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Social Mobility Explains Populism, Not Inequality or Culture

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Social Mobility Explains Populism, Not Inequality or Culture

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What explains contemporary developed-world populism? A largely-overlooked hypothesis, advanced herein, is economic unfairness. This idea holds that humans do not simply care about the magnitudes of final outcomes such as losses or inequalities. They care deeply about whether each individual's economic outcomes occur for fair reasons. Thus citizens turn to populism when they do not get the economic opportunities and outcomes they think they fairly deserve. A series of cross-sectional regressions show that low social mobility – an important type of economic unfairness – consistently correlates with the geography of populism, both within and across developed countries. Conversely, income and wealth inequality do not; and neither do the prominent cultural hypotheses of immigrant stocks, social media use, nor the share of seniors in the population. Collectively, this evidence underlines the importance of economic fairness, and suggests that academics and policymakers should pay greater attention to normative, moral questions about the economy.

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I. Introduction

The developed world is at a critical junction in the path of history. A wave of populist politics has grown since approximately the 1980s and 1990s (Colantone and Stanig 2019, Golder 2016), and now afflicts countries as diverse as the US, the UK, France, Italy, Spain, Greece, and the Czech Republic. A number of these nations have served as important beacons of freedom for the global community, particularly since 1945. But now the realities of populist movements are threatening long-standing democratic institutions and practices. Less-developed countries that elected populists earlier on, like Turkey and Hungary, have rapidly descended into authoritarianism – a trajectory that is suggestive of the risk populism poses broadly, if not confronted successfully. Understanding the roots of contemporary populism is therefore vital to the survival of liberal democracy.

Explanations for the rise of populism are broadly divided into two schools of thought, one cultural and one economic. The former school generally contends that factors such as rising immigration (Kaufmann 2018), heavy social media use (Sunstein 2018), and a "cultural backlash" of older against younger generations (Norris and Inglehart 2019) have led to growing intolerance of liberal globalist values in key segments of the electorate. The latter highlights the way policy choices in addition to shocks such as those from trade (Autor et al. 2016) and the Global Financial Crisis (Tooze 2018) have created economic pain for too many voters.

A dominant perspective in the economic school of thought, which this paper especially seeks to challenge, is the salience of unequal economic outcomes as a crucial channel leading to populism. Norris and Inglehart (2016) call "the economic inequality perspective" the "most widely-held view of mass support for populism," and in Norris and Inglehart (2017) argue that "decades of declining real income and rising inequality have produced a long-term period effect conducive to the populist vote." Guriev (2018) observes that "the traditional view of populism links it to inequality and redistribution." Guriev and Papaioannou (2020) review the literature on causes of populism, and note how economic shocks from trade and automation lead to unequal outcomes.

There is a nascent argument in the literature that the simultaneous rise of income inequality and contemporary developed-world populism has been vastly overinterpreted. Instead, *unfair* economic outcomes which violate desert-based distributional justice – or "reward according to contribution" (Debove, Baumard, and André 2017) – matter considerably more. Under this perspective, unequal economic outcomes can be produced for either fair or unfair reasons (e.g. someone works hard or innovates vs. someone steals or engages in nepotism), and on the whole humans prefer fair outcomes regardless of whether they happen to be equal or unequal. Rodrik (2018) theorizes that "not inequality per se, but perceived unfairness" explains the political salience of job losses from trade, where workers lose jobs to competitors in foreign countries that do not play by the same rules.

Guriev (2018) contends that "unfair inequality" determined by uncontrollable characteristics may be more closely associated with populism than "fair inequality" earned via skill and effort. There are good reasons to take this perspective seriously; Starmans, Sheskin, and Bloom (2017) review the behavioral science literature on attitudes towards inequality and convincingly argue that "there is no evidence that people are bothered by economic inequality itself. Rather, they are bothered by something that is often confounded with inequality: economic unfairness."

This paper presents empirical evidence to support the contention that *unfair* economic outcomes are linked to the rise of contemporary developed-world populism. It uses cross-sectional regression analysis to explore the correlation between low social mobility (specifically, intergenerational income elasticity), an indicator of unfair economic outcomes, and the geography of populism in several settings. Intuitively, in places with low social mobility economic outcomes are strongly influenced by parental wealth – a clear violation of reward according to contribution, and thus of fairness.

The regression results show that low social mobility consistently correlates with the county-level vote swing towards Trump in the 2016 and 2020 US elections versus the 2012 Republican Presidential vote share; the department-level vote share for Le Pen in the second round of the 2017 French Presidential Election; the country-level vote share for populist and far-right parties in the 2019 EU Parliamentary Election; and country-level dissatisfaction with national government

in 2016 (a proxy for populist discontent, previously studied by Askoy, Guriev, and Treisman 2018 in the context of the populist backlash against globalization) across the developed world broadly.

Not only do income and wealth inequality fail to display this relationship with populism, but neither do prominent cultural hypotheses: the share of immigrants in the population, the share of seniors in the population (who may undergo a "cultural backlash"), and the rate of active social media use. This contrast especially highlights the salience of economic unfairness as a correlate of populism.

Collectively, these results suggest that common policy prescriptions to combat populism may be misguided. For example, platforms for aggressive redistribution may be likelier to repulse rather than attract would-be populist voters, because enforced equal outcomes can break rather than remedy the relationship between contribution and fair reward. Indeed, Alesina, Stancheva, and Teso (2018) show that right-wing survey respondents with low trust in government do not see redistribution as a good solution for low social mobility.

The remainder of this paper is organized as follows. Section II reviews the literature on populism and economic unfairness. Section III details this paper's empirical strategy. Section IV presents results, Section V discusses findings, and Section VI concludes.

II. Literature Review

This section first briefly reviews definitions of populism. It then reviews the literature on two especially relevant questions for this paper: what is economic unfairness, and how is it plausibly linked to populism? Readers interested in a full review of the possible causes of populism, which is beyond the scope of this paper, are directed to Guriev and Papaioannou (2020).

