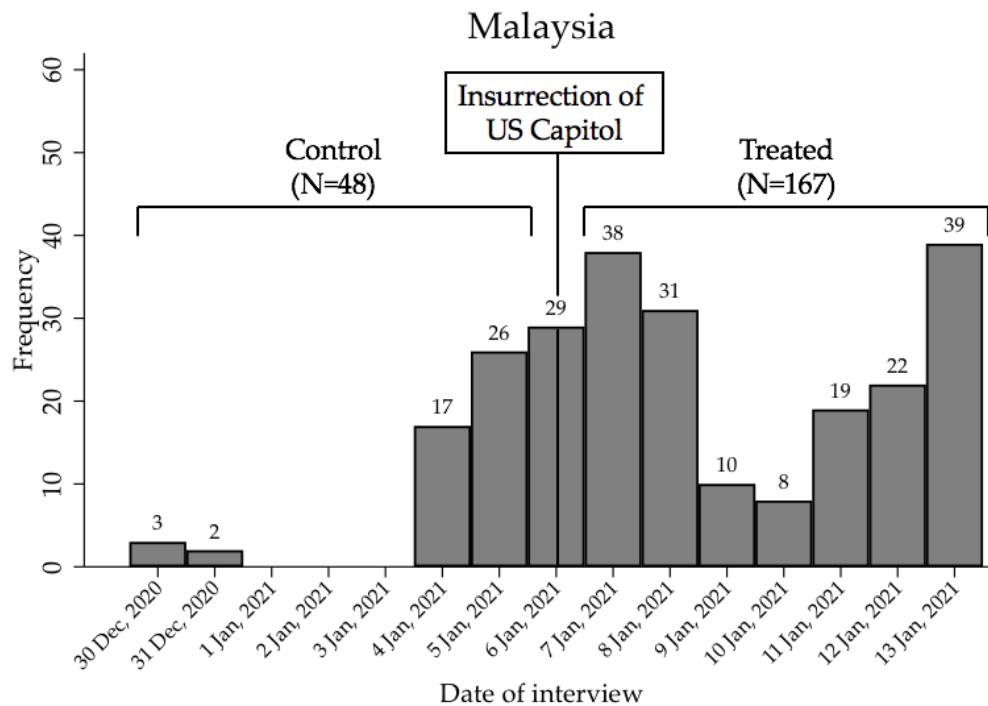
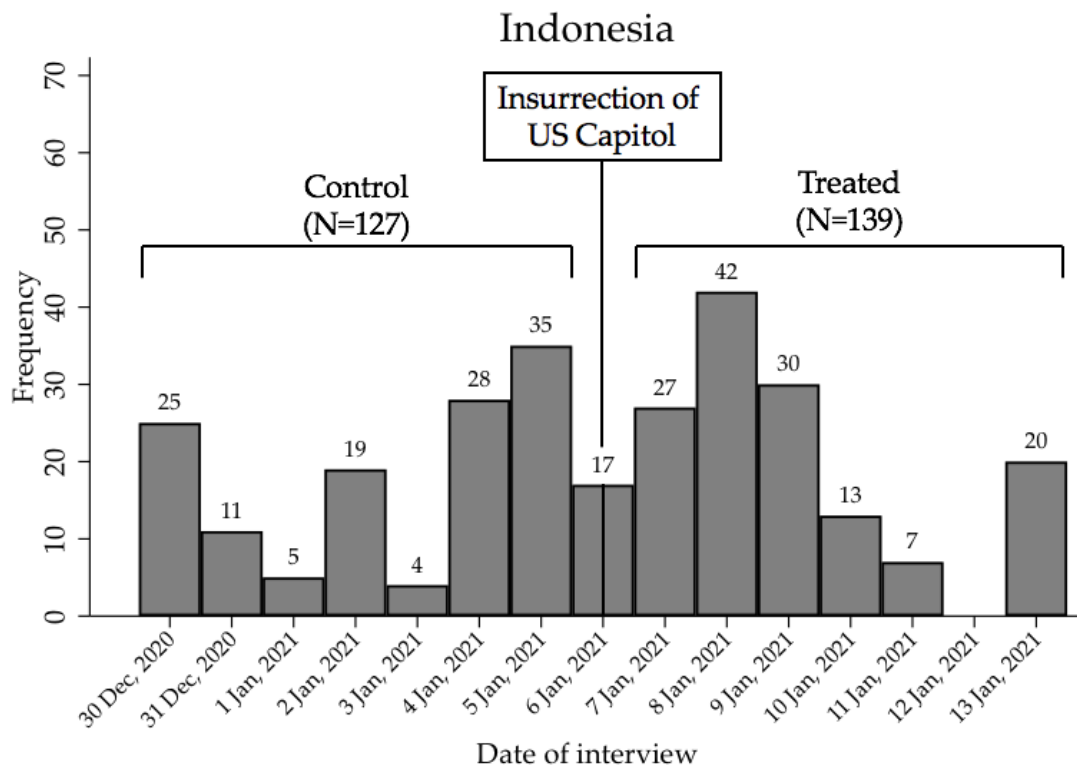
"So let's walk down Pennsylvania Avenue."

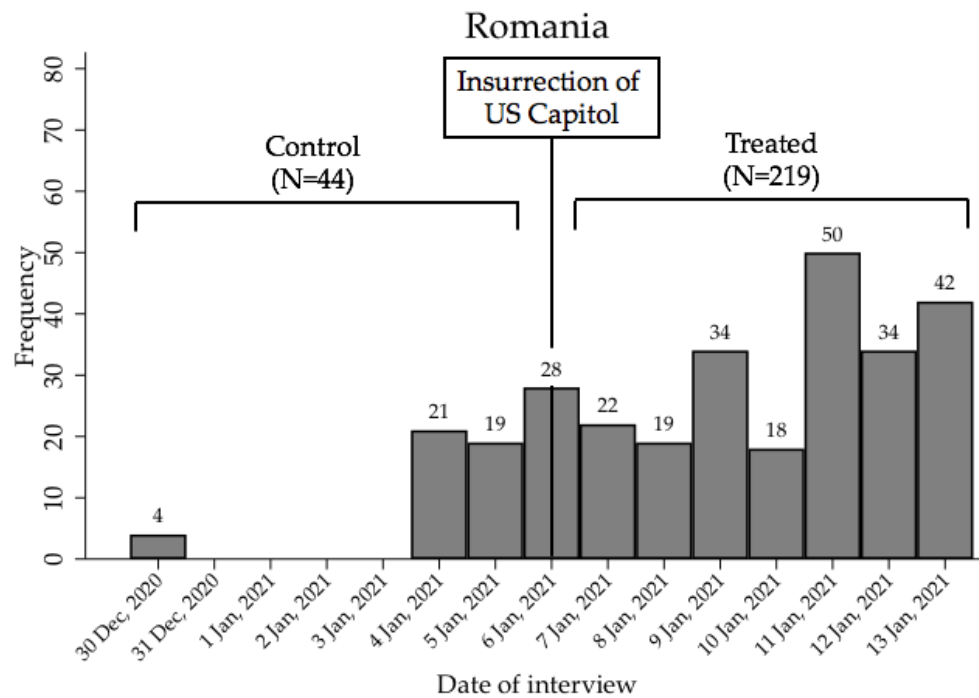
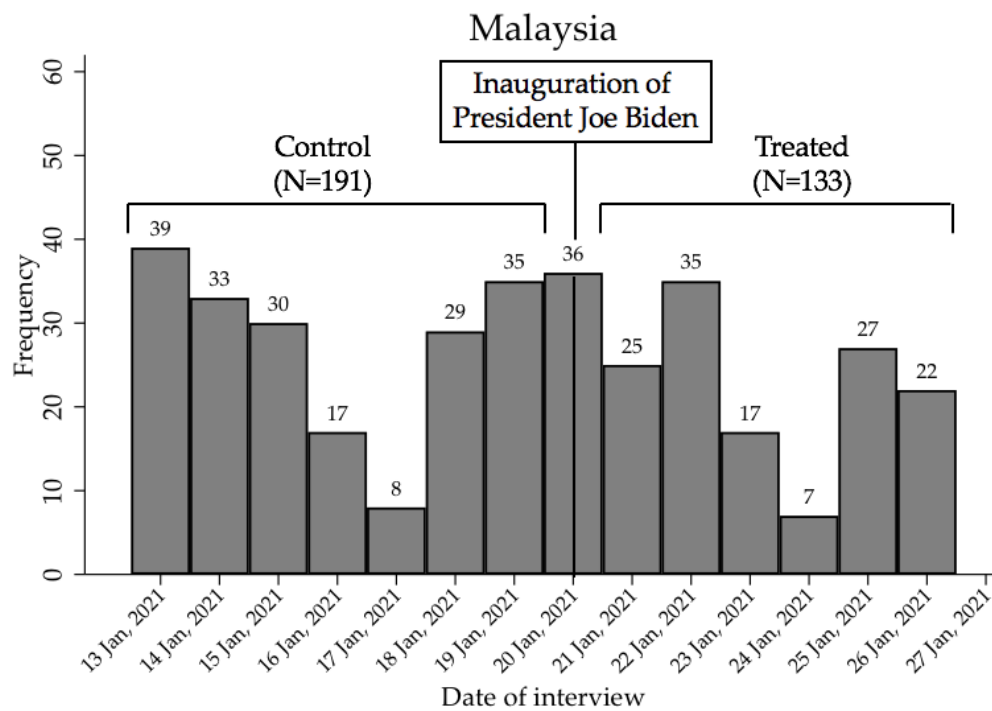
Other independent observers of American politics have also characterized the words of Trump as inciting the insurrection. The New York Times, for example, opened on January 7, 2021 with "Trump Incited Mob", the Washington Post opened with "President Incites Crowd to Acts of Insurrection", and the USA Today opened with "Trump Incited Crowd to March to Capitol Hill."

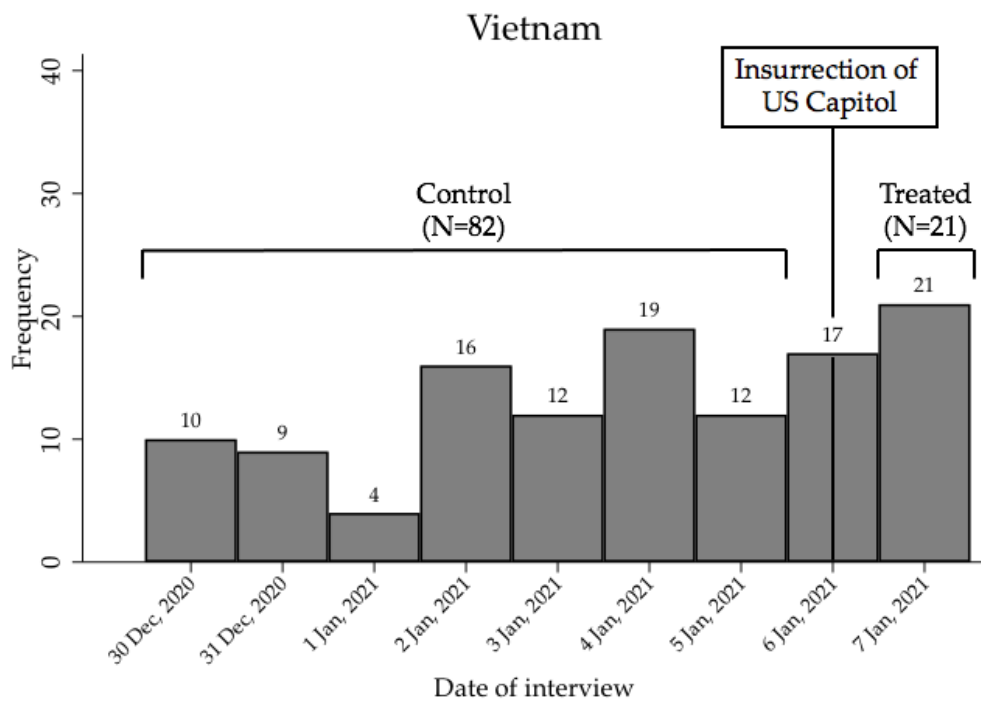
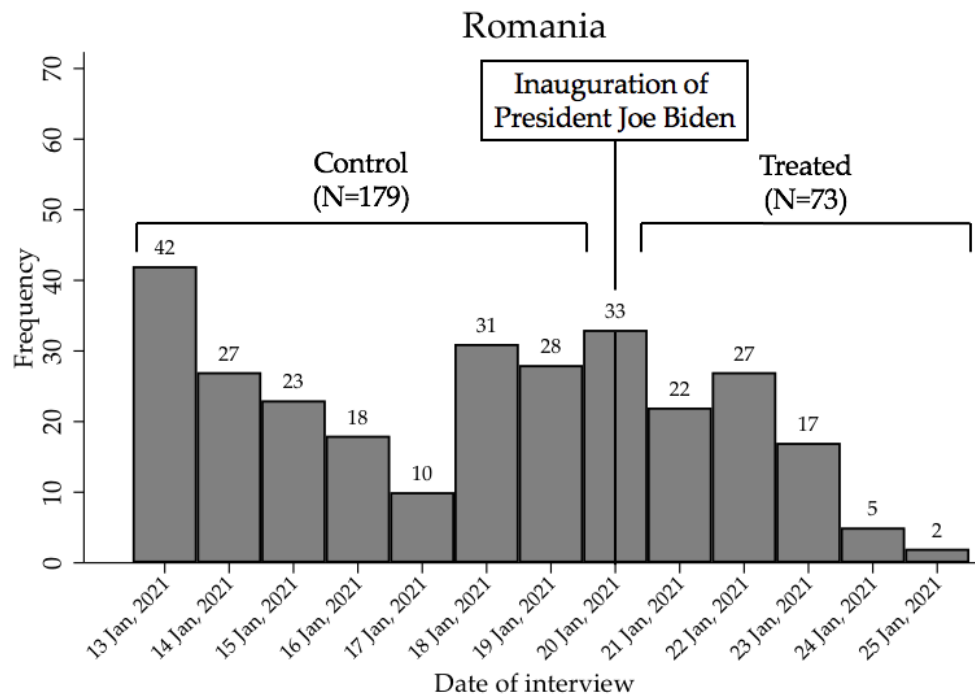
5.8.3 Distribution of respondents in each country

Below I plot the number of respondents that were interviewed by the World Gallup Poll in the 7 days before and after January 6 and January 20, 2021. In the main text I also report results when including all respondents that were interviewed longer before and after January 6 and January 20, 2021 (which increases the sample size with approximately 43%). Information on the distribution of these full-sample responses is available on request.









5.8.4 Measurement covariates

Below I describe the measurement of the covariates used for the balance on observables test in figure 1 of the main text.

Income: Natural log of annual per capita income measured in International Dollars.

Urban: Variable that takes the value 0 if a respondent lives in a rural area, 1 if a respondent lives in a small town or village, 2 if a respondent lives in a suburb of a large city, and 3 if a respondent lives in a large city. In the main text I use this variable as-if measured on the ratio level. Treating this variable as-if measured on the ordinal level makes no substantive difference to the results.

Male: Dichotomous variable that takes the value 1 if a respondent self-identifies as male, and 0 if a respondent self-identifies as female.