There are numerous prominent definitions of populism in the literature which do not fully agree. Müller (2016) characterizes populism as "a way of perceiving the political world that sets a morally pure and fully unified [but ultimately fictional] people against elites who are deemed corrupt or in some other way morally inferior… In addition to being anti-elitist, populists are always anti-pluralist: populists claim that they, and only they, represent the people." Norris and Inglehart (2019) concur that populism "challenges the authority of establishment elites," but also emphasize additional elements such as its tendency towards authoritarianism and xenophobia. Eichengreen (2018) similarly highlights the authoritarian aspects of populism. Guiso et al. (2017) make an additional distinction by arguing that populists are short-termist, offering benefits for the near future but eschewing the long-term consequences of their actions. Guriev and Papaioannou (2020) review numerous definitions of populism, and contend that the "lowest common

denominator" of features common to such definitions are anti-elitism and anti-pluralism.

Thus there is not full agreement on the definition of populism, but there are salient features which certainly suggest a common trend across numerous developed countries. This paper proceeds by either analyzing widely agreed-upon examples of populists (Donald Trump in the US and Marine Le Pen in France); or, in the case of the 2019 European Parliament Elections, using an existing classification scheme to examine populist and far-right parties (and, in a robustness check, only populist parties).

"Morality-As-Cooperation" (Curry, Mullins, and Whitehouse 2019) is a useful framework to understand fair economic outcomes as discussed in this paper, and why they are distinct from unequal economic outcomes. This theory holds that human morality consists of biologically and culturally evolved rules that enable complex cooperation. Rawls (1971) writes that "the circumstances of justice may be described as the normal conditions under which human cooperation is both possible and necessary." Rai and Fiske (2011) explain that "morality functions to facilitate the generation and maintenance of long-term social-cooperative relationships with others." Tomasello and Vaish (2013) argue that "human morality arose evolutionarily as a set of skills and motives for cooperating with others." Greene (2015) puts this forcefully when he writes that "the core function of morality is to promote and sustain cooperation." Curry (2016) explicitly identifies

that "morality turns out to be a collection of biological and cultural solutions to the problems of cooperation and conflict."

Economic fairness, then, is one key element of human morality that has evolved to optimize cooperation. In approximate, intuitive terms, economic fairness as described here entails a desert-based formulation of distributional justice – or "reward according to contribution" (Debove, Baumard, and André 2017). That is, the rewards from cooperation should be chiefly divided according to each agent's contribution (i.e. marginal productivity). Agents who attempt to extract rewards according to some other standard that is not intrinsically related to productivity (for example ethnicity, social class, or political power), or who extract rewards in a way that is injurious to other agents (for example theft, corruption, or rent-seeking), are generally punished as 'cheaters.'

Numerous authors have proposed technically detailed formulations of economic fairness and how it evolved; Debove (2015), for instance, reviews thirty-six such explanations. Starmans, Sheskin, and Bloom (2017) suggest some possible intuition behind these findings: "When individuals can choose the people with whom they interact for mutually beneficial tasks, cooperative individuals gain benefits from being included and selfish individuals lose out on those benefits by being shunned. But individuals who are too cooperative – too generous – run the risk of being taken advantage of by others. So a balance must be struck. To treat everyone equally would entail penalization of more productive individuals when they collaborate

with less productive individuals relative to highly productive individuals. In contrast with equality, fairness allows individuals with different levels of productivity to share the benefits of their collaboration proportionately."

Naturally, economic fairness coexists and competes with other values and behaviors that may enhance survival depending on specific circumstances. For example, some societies are xenophobic, and do not include outside ethnic groups in the collection of agents with which cooperation is possible. Solidarity, on the other hand, entails that some resources go towards members of society who cannot fully support themselves, even if they do not contribute much to cooperative production. The point here is not to describe a universal maxim that all humans perceive to be moral, but to describe one specific family of moral-economic rules that is arguably widespread because it has been promoted in an evolutionary process.

There is strong evidence that humans have in fact evolved to value economic fairness, in the sense of reward according to contribution, highly. Starmans, Sheskin and Bloom (2017) review the behavioral science literature on inequality to demonstrate that "there is no evidence that people are bothered by economic inequality itself. Rather, they are bothered by something that is often confounded with inequality: economic unfairness." They consider, among other results, two well-known findings: that of the ultimatum game, where people often reject arbitrarily-chosen reward distributions that are highly unequal; and that of Norton

and Ariely (2011), where people indicate that their ideal societal income distribution is decidedly *unequal*. While the former result would suggest at first glance that people are averse to inequality, the latter indicates otherwise. Starmans et al. reconcile the two findings through the lens of fairness, pointing out that in the ultimatum game nobody has done anything to earn a higher reward than anyone else. Thus in that particular circumstance unequal rewards are perceived to be unfair, but the generalizable principle is that people are averse to unfairness. Indeed, the authors show that in numerous experiments people consistently want to accord higher rewards to those who have exerted more effort – as that is the fair, albeit necessarily unequal, outcome. Children and even infants also exhibit this value, which indicates that economic fairness is in part an evolved instinct. When people consider society broadly, as in Norton and Ariely (2011), they correspondingly tend to idealize an unequal income distribution. The most plausible explanation is that a degree of economic inequality is seen to be the *fair* result of differences in productivity and contribution.

Aversion to economic unfairness in this sense appears to be so foundational to the human condition that it is even found in our biological relatives. Brosnan and De Waal (2003) study reactions to reward systems among capuchin monkeys, showing that "monkeys refused to participate if they witnessed a [peer] obtain a more attractive reward for equal effort, an effect amplified if the partner received such a reward without any effort at all." Comparable results have been found in

child and infant studies. Sloane and Baillargeon (2012) demonstrate that babies expect resources to be allocated towards the people who have done the most work. Starmans et al. (2017) highlight how six-year old children similarly prefer to give more resources to people who have done more work, and will choose this even when given the option of distributing rewards equally. These findings imply that biological evolution has deeply promoted economic fairness as a core instinct in the human lineage.

There is also evidence that economic fairness has been promoted through cultural evolution. Henrich et al. (2011) collect data and run the ultimatum game in fifteen different populations around the globe, which variously survive by foraging, fishing, hunting, horticulture, pastoralism, farming, and wage work. They find that "market integration (measured as the percentage of purchased calories) positively covaries with fairness." That is, individuals in societies which prefer to allocate economic rewards according to contribution obtain more food from market exchange. Of course, market integration is foundational to societal growth in an evolutionary sense; the authors contend that "larger and more-complex societies prospered and *spread* to the degree that their norms and institutions effectively sustained successful interaction in ever-widening socioeconomic spheres" [emphasis added]. If economic fairness is a critical input for cultural evolutionary success, that may help explain why it has spread widely across humanity.