Age: Years since a respondent was born.

Govt approval: Dichotomous variable that takes the value 1 if a respondent approves of the job performance of the leadership of his/her own country, and 0 if a respondent disapproves of the job performance of the leadership of his/her country. Respondents that answer that they don't know are coded as missing. Including the "don't knows" in the 0 category makes no substantive difference to the results.

College degree: Dichotomous variable that takes the value 1 if a respondent has completed four years of post-high school education and/or received a 4-year college degree, and 0 otherwise.

5.8.5 Potential simultaneous events

My identification strategy relies on the assumption that there were no other events on or around January 6, that could also have affected support for U.S. leadership in India, Indonesia, Malaysia, Romania, and Vietnam.

To test whether this assumption is likely to hold I read the New York Times and the USA Today cover to cover on all days from January 4 to January 8, 2021. I find that during this period there were no major geopolitical events involving the U.S. that could have plausibly affected support for U.S. leadership abroad. I also consider whether there occurred other domestic events within the U.S. that could have affected foreign approval of U.S. leadership. I find that this is also unlikely to be the case. The most important event on the eve of the January 6 insurrection appears to have been the U.S. Senate runoff elections in Georgia. I am not aware of any studies that find that U.S. Senate elections affect support for U.S. leadership abroad.

5.8.6 Results with controls

Table 5.3 adds the covariates described in Appendix 5.8.4 to the basic regression results. As can be seen controlling for these variables makes no substantive difference to the results.

Table 5.3 Results with controls.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|--|-------------------|-------------------|-------------------|-------------------|---------------------|---------------------|---------------------|---------------------|
| Panel A: Sample average treatment effect (SATE) | | | | | | | | |
| 6 January 2021 | -0.003 (0.033) | -0.000 (0.042) | -0.017 (0.057) | -0.053 (0.059) | | | | |
| 20 January 2021 | | | | | 0.213** (0.024) | 0.221** (0.025) | 0.229* (0.025) | 0.289** (0.025) |
| Mean of DV | 0.480 | 0.546 | 0.551 | 0.545 | 0.539 | 0.531 | 0.554 | 0.542 |
| Days pre/post | All | 7 | 2 | 1 | All | 7 | 2 | 1 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 3923 | 2311 | 846 | 424 | 2672 | 1348 | 449 | 237 |
| Panel B: Population average treatment effect (PATE) | | | | | | | | |
| 6 January 2021 | -0.029 (0.021) | -0.031 (0.026) | -0.045 (0.039) | -0.098 (0.056) | | | | |
| 20 January 2021 | | | | | 0.188*** (0.036) | 0.181*** (0.038) | 0.194*** (0.052) | 0.213*** (0.065) |
| Mean of DV | 0.487 | 0.561 | 0.565 | 0.570 | 0.524 | 0.531 | 0.553 | 0.582 |
| Days pre/post | All | 7 | 2 | 1 | All | 7 | 2 | 1 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 3923 | 2311 | 846 | 424 | 2672 | 1348 | 449 | 237 |

Notes: Each column within panel A and B is a separate OLS regression. The dependent variable is a dichotomous variable that takes the value 1 if a respondent approves of the job performance of the leadership of the US, and 0 if a respondent disapproves. The independent variable is a dichotomous variable that takes the value 1 if a respondent is surveyed after January 6 or after January 20, 2021, and 0 if a respondent is surveyed before this day. Countries included in columns (1) to (4) are: India, Indonesia, Malaysia, Romania, and Vietnam. Countries included in columns (5) to (8) are: India, Malaysia, and Romania. All regressions include country fixed effects. Robust standard errors clustered on the country-level are reported in parentheses. The mean of the dependent variable is measured among all respondents that were interviewed pre-treatment. Panel B uses the household probability weights provided by Gallup.

*** p<0.001, ** p<0.01, * p<0.05.

5.8.7 Logit/probit estimation

In the main text I use simple ordinary least squares (OLS) to regress a dichotomous variable that takes the value 1 if a respondent approves of the job performance of the leadership of the US, and 0 if a respondent disapproves, on a dichotomous variable capturing whether a respondent was interviewed before (category 0) or after (category 1) January 6, 2021. I choose to use a linear probability model in the main text for the ease of interpretation. Tables 5.4 and 5.5 show that all results remain unchanged when using logit or probit instead of OLS.

Table 5.4 Results with logit estimation.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|--|------------------|------------------|------------------|------------------|---------------------|---------------------|---------------------|---------------------|
| Panel A: Sample average treatment effect (SATE) | | | | | | | | |
| 6 January 2021 | 0.957 (0.106) | 0.978 (0.131) | 0.962 (0.195) | 0.819 (0.186) | | | | |
| 20 January 2021 | | | | | 2.553*** (0.314) | 2.517*** (0.456) | 3.016*** (0.590) | 5.026*** (2.032) |
| Mean of DV | 0.528 | 0.570 | 0.558 | 0.563 | 0.537 | 0.528 | 0.551 | 0.526 |
| Days pre/post | All | 7 | 2 | 1 | All | 7 | 2 | 1 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 4317 | 2476 | 923 | 472 | 2778 | 1393 | 466 | 245 |
| Panel B: Population average treatment effect (PATE) | | | | | | | | |
| 6 January 2021 | 0.864 (0.076) | 0.864 (0.094) | 0.867 (0.143) | 0.702 (0.161) | | | | |
| 20 January 2021 | | | | | 2.171*** (0.355) | 2.014*** (0.352) | 2.367** (0.632) | 3.327** (1.319) |
| Mean of DV | 0.527 | 0.578 | 0.570 | 0.581 | 0.522 | 0.527 | 0.552 | 0.565 |
| Days pre/post | All | 7 | 2 | 1 | All | 7 | 2 | 1 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 4317 | 2476 | 923 | 472 | 2778 | 1393 | 466 | 245 |

Notes: Each column within panel A and B is a separate logistic regression. Coefficients are odds ratios. The dependent variable is a dichotomous variable that takes the value 1 if a respondent approves of the job performance of the leadership of the US, and 0 if a respondent disapproves. The independent variable is a dichotomous variable that takes the value 1 if a respondent is surveyed after January 6 or after January 20, 2021, and 0 if a respondent is surveyed before this day. Countries included in columns (1) to (4) are: India, Indonesia, Malaysia, Romania, and Vietnam. Countries included in columns (5) to (8) are: India, Malaysia, and Romania. All regressions include country fixed effects. Standard errors clustered on the country-level are reported in parentheses. The mean of the dependent variable is measured among all respondents that were interviewed before January 6, 2021. Panel B uses the household probability weights provided by Gallup.

*** p<0.001, ** p<0.01, * p<0.05.

Table 5.5 Results with probit estimation.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|--|-------------------|-------------------|-------------------|-------------------|---------------------|---------------------|---------------------|---------------------|
| Panel A: Sample average treatment effect (SATE) | | | | | | | | |
| 6 January 2021 | -0.010 (0.026) | -0.005 (0.031) | -0.009 (0.048) | -0.046 (0.052) | | | | |
| 20 January 2021 | | | | | 0.220*** (0.026) | 0.216*** (0.038) | 0.242*** (0.036) | 0.328*** (0.064) |
| Mean of DV | 0.528 | 0.570 | 0.558 | 0.563 | 0.537 | 0.528 | 0.551 | 0.526 |
| Days pre/post | All | 7 | 2 | 1 | All | 7 | 2 | 1 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 4317 | 2476 | 923 | 472 | 2778 | 1393 | 466 | 245 |
| Panel B: Population average treatment effect (PATE) | | | | | | | | |
| 6 January 2021 | -0.034 (0.021) | -0.035 (0.026) | -0.033 (0.039) | -0.083 (0.053) | | | | |
| 20 January 2021 | | | | | 0.184*** (0.038) | 0.168*** (0.040) | 0.194*** (0.057) | 0.243*** (0.072) |
| Mean of DV | 0.527 | 0.578 | 0.570 | 0.581 | 0.522 | 0.527 | 0.552 | 0.565 |
| Days pre/post | All | 7 | 2 | 1 | All | 7 | 2 | 1 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 4317 | 2476 | 923 | 472 | 2778 | 1393 | 466 | 245 |

Notes: Each column within panel A and B is a separate probit regression. Coefficients are average marginal effects. The dependent variable is a dichotomous variable that takes the value 1 if a respondent approves of the job performance of the leadership of the US, and 0 if a respondent disapproves. The independent variable is a dichotomous variable that takes the value 1 if a respondent is surveyed after January 6 or after January 20, 2021, and 0 if a respondent is surveyed before this day. Countries included in columns (1) to (4) are: India, Indonesia, Malaysia, Romania, and Vietnam. Countries included in columns (5) to (8) are: India, Malaysia, and Romania. All regressions include country fixed effects. Standard errors clustered on the country-level are reported in parentheses. The mean of the dependent variable is measured among all respondents that were interviewed before January 6, 2021. Panel B uses the household probability weights provided by Gallup.

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

5.8.8 Result disaggregated by country

In the main text I report all results pooled across countries (with country fixed effects). I choose to do so to economize on space. In tables 5.6 to 5.10 I report the results in each individual country. As can be seen the results are generally the same within India, Indonesia, Malaysia, Romania, and Vietnam. The exception is Romania. In Romania the January 6 insurrection *did* have a significant negative effect on US leadership approval (see columns (1) to (4) in table 5.9). This negative effect in Romania was, however, entirely offset by the positive effect of the replacement of Donald Trump by Joe Biden on January 20, 2021 (see columns (5) to (8) in table 5.9).

Table 5.6 Results in India.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|--|-------------------|-------------------|------------------|-------------------|---------------------|---------------------|---------------------|---------------------|
| Panel A: Sample average treatment effect (SATE) | | | | | | | | |
| 6 January 2021 | 0.021 (0.024) | 0.027 (0.026) | 0.047 (0.042) | 0.001 (0.060) | | | | |
| 20 January 2021 | | | | | 0.212*** (0.042) | 0.220*** (0.045) | 0.253*** (0.056) | 0.315*** (0.059) |
| Mean of DV | 0.587 | 0.581 | 0.581 | 0.591 | 0.608 | 0.600 | 0.654 | 0.648 |
| Days pre/post | All | 7 | 2 | 1 | All | 7 | 2 | 1 |
| N | 2220 | 1629 | 546 | 272 | 1737 | 797 | 234 | 135 |
| Panel B: Population average treatment effect (PATE) | | | | | | | | |
| 6 January 2021 | -0.012 (0.028) | -0.006 (0.031) | 0.021 (0.049) | -0.051 (0.071) | | | | |
| 20 January 2021 | | | | | 0.185** (0.057) | 0.189** (0.059) | 0.249*** (0.071) | 0.242** (0.080) |
| Mean of DV | 0.601 | 0.594 | 0.609 | 0.618 | 0.589 | 0.585 | 0.638 | 0.697 |
| Days pre/post | All | 7 | 2 | 1 | All | 7 | 2 | 1 |
| N | 2220 | 1629 | 546 | 272 | 1737 | 797 | 234 | 135 |

Notes: Each column within panel A and B is a separate OLS regression. The dependent variable is a dichotomous variable that takes the value 1 if a respondent approves of the job performance of the leadership of the US, and 0 if a respondent disapproves. The independent variable is a dichotomous variable that takes the value 1 if a respondent is surveyed after January 6 or January 20, 2021, and 0 if a respondent is surveyed before this day. Robust standard errors in parentheses.

* * * $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table 5.7 Results in Indonesia.

| | (1) | (2) | (3) | (4) |
|--|-------------------|-------------------|-------------------|-------------------|
| Panel A: Sample average treatment effect (SATE) | | | | |
| 6 January 2021 | -0.028 (0.046) | -0.044 (0.060) | -0.112 (0.086) | -0.030 (0.127) |
| Mean of DV | 0.409 | 0.425 | 0.460 | 0.400 |
| Days pre/post | All | 7 | 2 | 1 |
| N | 777 | 266 | 132 | 62 |
| Panel B: Population average treatment effect (PATE) | | | | |
| 6 January 2021 | -0.027 (0.055) | -0.086 (0.073) | -0.106 (0.101) | -0.028 (0.149) |
| Mean of DV | 0.398 | 0.458 | 0.453 | 0.376 |
| Days pre/post | All | 7 | 2 | 1 |
| N | 777 | 266 | 132 | 62 |

Notes: Each column within panel A and B is a separate OLS regression. The dependent variable is a dichotomous variable that takes the value 1 if a respondent approves of the job performance of the leadership of the US, and 0 if a respondent disapproves. The independent variable is a dichotomous variable that takes the value 1 if a respondent is surveyed after January 6 or January 20, 2021, and 0 if a respondent is surveyed before this day. Robust standard errors in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table 5.8 Results in Malaysia.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|--|-------------------|-------------------|-------------------|-------------------|---------------------|--------------------|-------------------|------------------|
| Panel A: Sample average treatment effect (SATE) | | | | | | | | |
| 6 January 2021 | 0.002 (0.043) | -0.022 (0.079) | -0.062 (0.095) | -0.004 (0.123) | | | | |
| 20 January 2021 | | | | | 0.181*** (0.051) | 0.154** (0.056) | 0.206* (0.088) | 0.234 (0.128) |
| Mean of DV | 0.359 | 0.375 | 0.395 | 0.346 | 0.361 | 0.387 | 0.344 | 0.286 |
| Days pre/post | All | 7 | 2 | 1 | All | 7 | 2 | 1 |
| N | 528 | 215 | 112 | 64 | 452 | 324 | 124 | 60 |
| Panel B: Population average treatment effect (PATE) | | | | | | | | |
| 6 January 2021 | -0.009 (0.052) | -0.059 (0.103) | -0.118 (0.120) | -0.038 (0.159) | | | | |
| 20 January 2021 | | | | | 0.120 (0.061) | 0.076 (0.068) | 0.125 (0.103) | 0.075 (0.144) |
| Mean of DV | 0.370 | 0.410 | 0.430 | 0.395 | 0.362 | 0.405 | 0.343 | 0.323 |
| Days pre/post | All | 7 | 2 | 1 | All | 7 | 2 | 1 |
| N | 528 | 215 | 112 | 64 | 452 | 324 | 124 | 60 |

Notes: Each column within panel A and B is a separate OLS regression. The dependent variable is a dichotomous variable that takes the value 1 if a respondent approves of the job performance of the leadership of the US, and 0 if a respondent disapproves. The independent variable is a dichotomous variable that takes the value 1 if a respondent is surveyed after January 6 or January 20, 2021, and 0 if a respondent is surveyed before this day. Robust standard errors in parentheses.