It is thus eminently plausible that, on the whole, citizens of advanced market democracies care deeply about fair economic outcomes such that reward should be a function of contribution. There can always be individuals and societies that reject fairness, but they arguably constitute evolutionary dead ends.

What's more, there is empirical research showing that economic unfairness is associated with political discontent and contemporary populism. To begin, there is general evidence linking unfair economic outcomes to political dissatisfaction with the status quo. Guriev (2017) statistically decomposes income inequality into "unfair" and "fair" components in post-Soviet states. The "unfair" portion of income inequality is attributable to uncontrollable characteristics such as ethnicity, gender, and parental wealth, and the "fair" portion is what remains. While *unfair* inequality is associated with lower support for capitalism and democracy, once it is controlled for *fair* inequality is in fact associated with higher support. Guriev (2018) emphasizes that this is unsurprising given that the previous Soviet economies imposed "unfair equality."

In the context of developed countries, there is good reason to think that some of the major economic shocks associated with the rise of populism in fact acted through the channel of unfair economic outcomes. First, consider the impact of the 2008-2009 Global Financial Crisis (GFC), an immensely disruptive event that has been linked to the rise of populism by authors like Tooze (2018). On the face of it, it seems obvious that the large losses imposed by the GFC would contribute to

political discontent. But deeper insight is provided by Funke, Schularick, and Trebesch (2016). The authors not only show that financial crises have resulted in 30% more support for far-right parties among developed countries from 1870 – 2014; but critically, that non-financial macroeconomic disasters which create losses of the same magnitudes *have no such effect*. Funke et al. suggest that this may be because financial crises are seen as the "inexcusable" result of a self-serving financial elite that has put its own interests above those of broader society. This indicates that people do not simply care about the size of the loss they incur, but the reason behind it – that is, they care about how unfair the loss was.

Second, consider the fallout of job losses resultant from globalization, popularized as the "China Shock." Autor et al. (2016) show that trade shocks from import competition with China resulted in increased local political polarization in the US, while Colantone and Stanig (2018) repeat the exercise for the UK in the context of Brexit. To explore the causal link between trade shocks and populism, Rodrik (2018) asks "why trade gets picked on so much by populists both on the right and the left. After all, imports are only one source of churn in labor markets, and typically not even the most important source." His answer is that "it's one thing to lose your job to someone who competes under the same rules as you do. It's a different thing when you lose your job to someone who takes advantage of lax labor, environmental, tax, or safety standards in other countries… What arouses popular opposition… is perceived unfairness." Once more, it is not just the size of

the economic loss that matters, but whether the loss was perceived to be especially unfair.

Of course, these two events are not panaceas that explain the totality of the contemporary populist wave. The rise of populism can be traced to well before the GFC, and many countries that trade heavily with China have not been substantially disrupted by populism. Acemoglu et al. (2016) find that 9.7% of US manufacturing job losses from 1999 – 2011 were attributable to import competition with China, while Balsvik, Jensen, and Salvanes (2014) show that approximately 10% of Norwegian manufacturing job losses 1996 – 2007 were caused by the China Shock; and Murray (2017) finds the proportion from 2001 – 2011 in Canada was 20.7%. Yet there is no Norwegian nor Canadian Trump.

But this does not mean these hypotheses are wrong per se, and that people do not care about unfairness. It simply indicates that certain societies are especially *vulnerable* to the unfair economic outcomes that can result from these kinds of shocks. That is, final outcomes are determined not only by shocks but also by absorbers. Whereas Acemoglu et al. (2016) find no statistically significant evidence of job recovery following the China Shock in the US, Murray (2017) finds that 60% of jobs lost in Canada due to the China Shock were recovered in the non-tradeable sector. Eriksson et al. (2019) further investigate why the China Shock in particular had such a strong impact in the context of the modern US economy as opposed to earlier trade shocks spanning the previous century. They find that a crucial factor

was the economic environment of the place that was shocked – specifically, that places characterized by industrial decline, low education, and high wages have been the most vulnerable. This suggests that the incumbent policy regime critically determines whether shocks translate to unfair economic outcomes (and consequently, populism). Indeed, *The Economist*, which reviewed Eriksson et al. (2019), commented that "it may be tempting to conclude that America has paid too high a price for China's entry into the global trading system… A more helpful conclusion is that politicians should take more care to equip workers labouring far from the innovation frontier to adapt to shocks in their industries – from import competition or anywhere else."

In fact, there is evidence that economic outcomes in the United States, at least, are becoming more unfair broadly, irrespective of any specific shocks. Hufe, Kanbur, and Piechl (2018) decompose the growth of US income inequality into an "unfair" component explained by uncontrollable demographic variables – including gender, ethnicity, parental income, and parental occupation – and a "fair" remainder, in a similar way as Guriev (2017). They show that the growth of US inequality was largely "fair" before the 1990s, but "unfair" thereafter. That is, from the 1990s onwards uncontrollable characteristics became more decisive determinants of economic outcomes in the US.

III. Empirical Strategy, Specifications, and Data Description

This paper uses low social mobility to investigate the correlation between unfair economic outcomes and the geography of contemporary developed-world populism. Specifically, social mobility here refers to what is known in the literature as intergenerational income elasticity. This takes a particular geography, like a municipality, province, or country, and uses tax return data to examine the correlation between each individual's income and the income of their parents when they were the same age. In places with high social mobility an individual's economic outcomes are not very dependent on how wealthy their parents were. Conversely, in places with low social mobility an individual's economic success strongly depends on how wealthy their parents were. This latter situation clearly violates economic fairness (as described in the literature review), because rewards are allocated according to a standard that is not intrinsically linked to an individual's potential productivity.

Low social mobility is arguably a useful variable to analyze because it reflects a place's vulnerability to unfair economic outcomes in a way that does not depend on any specific shocks. For instance, it may plausibly be difficult to consistently link the China Shock to the cross-country geography of populism because final economic outcomes depend not just on shocks but also absorbers (as highlighted by Eriksson et al. 2019). Conversely, a place with low social mobility exhibits, in

an important way, a *general* pattern of unfair economic difficulty for those in disadvantaged circumstances. Low social mobility does not just examine the incidence of a *shock*, which has to filter through absorbers, but instead broadly captures unfair *outcomes*.