* * * $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table 5.9 Results in Romania.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|--|---------------------|---------------------|--------------------|---------------------|---------------------|---------------------|--------------------|--------------------|
| Panel A: Sample average treatment effect (SATE) | | | | | | | | |
| 6 January 2021 | -0.129** (0.048) | -0.244** (0.079) | -0.235* (0.109) | -0.471** (0.140) | 0.272*** (0.060) | 0.288*** (0.066) | 0.253** (0.093) | 0.416** (0.130) |
| 20 January 2021 | | | | | | | | |
| Mean of DV | 0.542 | 0.659 | 0.650 | 0.789 | 0.413 | 0.397 | 0.441 | 0.357 |
| Days pre/post | All | 7 | 2 | 1 | All | 7 | 2 | 1 |
| N | 511 | 263 | 81 | 41 | 429 | 252 | 108 | 50 |
| Panel B: Population average treatment effect (PATE) | | | | | | | | |
| 6 January 2021 | -0.166** (0.057) | -0.224* (0.095) | -0.231 (0.126) | -0.492** (0.148) | 0.272*** (0.070) | 0.261*** (0.077) | 0.183 (0.115) | 0.389* (0.150) |
| 20 January 2021 | | | | | | | | |
| Mean of DV | 0.576 | 0.618 | 0.595 | 0.784 | 0.410 | 0.421 | 0.478 | 0.375 |
| Days pre/post | All | 7 | 2 | 1 | All | 7 | 2 | 1 |
| N | 511 | 263 | 81 | 41 | 429 | 252 | 108 | 50 |

Notes: Each column within panel A and B is a separate OLS regression. The dependent variable is a dichotomous variable that takes the value 1 if a respondent approves of the job performance of the leadership of the US, and 0 if a respondent disapproves. The independent variable is a dichotomous variable that takes the value 1 if a respondent is surveyed after January 6 or January 20, 2021, and 0 if a respondent is surveyed before this day. Robust standard errors in parentheses.

*** p<0.001, ** p<0.01, * p<0.05.

Table 5.10 Results in Vietnam.

| | (1) | (2) | (3) | (4) |
|--|-------------------|-------------------|------------------|-------------------|
| Panel A: Sample average treatment effect (SATE) | | | | |
| 6 January 2021 | -0.006 (0.089) | 0.017 (0.098) | 0.132 (0.122) | -0.024 (0.142) |
| Mean of DV | 0.815 | 0.793 | 0.677 | 0.833 |
| Days pre/post | All | 7 | 2 | 1 |
| N | 281 | 103 | 52 | 33 |
| Panel B: Population average treatment effect (PATE) | | | | |
| 6 January 2021 | -0.069 (0.151) | -0.003 (0.164) | 0.077 (0.190) | -0.042 (0.205) |
| Mean of DV | 0.804 | 0.739 | 0.658 | 0.778 |
| Days pre/post | All | 7 | 2 | 1 |
| N | 281 | 103 | 52 | 33 |

Notes: Each column within panel A and B is a separate OLS regression. The dependent variable is a dichotomous variable that takes the value 1 if a respondent approves of the job performance of the leadership of the US, and 0 if a respondent disapproves. The independent variable is a dichotomous variable that takes the value 1 if a respondent is surveyed after January 6 or January 20, 2021, and 0 if a respondent is surveyed before this day. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05.

5.8.9 Saliency of insurrection in each country

One explanation for *why* the January 6 insurrection did not cause support for US leadership to decline in India, Indonesia, Malaysia, Romania, and Vietnam is that the general public in these countries was simply insufficiently aware of the event. To consider this possibility I examine news media (section 5.8.9) and Google Trends (section 5.8.9) data from the days surrounding January 6, 2021.

As can be seen in figures 5.2 to 5.11 this data suggests that the January 6 insurrection was prominently covered in the news media of each individual country, and terms related to the insurrection were among the most searched terms on Google in the days following January 6, 2021.

These two pieces of evidence taken together suggest that it is unlikely that support for US leadership did not decline after the January 6 insurrection because of a lack of information.

News media coverage

In figures 5.2 to 5.6 I report screenshots of major news media in India, Indonesia, Malaysia, Romania, and Vietnam on January 6, 7, and 8, 2021. This data comes primarily from the websites of these news media themselves, as well as from Twitter.

Although language barriers, and the fact that many news media do not appear to keep accessible archives of their past issues, make it difficult to provide a complete and fully representative overview of the media coverage during the January 6-8, 2021 period, figures 5.2 to 5.6 do clearly suggest that the January 6 insurrection was widely covered in the news in India, Indonesia, Malaysia, Romania, and Vietnam.

Fig. 5.3 News media coverage in Indonesia.



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**Utusan
Malaysia**

TERKINI NASIONAL EKONOMI LUAR NEGARA GAYA

AMERIKA
LUAR NEGARA
PREMIUM

Demokrasi AS 'calar-balar'



Smårikat - AFP



WASHINGTON: Semalam menjadi hari paling bersejarah dalam politik Amerika Syarikat (AS) apabila pertikaian keputusan pilihan raya presiden tiba ke kemuncaknya selepas penyokong Presiden Donald Trump berusaha dan menverbu bangunan Capitol.

[illegible]

Fig. 5.5 News media coverage in Romania.