There are several reasons why, in principle, one might argue against relating social mobility to populism. For one, some authors contend that social mobility only changes very slowly. Hertz (2007) and Lee and Solon (2009) show that US social mobility did not change substantially over 1977 – 2000, while Chetty et al. (2014) demonstrate the same pattern extending to the 2010s. The OECD (2018) finds that social mobility for people born to low-education parents after 1975 has generally been stagnant among its member countries. Importantly, however, there is not consensus on this position. Carr and Wiemers (2016) show that the chance of an individual jumping from the middle to the top of the US income distribution has declined since the 1980s, while Hufe, Kanbur, and Piechl (2018) demonstrate that parental economic status has become a substantially stronger predictor of an American's income since the 1970s. Markussen and Roed (2020) also find that Norwegian social mobility has declined over the last century. Of course, even if social mobility changes slowly it is also possible to imagine a different sort of relationship where a persistently low level of social mobility induces a change in political results. Finally, one might hypothesize that a policy regime of low social mobility ought to be more vulnerable to the unfair outcomes that can result from a

procession of economic shocks. The same barriers that hold back somebody born into disadvantaged circumstances could plausibly also affect someone who has just lost a job or a home. Thus there are a number of reasons to think that the timeframes of low social mobility and the populist wave are compatible.

One might also object that social mobility and income inequality are, in practice, so closely related that the former variable cannot provide any new insight. The Great Gatsby Curve, as seen in Corak (2013), for example shows that social mobility and income inequality are correlated across countries. The two variables are of course causally related to one another in some sense; Bénabou (2017) describes the chief mechanisms by which income inequality lowers social mobility. But social mobility is also a function of a wide variety of other factors, and the claim that it is indistinguishable from income inequality is not borne out empirically. Chetty and Hendren (2016) show that income inequality is just one of several important factors that influence social mobility in local US labor markets, while Connoly, Corak, and Haeck (2019) show that in both American and Canadian local labor markets the two variables are positively correlated but with substantial residuals.

Alternatively, one might contend that high social mobility means both upwards and downwards mobility, and while people adore the former they surely detest the latter. What that perspective fails to recognize is the importance of fairness. People do not only care about the magnitudes of their economic gains and losses; as

emphasized, they also care about the fairness of how those gains and losses occur. When someone is unable to get ahead regardless of their talent and effort – say because their family background prevents them from accessing good education or healthcare – that is an obvious source of anger. But if someone is downwardly mobile in a highly socially mobile society where outcomes are earned – say because they didn't study hard enough in school – that is their responsibility. It seems far less plausible that this latter situation would generate mass discontent, as there is no obvious third party to blame.

This paper thus uses cross-sectional regression analysis to determine how the geography of populism correlates with social mobility versus competing explanations that are not directly related to economic unfairness: income inequality, immigration, social media use, and the presence of older generations who may undergo a "cultural backlash". Of course, this kind of analysis cannot establish definitive causality, and makes no pretense of doing so. It simply shows that there are repeated, compelling correlations *consistent* with the hypothesis that the economic causes of populism more plausibly act through generating unfair rather than unequal outcomes.

The analysis examines two national and two international settings, using data where available to variously address the aforesaid theories explaining populism. In all cases it addresses social mobility, income inequality, and immigration. Due to data availability it only examines social media at the international level. It does not

examine separate election events together due to the inherent incompatibility of different national political frameworks. It carefully applies controls using available data to take into account possible factors that are not directly tied to economic unfairness, but to also avoid variables that plausibly interact strongly with both populism and social mobility.

The four settings examined are as follows: first, US counties in the 2016 and 2020 Presidential Elections, where populism is measured as the vote swing towards Trump relative to 2012 levels of Republican support. This context is advantageous because it can be leveraged for a large number of datapoints. Second, French departments in the second round of the 2017 Presidential Election, where populism is measured as the vote share for Le Pen. The data here is more limited, but the results are still suggestive. Third, the 2019 European Parliament elections, where populism is measured in terms of the vote share for parties classified as populist or far-right. This setting is useful because it is one of the only valid examples of internationally-comparable election results. Fourth, developed countries across the world; although election results are not directly comparable across these countries, populist sentiment is proxied for with surveyed confidence in government, a measure of discontent with the political status quo.

Various robustness checks additionally show the results are not sensitive to arbitrary choices like the use of particular datasets or definitions. The regression specifications and data sources used in each of these contexts are described below.

All specifications transform the data by demeaning each variable and dividing by its standard deviation, for ease of interpretability of the regression results.

A. US Presidential Elections

A cross-sectional OLS regression framework is used:

$$(1) \qquad Trump_i = \beta_0 + \beta_1 H_i + \gamma W_i + \varepsilon_i$$

Where i subscripts each US county; $Trump_i$ measures the change in the Republican Presidential vote share from 2012 to the election year in question (a standard approach for examining support for Trump, seen for example in Broz, Frieden, and Weymouth 2019); H_i represents a hypothesis to explain populism (each of which is considered one at a time to avoid potential multicollinearity); W_i is a vector of controls; and ε_i is the error term. The hypotheses considered are social mobility as measured by intergenerational income elasticity, the Gini coefficient for income inequality, the share of immigrants in the population, and the share of people aged at least 65 in the population. The controls for 2016 consist of the percent Republican Presidential vote share in 2012, log income per capita, the percentage of the county that is ethnically white, the percentage of the county that is religious, and log population density. The 2020 controls are identical but with

the addition of COVID-19 deaths per capita up to the election date. In robustness checks alternative hypotheses are considered together with social mobility, one at a time, to ensure that their inclusion does not alter the latter's significance. In a sanity check, the main specification is used to instead analyze the vote swing towards Mitt Romney in 2012 vs. 2008.

Some controls that plausibly interact with both social mobility and populism, such as educational attainment, are deliberately not included here to avoid the bad control problem. That is not to say such variables were necessarily unimportant factors in Trump's election; but simply that their inclusion here would obscure the main research question.

The change in the Republican Presidential vote share versus 2012 is used to analyze Trump's election results because votes in his favor were undoubtedly influenced by baseline support levels for the Republican Party. By looking at this change we can better identify factors associated with support for Trump specifically. In the main specification the absolute change in the vote share from is examined. For example, if a hypothetical county voted 10% Republican in 2012 and 15% Republican in 2016 this would be treated as a 5% change. In a robustness check the percentage change in levels is used, where the same county's outcome would be treated as a 50% change.

Intergenerational income elasticity is taken from Chetty (2014). The rank-rank slope is used, which conceptually corresponds to the correlation between parent and

child income at the same age after life cycle corrections (for example, if the child is still in university at a certain age). Income inequality data is also drawn from Chetty (2014). See Chetty (2014) for further technical details. Only 2769 of all 3242 American counties are covered by Chetty (2014), somewhat restricting the scope of analysis. However, those counties with missing data tend to be those with the least inhabitants.

Pre-2020 voting data is drawn from the MIT Election and Data Science Lab. 2020 voting data was scraped from the New York Times' 2020 election result reporting. Data for the percentage of the county that is white, population density, income per person, the population share over 65, and the share of immigrants in the population are from Chetty (2016), while religiosity by county is from Chetty et al. (2016). COVID-19 deaths per capita are from Dong, Du, and Gardner (2020).

B. 2017 French Presidential Election

A cross-sectional OLS regression framework is used:

(2) $$Le\ Pen_i = \beta_0 + \beta_1 H_i + \gamma W_i + \varepsilon_i$$

Where i subscripts each French department, $Le\ Pen_i$ measures the percent vote share for Le Pen in the second round of the 2017 Presidential election, H_i is a hypothesis for populism considered one at a time, W_i is a vector of controls, and ε_i is the error term. The hypotheses considered here are social mobility as represented by intergenerational income elasticity, the Gini coefficient for income inequality and the share of births with at least one immigrant parent. In robustness checks social mobility is considered together with each alternative hypothesis one at a time. Unfortunately certain demographic statistics in France are generally not collected, but controls are included for each department's log population density and log income per capita.

Whereas the US specification uses the change in Republican vote share as the outcome variable, here the specification for France simply uses the 2017 vote share for Le Pen. This is because Le Pen was not an insurgent within her own party, and thus it does not make sense to focus on the change in the vote share for the *Front National*. The French voting data comes from the French Ministère de l'Intérieur.

The intergenerational income elasticity data covers 39 of France's most populous departments, and is drawn from Kenedi (2017), a master's thesis in economics from the Paris Institute of Political Studies (*Sciences Po*)[1]. Due to the incomplete coverage the results for this section must be interpreted with caution; nevertheless

[1] Recognized as the best master's thesis in that graduating class.

they seem indicative. The departments covered are: Aisne, Alpes-Marities, Bouches-du-Rhône, Calvados, Doubs, Finistère, Haute-Garonne, Gironde, Ille-et-Vilaine, Indre-et-Loire, Isère, Loire, Loire-Atlantique, Loiret, Maine-et-Loire, Marne, Meurthe-et-Moselle, Morbihan, Moselle, Nord, Oise, Pas-de-Calais, Puy-de-Dôme, Pyrénées-Atlantiques, Bas-Rhin, Haut-Rhin, Saôone-et-Loire, Sarthe, Paris, Seine-Maritime, Seine-et-Marne, Yvelines, Somme, Var, Essonne, Hauts-de-Seine, Seine-Saint-Denis, Val-de-Marne, and Val-d'Oise. Together, these departments cover approximately 64% of France's population.

Data on each department's income inequality in 2015, population density in 2016, and percentage of immigrant births in 2015 are from INSEE. Departmental GDP per capita in 2016 is drawn from Eurostat.

C. 2019 European Parliament Elections

A cross-sectional OLS regression framework is used:

$$Populist_i = \beta_0 + \beta_1 H_i + \gamma W_i + \varepsilon_i \tag{2}$$

Where i subscripts each European Union country, $Populist_i$ measures the percent vote share received by populist and far-right parties in the 2019 European

Parliament election, H_i is a hypothesis for populism considered one at a time, W_i is a vector of controls, and ε_i is the error term. The hypotheses considered are social mobility as measured by intergenerational income elasticity, the Gini coefficient for income inequality, the proportion of the population who actively use social media, the share of immigrants in the population, and the share of people aged 65 years or older in the population. In robustness checks social mobility is considered together with each alternative hypothesis one at a time. For controls log GDP per capita and log population are used.

The PopuList classification scheme devised by Rooduijn et al. (2019) is used to identify populist and far-right parties. In the main specification both populist and far-right parties are considered in order to use a somewhere more expansive net which includes parties, like Greece's Golden Dawn, that some scholars describe as populist and far-right but Rooduijn et al. (2019) consider only to be far-right. In a robustness check only parties labeled by Rooduijn et al. (2019) as populist are considered. Data for the Gini coefficient of income inequality is taken from the OECD for either 2016 or 2017 and, for non-OECD countries, the World Economic Forum's 2018 Inclusive Development Index. Intergenerational income elasticity data is taken from the World Bank's Global Database on Intergenerational Mobility for the most recent year available. In robustness checks alternative sources are used: the Gini coefficient is replaced where possible with the most recent data from the Luxembourg Income Study; wealth inequality as reported by the World Economic

Forum is considered instead of income inequality; and intergenerational income elasticity is replaced where possible with results from Corak (2013). An additional robustness check considers the change in the share of immigrants in each country from 2000–2015, a potentially relevant measure for the EU given the large immigration flows that followed new country accessions and international refugee flows over this time period.

GDP per capita (PPP) at 2010 USD, population, and age data from 2017 are taken from the World Bank's World Development Indicators. The percentage of the population accounted for by immigrants is produced by dividing the country's international migrant stock, also taken from the World Development Indicators, by population. Social media penetration data is from Hootsuite, for either 2016 or 2017 depending on data availability for each country.

D. Confidence in National Government

A cross-sectional OLS regression framework is used:

$$Confidence_i = \beta_0 + \beta_1 H_i + \gamma W_i + \varepsilon_i \tag{3}$$

Where i subscripts the country in question, $Confidence_i$ is confidence in national government, H_i is a hypothesis for populism examined one at a time, W_i is a vector of controls, and ε_i is the error term. The hypotheses examined are social mobility as measured by intergenerational income elasticity, the Gini coefficient for income inequality, the share of the population that actively uses social media, the share of immigrants in the population, and the share of people aged 65 or older in the population. In robustness checks social mobility is considered together with each alternative hypothesis one at a time. The controls are comprised of log GDP per capita and log population.

In the main specification the analysis is restricted to countries with GDP per capita levels of at least $25,000, as the research question pertains to developed countries. In a robustness check this threshold is increased to $35,000.