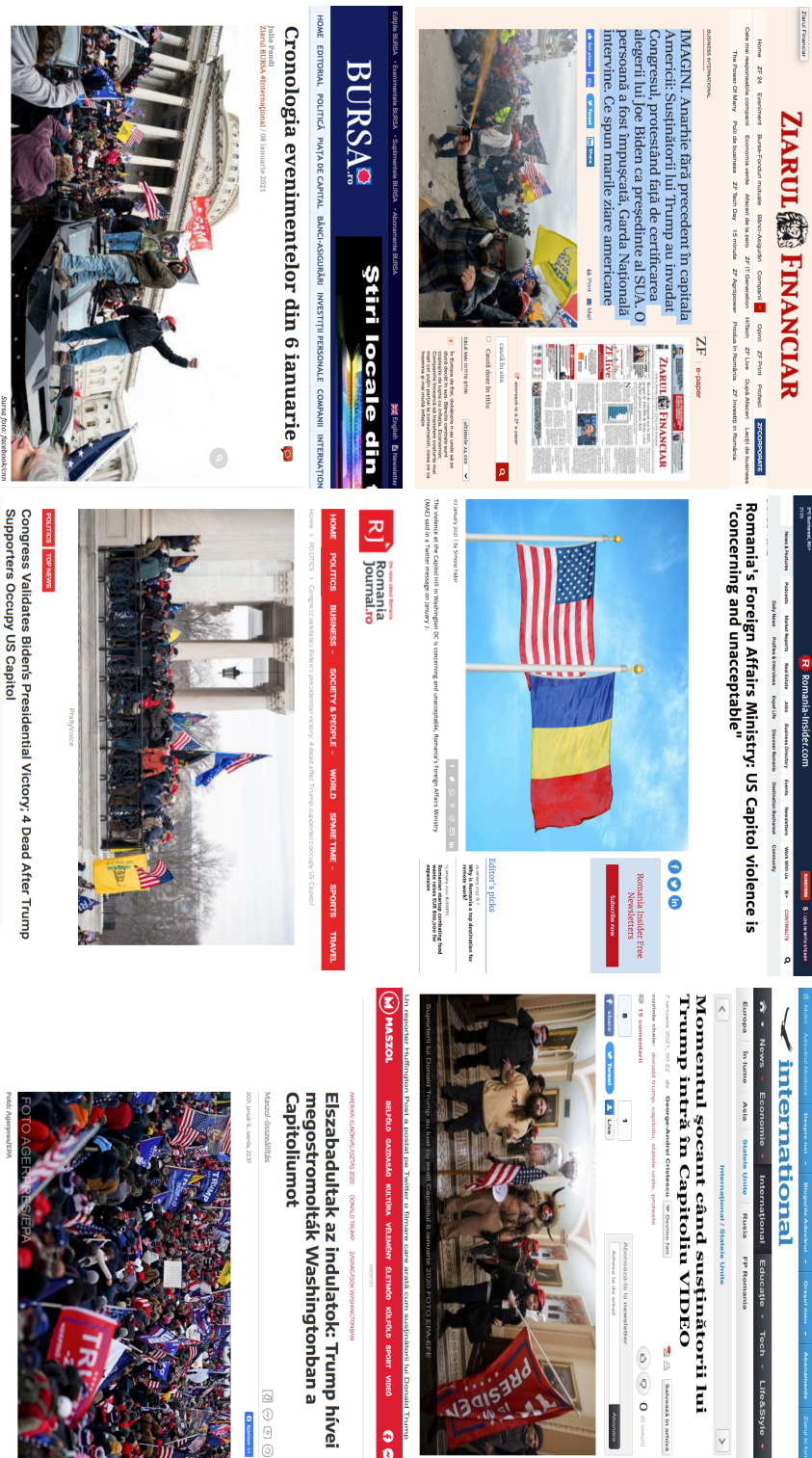
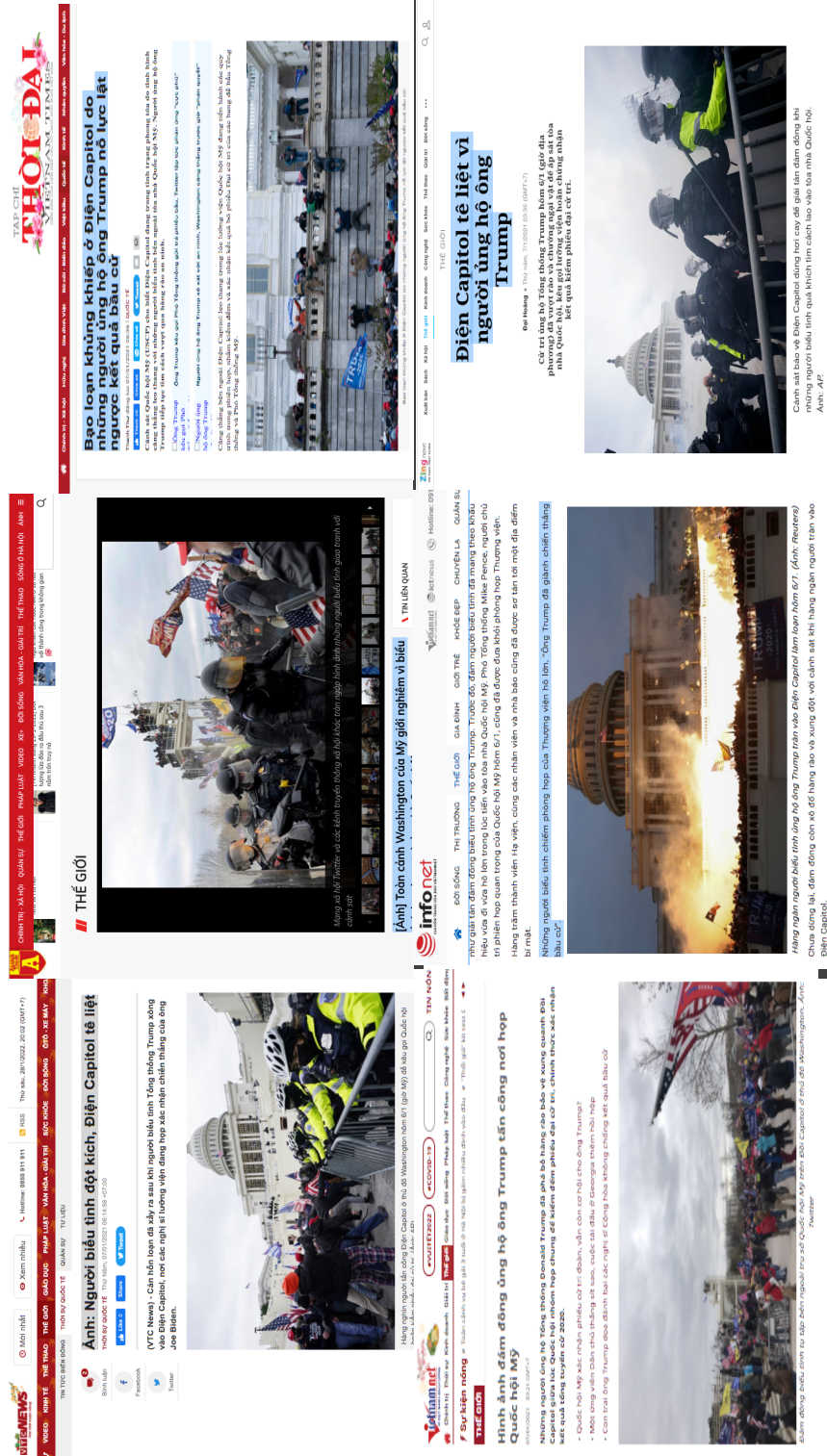


Fig. 5.6 News media coverage in Vietnam.



Searches on Google

In figures 5.7 to 5.11 I examine the search behavior on Google in the days following the January 6 insurrection. Given language differences I assess the popularity of the terms “Capitol” and “Trump”—terms that are clearly related to the January 6 insurrection and that do not tend to get translated in foreign languages—in the weeks before and after January 6, 2021.

The data comes from Google Trends and captures the day-level online search interest for “capitol” and “Trump” relative to all other searches on Google in each country from December 1, 2020 until January 19, 2021 (i.e., the day before the inauguration of Joe Biden). A value of 100 means that the search term was more popular than any other search term on the same day, while a value of 50 means that a search term was used half as often as the most used search term on the same day.

As figures 5.7 and 5.11 show the search term “Trump” became the most searched term, or very close to the most searched term, in all countries directly after January 6, 2021. The search term “capitol” got very close to being the most searched term in India, Indonesia, and Malaysia. In Romania and Vietnam the search term “capitol” was more popular than the bottom 60% and 25% of Google search terms, respectively.

Fig. 5.7 Google searches for “Capitol” and “Trump” in India.

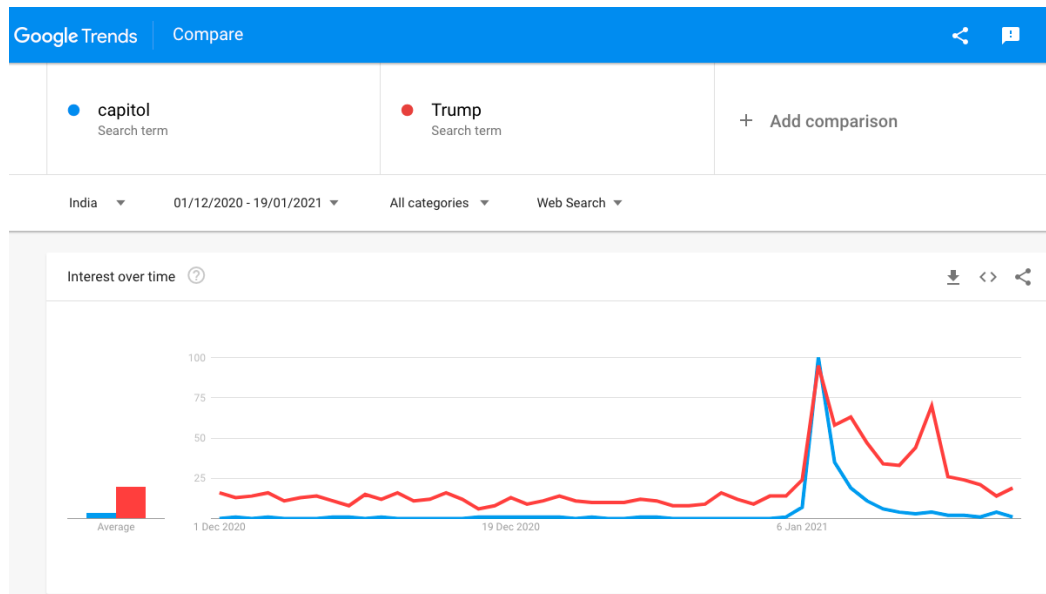


Fig. 5.8 Google searches for “Capitol” and “Trump” in Indonesia.