The outcome variable is taken from the 2016 World Gallup Poll, which asks respondents the binary question of whether they have confidence in national government. This aims to proxy for populist sentiment, the idea being that populist voters are generally highly dissatisfied with the political status quo. Askoy, Guriev, and Treisman (2018) similarly study this outcome variable in the context of the populist backlash against globalization, and Guriev (2018) discusses how the authors' research with this variable informs the rise of populism.

Data for the Gini coefficient, intergenerational income elasticity, immigrants, age, social media penetration, population, and GDP per capita from the same sources described in part C.

IV. Results

A. US Presidential Elections

Table 1 shows the main regression results for variables of interest concerning the 2016 US Presidential Election. Each specification includes a hypothesis for populism, examined one at a time. Intergenerational income elasticity (IGE) is significant with the expected sign (higher IGE means worse social mobility, which here is positively correlated with the vote swing towards Trump). The presence of seniors in the population is also significant with a positive coefficient, while income inequality and immigration are significant with *negative* coefficients. Table A1 shows that IGE retains its sign and significance if these alternative hypotheses are included, one at a time, in the same specification.

Table 2 shows the main results for the 2020 US Presidential Election. The sign and significance for IGE, income inequality, and seniors are the same, whereas that on immigration is now marginally significant in the positive direction. Notably, the magnitude of the coefficient on IGE is approximately one third of that observed for

2016. This result is consistent with Trump retaining a weakened level of attraction among communities affected by the economic unfairness of low social mobility in 2020. Table A2 shows that IGE retains its sign and significance when other hypotheses are included in the same specification.

TABLE 1 — MAIN RESULTS FOR 2016 US ELECTION, VARIABLES OF INTEREST

	(1)	(2)	(3)	(4)
IGE	0.200***	-	-	-
	(0.024)			
Income Inequality	-	-0.055***	-	-
		(0.021)		
Immigrants	-	-	-0.110***	-
			(0.021)	
Seniors	-	-	-	0.185***
				(0.025)
Observations	2750	2750	2750	2750
R^2	0.458	0.430	0.434	0.453

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE 2 — MAIN RESULTS FOR 2020 US ELECTION, VARIABLES OF INTEREST

	(1)	(2)	(3)	(4)
IGE	0.067***	-	-	-
	(0.020)			
Income Inequality	-	-0.128***	-	-
		(0.019)		
Immigrants	-	-	0.041*	-
			(0.024)	
Seniors	-	-	-	0.113***
				(0.019)
Observations	2749	2749	2749	2749
R^2	0.534	0.542	0.532	0.540

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Additional robustness and sanity checks are presented in the Appendix. Table A3 shows that changing the outcome variable from the percent change to absolute change in Trump support, as described in Section II.A, does not affect the sign nor significance of the social mobility variable for 2016; however, the 2020 result becomes insignificant. Table A4 shows that when the same specification employed in Table 1 is used for Mitt Romney's 2012 election results versus the 2008 Republican Presidential vote share, social mobility becomes significant with a *negative* slope. This suggests that Trump in particular, and not Republican candidates in general, may have successfully leveraged the prevalence of economic unfairness as an election strategy.

B. 2017 French Presidential Election

The main results for the 2017 French Presidential Election are shown in Table 3. Given the incomplete coverage of intergenerational mobility data for French departments this result must be interpreted with some caution. Nevertheless, low social mobility is significantly associated with a higher vote share for Le Pen. Table A5 shows that including alternative hypotheses in the same specification as IGE does not alter its sign nor significance.

TABLE 3 — MAIN RESULTS FOR 2017 FRENCH ELECTION, VARIABLES OF INTEREST

	(1)	(2)	(3)
IGE	0.378***	-	-
	(0.099)		
Income Inequality	-	0.881***	-
		(0.202)	
Immigrant Births	-	-	0.050
			(0.255)
Observations	39	39	39
R^2	0.627	0.665	0.489

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

C. 2019 European Parliament Elections

Latvia and Slovenia are dropped as outliers for these regressions. Latvia's reported intergenerational income elasticity is far beyond the range of any other country in the pool of available data. Slovenia voted for populists at an unusually high rate for a country with such a small population; its inclusion reduces the R squared of the five main specifications tested by nearly 40% on average, suggesting that Slovenia's experience was broadly incongruent with other countries[2]. The main analysis thus considers 19 countries for which data is available: Austria, Belgium, Croatia, the Czech Republic, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Slovakia, Spain, Sweden, and the United Kingdom. The main result is shown below in Table 4. Worse intergenerational

[2] Nevertheless Slovenia's inclusion does not alter the sign nor significance of social mobility in specification (1) from Table 4.

mobility is significantly associated with a higher vote share for populists and the far right, while all the alternative hypotheses are statistically insignificant.

TABLE 4 — MAIN RESULTS FOR 2019 EU ELECTION, VARIABLES OF INTEREST

	(1)	(2)	(3)	(4)	(5)
IGE	0.478**	-	-	-	-
	(0.200)				
Income Inequality	-	-0.002	-	-	-
		(0.221)			
Immigrants	-	-	0.317	-	-
			(0.197)		
Seniors	-	-	-	0.166	-
				(0.340)	
Social Media	-	-	-	-	-0.206
					(0.216)
Observations	19	19	19	19	19
R^2	0.382	0.180	0.221	0.198	0.215

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Robustness checks are presented in the Appendix. Table A6 shows that including alternative hypotheses in the same specification as social mobility does not alter its sign nor significance. Table A7 shows that social mobility is marginally significant with the same sign if the outcome variable is changed from populist and far-right parties, to only populist parties. Table A8 shows that the change in the share of immigrants in the population of each country from 2000–2015 is not significantly correlated with the vote share for populist and far-right parties. Table A9 swaps in alternative intergenerational income elasticity data where possible from Corak (2013), and likewise shows that the variable's sign and significance are unchanged.

Table A10 repeats specification (2) from Table 4 with alternative income inequality data from the Luxembourg Income Study, and with wealth inequality data from the World Economic Forum. Income inequality remains statistically insignificant, while wealth inequality is marginally significant in the negative direction. Finally, Table A11 repeats specification (1) from Table 4 with countries that have GDP per capita levels of at least $30,000 USD; intergenerational income elasticity retains its sign and significance.

D. 2016 Confidence in National Government

This international analysis covers 25 developed countries with GDP per capita levels of at least $25,000: Australia, Austria, Belgium, Canada, the Czech Republic, Denmark, Finland, France, Germany, Ireland, Italy, Japan, South Korea, Luxembourg, the Netherlands, New Zealand, Portugal, Singapore, Slovakia, Slovenia, Spain, Sweden, Switzerland, the United Kingdom, and the United States. While data is additionally available for Latvia it is dropped as an outlier, as in the analysis of the 2019 EU Parliament Elections.