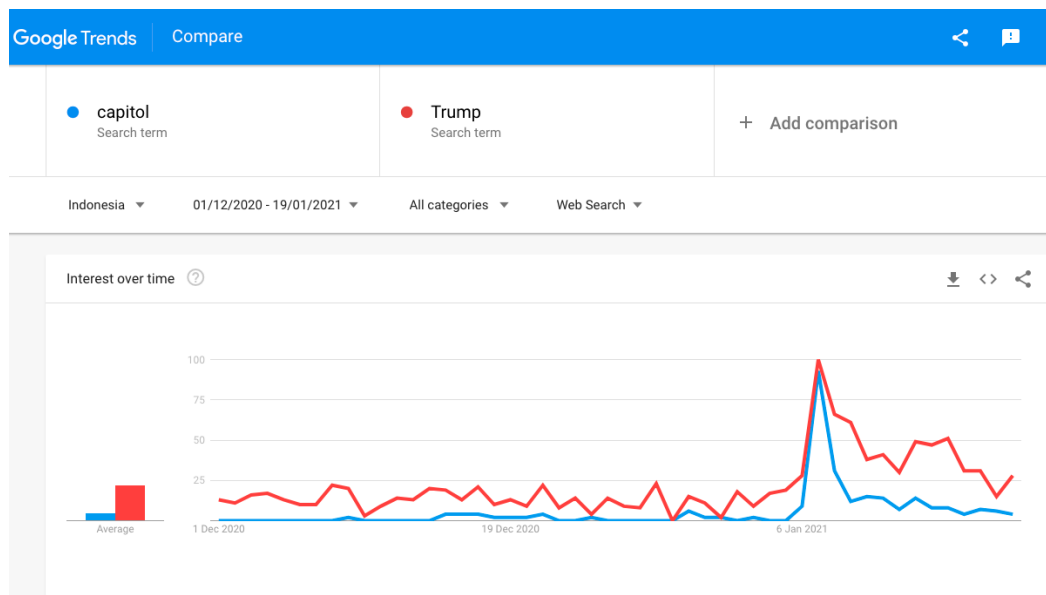


Fig. 5.9 Google searches for “Capitol” and “Trump” in Malaysia.

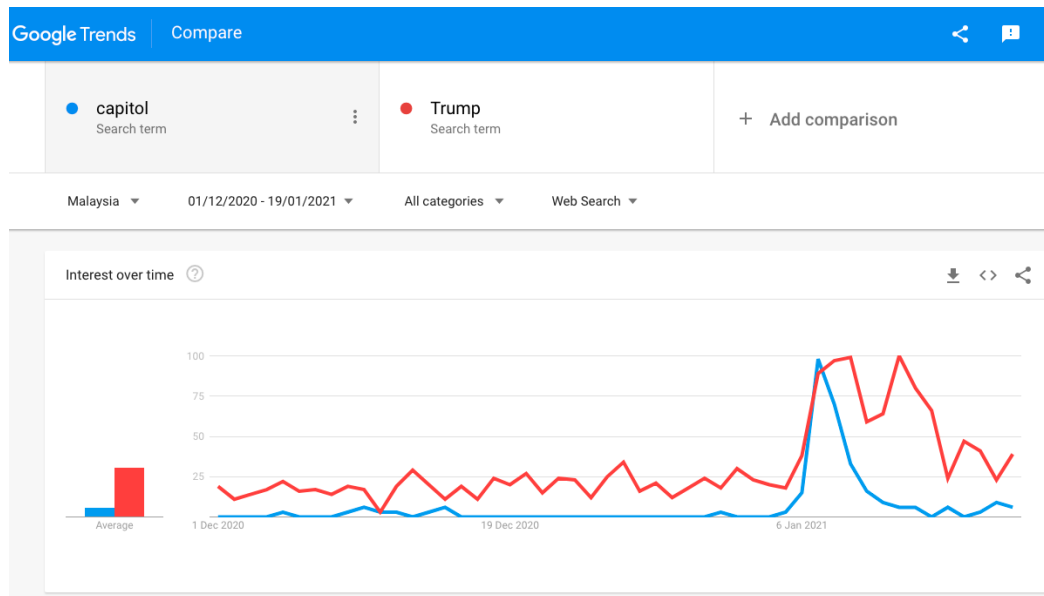


Fig. 5.10 Google searches for “Capitol” and “Trump” in Romania.

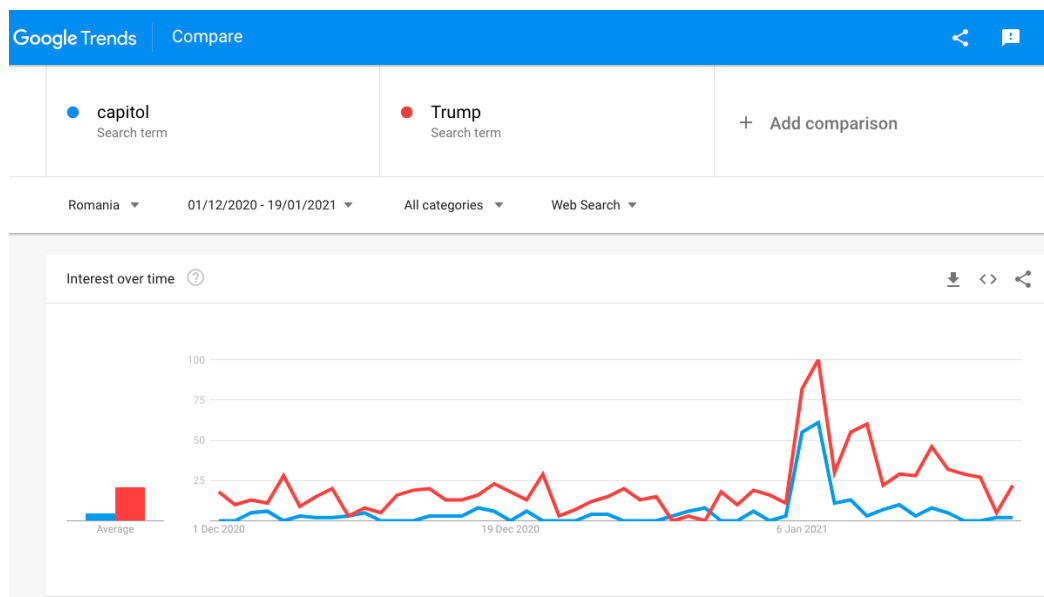
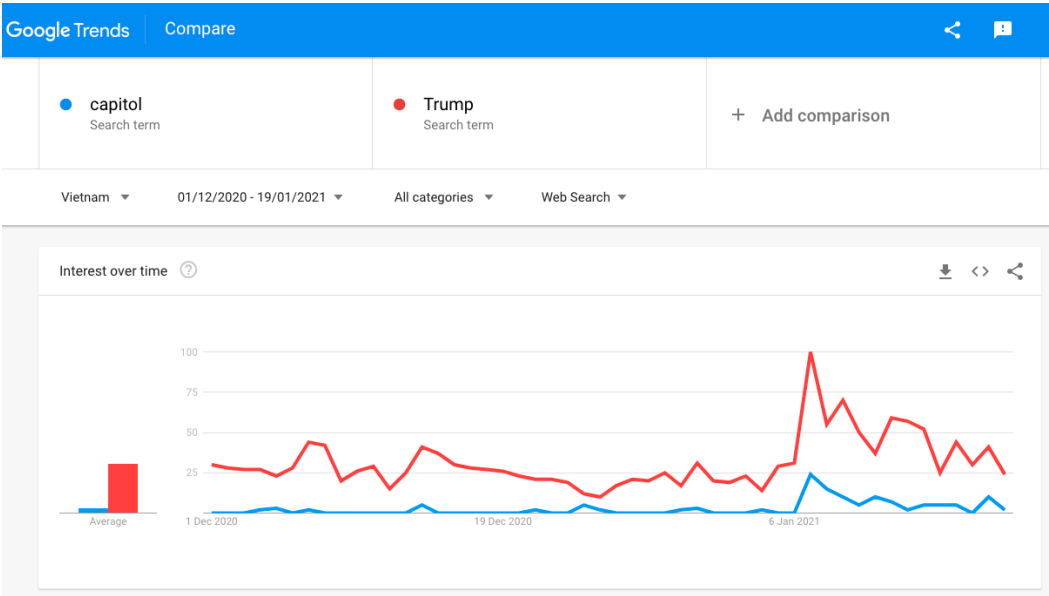


Fig. 5.11 Google searches for “Capitol” and “Trump” in Vietnam.