The main results are shown in Table 5. Low social mobility is significantly associated with lower confidence in national government, while alternative hypotheses are never significant.

TABLE 5 — MAIN RESULTS FOR 2016 CONFIDENCE IN NATIONAL GOVERNMENT, VARIABLES OF INTEREST

	(1)	(2)	(3)	(4)	(5)
IGE	-0.259**	-	-	-	-
	(0.128)				
Income Inequality	-	-0.030	-	-	-
		(0.194)			
Immigrants	-	-	0.239	-	-
			(0.206)		
Seniors	-	-	-	0.086	-
				(0.138)	
Social Media	-	-	-	-	-0.034
					(0.166)
Observations	25	25	25	25	25
R^2	0.598	0.542	0.570	0.547	0.542

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Robustness checks are presented in the Appendix. Table A12 shows that social mobility retains its sign and at least marginal significance when alternative hypotheses are included in the same specification. Table A13 swaps in intergenerational income elasticity data from Corak (2013) where possible. With the alternative data Switzerland becomes a possible high-residual outlier (a Bonferroni outlier test yields a p-value of 0.108), and is dropped accordingly. Intergenerational income elasticity retains its sign and is marginally significant, although with Switzerland's inclusion it is not significant. Table A14 reruns specification (2) from Table 4 with income inequality data from the Luxembourg Income Study and wealth inequality data from the World Economic Forum. Income and wealth inequality are statistically insignificant. Table A15 restricts the analysis

to countries with GDP per capita levels of at least $35,000 USD; intergenerational income elasticity retains its sign and significance.

V. Discussion

The above results showcase a narrative of populism taking root in places with low social mobility. Low social mobility is consistently and significantly correlated with populism, while the alternative hypotheses unrelated to economic fairness are generally insignificant or have conflicting signs across different settings. Collectively, this evidence is consistent with the hypothesis that economic unfairness is linked to the rise of contemporary developed-world populism.

The results for the American elections demonstrate that vote swings towards Trump in 2016 were correlated with low social mobility. This was also true in 2020, but the magnitude of the coefficient was considerably smaller. It is plausible that Trump may, after four years in office and a pandemic, have been a somewhat less attractive candidate to voters affected by the economic unfairness of low social mobility. The fact that social mobility was significant in the negative direction for Romney – the "Massachusetts moderate" – also reinforces the contention that Trump found success with voters affected by economic unfairness because he ran as a populist and not a Republican. The same underlying problems may have been

there in 2012, but it took Trump's anti-establishment politics to convert them to electoral success.

The analyses of European countries yield comparable findings. While regressions concerning the 2017 French Presidential election must be interpreted with caution given the limited availability of data, the consistency of results for social mobility with those in the American context is encouraging. The regressions for the 2019 European Parliament election likewise demonstrate a connection between low social mobility and populism.

The international analysis of confidence in government also indirectly supports this paper's hypothesis. Low social mobility is correlated with discontent with the political status quo in developed countries.

Importantly, it is not only true that low social mobility is consistent with measures of populism in all the above settings; alternative hypotheses, equally, are not. Whereas income inequality significantly and positively contributes to the vote share for Le Pen in France it is significant and negative in the 2016 and 2020 US Presidential elections, and insignificant in other settings. The presence of seniors in the population is positive and significant in the US in both 2016 and 2020, but insignificant elsewhere. While the presence of immigrants is marginally significant and positive in the US in 2020, it is negative and significant in the US in 2016 and insignificant in other settings. Social media use is never significant. On the whole, then, there is a far more consistent relationship between low social mobility and

populism than is exhibited by alternative hypotheses. This remains generally true after applying a battery of robustness checks that involve alternative sources and definitions for variables.

Of course, this analysis consists of multiple regression and not causal inference. It can only demonstrate a persistent correlation between low social mobility and populism, and a good natural experiment seems unlikely to be found. But the robustness of the pattern these regressions display to very different contexts, data sources, and definitions is not insubstantial.

While it may be technically feasible to extend this paper's analysis by examining the vote share received by populist parties in different countries and different elections, as done by Pastor and Veronesi (2018), that approach is problematic because the outcome variable does not always properly correspond to true populist sentiment. Many people voted for Trump in 2016 because he was a Republican, for instance, and hence in the US it is most appropriate to examine the change in the Republican vote share from 2012 to 2016. Yet in the case of Le Pen in France the same transformation is not appropriate. Even if one could use the same transformation in both cases, the vagaries of French versus American politics mean that the same vote share may not be meaningfully comparable across the two countries. Unless looking at election events in the same settings with the same rules, more general indicators of political dissatisfaction – such as that used in this paper,

and by Askoy, Guriev, and Treisman (2018) – are arguably likelier to paint an accurate picture.

These findings have important implications for the way academics and policymakers understand and respond to the economic causes of populism. For example, these results suggest that the redistributive policy prescriptions associated with the typical focus on income and wealth inequality may be not only misguided but possibly counterproductive. Enforced equal outcomes are liable to be perceived as unfair, and right-wing voters with low trust in government do not think redistribution is a good remedy for low social mobility (Alesina, Stancheva, and Teso 2018). Instead, policymakers may want to pay attention to the legitimacy of complaints about unfairness, and focus on questions of economic fairness and social mobility to defuse populism. Depending on the country this may plausibly necessitate greater investment in the public goods that create substantive equal opportunity, like education, healthcare, and infrastructure, or market reforms that allow citizens to translate that opportunity to fair rewards. Pioneering work like Chetty and Hendren (2016), which investigates the determinants of social mobility across different US geographies, provides some guidance and lays the ground for future research.

These results also suggest that Milton Friedman's (1953) exhortation for economists to eschew normative issues is ultimately wrong-headed. The way the field of economics has focused on welfare – such as magnitudes and inequalities of

gains and losses – paints an incomplete picture of economic life, and arguably has helped create the conditions for populism in high-income democracies. By divorcing the study of economics from normative concerns about justice, the discipline has missed the fact that people don't simply care about how large or unequal economic rewards are; they deeply care about whether outcomes are fair. The early students of economics – more rightly thought of as political economists, such as Adam Smith in *A Theory of Moral Sentiments* – put normative questions at the center of their thinking. If the state of liberal democracy critically depends on the fairness of the society that hosts the economy, perhaps modern economists should return to their roots.