5.8.10 Attitudes towards U.S. democracy prior to January 6, 2021

Another explanation for *why* the January 6 insurrection did not cause support for US leadership to decline is that people in India, Indonesia, Malaysia, Romania, and Vietnam simply did not have a high opinion of American democracy to begin with.

To examine this possibility I analyze data from multiple sources on how the public in each individual country viewed American democracy before January 6, 2021. This data suggests that people in India, Indonesia, Malaysia, Romania, and Vietnam held on average quite positive opinions of US democracy before January 6, 2021. This therefore makes it unlikely that a lack of appreciation of American democracy prior to the January 6 insurrection explains the fact that support for US leadership was not affected.

India

The Eurasia Group Foundation conducted an online survey among 730 Indian respondents from February 15 until March 3, 2020. Data from this survey suggests that 84% of Indians held positive opinions of “American-style” democracy (Hannah, 2020, p. 20). Evidence from a larger, and potentially more representative sample, is available from wave 5 of the Asianbarometer. This in-person survey was conducted among 4,034 randomly selected Indians in November 2019. The survey asked respondents the following question: “In your opinion how much of a democracy is the United States?” (1 = completely undemocratic; 10 = completely democratic). The average score on this variable was 6.3, with a standard deviation of 2.6.

Indonesia

The Asianbarometer also conducted a nationally-representative survey among 869 Indonesians in April-May 2019. Respondents in Indonesia rated American democracy with a 6.2 out of 10 on average (standard deviation: 1.9).

Malaysia

The Asianbarometer also conducted a nationally-representative survey among 1,151 Malaysians in April-May 2019. Respondents in Malaysia rated American democracy with a 6.4 out of 10 on average (standard deviation: 2.2).

Vietnam

The Asianbarometer also conducted a nationally-representative survey among 1,096 Vietnamese in April-May 2019. Respondents in Vietnam rated American democracy with an 7.5 out of 10 on average (standard deviation: 4.2).

Romania

Data on attitudes towards American democracy is, to the best of my knowledge, not available for Romania. I have assessed the following datasources: European Social Survey, World Value Survey, European Value Survey, International Social Survey Programme, Global Barometer, Eurobarometer, Polling the Nations, Gallup, and Pew Research.

Chapter 6

Conclusion

This dissertation has shown that (1) a large share of employment in manufacturing (i.e., industrialization) tends to make democratic forms of government more likely, (2) that relatively few American voters have withdrawn their support from Donald Trump and the Republican Party as a result of the January 6 insurrection at the U.S. Capitol, and (3) that this same January 6 insurrection had little to no effect on support for U.S. leadership in India, Indonesia, Malaysia, Romania, and Vietnam.

The dissertation has left several important questions open for future research.

In support of the hypothesis that industrialization tends to induce democracy I have shown that industrialization is strongly correlated with democracy across 145 countries and over 170 years (1845-2015), and that at least in the specific case of Norway industrialization appears to have caused democratization. While being an important first step this evidence is still too preliminary to unconditionally accept the hypothesis that industrialization tends to induce democracy. Future research is necessary to discover more quasi/natural experiments that can be used to rigorously test the effect of industrialization on democracy. In ongoing work outside of this dissertation I am seeking to test my theory in 19th- and early 20th-century Switzerland, which also, like Norway, industrialized primarily based on hydropower. As of now I have unfortunately not yet discovered credible identification strategies to test the effect

of manufacturing employment in more recent cases (e.g., Spain, South Korea, and Taiwan).

I have focused on identifying the average effect of manufacturing employment on democracy. There exists, however, significant heterogeneity in the nature of production and employment *within* the manufacturing sector. Future research may therefore fruitfully contribute by studying whether and how the average effect of industrialization on democracy that I recover varies across specific types of (de)industrialization (e.g., state versus market-led industrialization, export-oriented versus import-substitution industrialization, premature versus mature deindustrialization) and/or across different forms of manufacturing (e.g., high- versus low-tech, heavy- versus light industry). Also important for future research is whether it matters for the effect on democracy whether a particular increase in manufacturing employment is “efficient” from an economic point of view, or whether it is, for example, forced at great socio-economic costs through government policy.

Future research can also fruitfully contribute by further examining the causal mechanisms that mediate the effect of manufacturing employment on democracy. I have shown that industrialization strongly increases the probability of mass revolts by industrial workers, that such revolts are strongly associated with democratization, and that accounting for such revolts on the right-hand side of a regression of democracy on industrialization accounts for a significant portion of the effect of industrialization. I also show that structural socio-economic changes, such as increases in income, equality, urbanization, and education, are unlikely to mediate the effect of industrialization on democracy because all currently highly industrialized countries in fact industrialized and democratized decades before reaching high levels of income, equality, urbanization, and education. Ideally we would, however, have a much more detailed understanding of how important the bottom-up mechanism that I highlight is as compared to alternative

causal mechanisms, such as Llavador and Oxoby's (2005) mechanism of industrial capitalists extending the suffrage to industrial workers without any pressure from below. In case several distinct mechanisms mediate the effect of industrialization on democracy, we would like to know more about the conditions that determine why one mechanism predominates over another in a specific context.

Important research also remains to be done regarding the future of manufacturing employment and democracy. Of particular importance is research that examines whether the nature of manufacturing employment is changing in ways that make industrialization more or less likely to induce democracy in the future. While a medium-sized literature in development economics exists on what consequences changes in manufacturing technologies have for economic development (e.g., Rodrik (2014), McMillan and Rodrik (2011), and Haraguchi, Cheng and Smeets (2017)), no existing research, to the best of my knowledge, exists on the potential political consequences of changes in the nature of manufacturing employment.¹

The results of the dissertation also strongly underline the need to better understand the causes of manufacturing employment. A specific focus on the drivers of increases in manufacturing employment is, to the best of my knowledge, currently lacking because the development economics literature tends to be preoccupied with explaining manufacturing output (rather than employment), while the political science literature rarely takes a sectoral view of the economy, and at most, focuses on the potentially negative effect of mining (i.e., the "political resource curse" – Ross (2001, 2015)), rather than the causes and effects of manufacturing employment.

The limitations of papers III and IV are primarily related to external validity. Crucial questions for future research are how American voters and foreign publics would react to other type of anti-democratic treatments, outside of the January 6 insurrection at the U.S. Capitol. Important is also to understand whether the effect

¹A partial exception is Boix (2019).

would be different if a number of January 6-like treatments would occur in a short period of time. Important for the results of paper IV is also how publics of other countries outside of India, Indonesia, Malaysia, Romania, and Vietnam have reacted to the January 6 insurrection event. Theoretically, contributions can still be made in explaining *why* the domestic and international reactions to the January 6 insurrection at the U.S. Capitol were relatively modest and short-lasting.

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