VI. Conclusion

This paper has argued that unfair economic outcomes are more plausibly associated with contemporary developed-world populism than a number of prominent alternative hypotheses. A series of cross-sectional regressions showed that low social mobility – an important type of economic unfairness – is a better correlate of the geography of populism, both within and across developed countries, than income and wealth inequality, immigrant stocks, social media use, and the share of seniors in the population who may undergo a "cultural backlash."

First, it was shown that swings in support towards Trump in the 2016 US Presidential election (versus the Republican Presidential vote share in 2012) among

US counties were significantly related to low social mobility. This was also true in 2020 versus 2012, but the coefficient on social mobility was substantially smaller. This finding is compatible with the idea that Trump's appeal to voters affected by economic unfairness waned by 2020, which may have contributed to his loss. The same analysis was repeated for the vote swing towards Romney in 2012 versus 2008; social mobility became significant and *negative*, indicating that Trump in particular – not just the Republican Party – engendered support from populations that suffer from low social mobility.

Second, it was shown that vote shares for Marine Le Pen in the second round of the 2017 French Presidential election among 39 departments were significantly related to poor social mobility. Data restrictions in the French context mean that this result must be interpreted with caution, but it is nevertheless notable that it displays a similar pattern as in the US.

Third, it was shown that vote shares for populist and far-right parties in the 2019 European Parliament elections were significantly related to low social mobility. This setting is especially advantageous as it yields one of the few instances of vote shares that are convincingly comparable across countries.

Fourth, it was shown that a lack of confidence in national government – an indicator of dissatisfaction with the political status quo that has been previously used in the literature on populism – is significantly related to low social mobility.

The alternative hypotheses, by comparison, were either insignificant or only occasionally significant, often with conflicting signs, across different settings. Thus low social mobility is a salient correlate of developed-world populism, more so than any of the hypotheses unrelated to economic fairness. Where possible, alternative data sources and definitions showed that the above findings are robust.

These findings suggest that some of the most commonly-discussed policy prescriptions to defuse populism may be misguided. For example, aggressively redistributing income and wealth may be counterproductive because enforced equal outcomes are liable to be seen as deeply unfair. Indeed, Alesina, Stancheva, and Teso (2018) show that right-wing voters with low trust in government do not see redistribution as a good solution for low social mobility. Instead, policymakers face the complex problem of improving economic fairness and social mobility. Depending on the country this may plausibly necessitate greater investment in the public goods that create equal opportunity, like education, healthcare, and infrastructure; or market reforms that allow citizens to translate that opportunity to fair rewards.

These results also suggest that more attention to questions of economic justice is warranted in academia. Although Milton Friedman (1953) contends that economists should ignore normative issues, normative problems in economics are arguably at the heart of the human condition and important to the survival of liberal democracy.

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Appendix

Table A1 presents a robustness check for the 2016 US election, where each alternative hypothesis is included in a specification together, one at a time, with social mobility.

TABLE A1 — ROBUSTNESS CHECK FOR 2016 US ELECTION, SOCIAL MOBILITY & ALTERNATIVE HYPOTHESES

	(1)	(2)	(3)
IGE	0.212***	0.196***	0.181***
	(0.023)	(0.026)	(0.022)
Income Inequality	-0.088***	-	-
	(0.020)		
Immigrants	-	-0.011	-
		(0.022)	
Seniors	-	-	0.162***
			(0.023)
Observations	2750	2750	2750
R^2	0.463	0.458	0.478

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A2 presents a robustness check for the 2020 US election, where each alternative hypothesis is included in a specification together, one at a time, with social mobility.

TABLE A2 — ROBUSTNESS CHECK FOR 2020 US ELECTION, SOCIAL MOBILITY & ALTERNATIVE HYPOTHESES

	(2)	(3)	(4)
IGE	0.084***	0.098***	0.056***
	(0.020)	(0.020)	(0.019)
Income Inequality	-0.140***	-	-
	(0.020)		
Immigrants	-	0.090***	-
		(0.026)	
Seniors	-	-	0.108***
			(0.018)
Observations	2749	2749	2749
R^2	0.547	0.537	0.542

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A3 presents a robustness check for the US elections where the outcome variable is the percentage change in the Republican Presidential vote share vs 2012. Specification (1) is for 2016, and specification (2) is for 2020.

TABLE A3— ROBUSTNESS CHECK FOR US ELECTIONS, OUTCOME VARIABLE CHANGED

	(1)	(2)
IGE	0.171***	-0.008
	(0.020)	(0.032)
Observations	2750	2749
R^2	0.479	0.446

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A4 presents a sanity check that runs specification (1) from Table 1 for the 2012 US Presidential Election versus the 2008 Republican Presidential vote share.

TABLE A4—SANITY CHECK FOR 2012 US ELECTION

	(1)
IGE	-0.118***
	(0.019)
Observations	2667
R^2	0.291

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A5 presents a robustness check for the 2017 French Presidential election, where each alternative hypothesis is included in a specification together, one at a time, with social mobility.

TABLE A5 —ROBUSTNESS CHECK FOR 2017 FRENCH ELECTION, SOCIAL MOBILITY & ALTERNATIVE HYPOTHESES

	(1)	(2)
IGE	0.249**	0.390***
	(0.099)	(0.093)
Income Inequality	0.680***	-
	(0.199)	
Immigrant Births	-	0.144
		(0.166)
Observations	39	39
R^2	0.716	0.633

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A6 presents a robustness check for the 2019 European Parliament election, where each alternative hypothesis is included in a specification together, one at a time, with social mobility.

TABLE A6 — ROBUSTNESS CHECK FOR 2019 EU ELECTION, SOCIAL MOBILITY & ALTERNATIVE HYPOTHESES

	(1)	(2)	(3)	(4)
IGE	0.496**	0.457**	0.785***	0.457**
	(0.227)	(0.225)	(0.228)	(0.208)
Income Inequality	-0.096	-	-	-
	(0.223)			
Immigrants	-	-0.097	-	-
		(0.257)		
Seniors	-	-	0.674**	-
			(0.318)	
Social Media	-	-	-	-0.087
				(0.148)
Observations	19	19	19	19
R^2	0.390	0.386	0.582	0.388

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A7 presents a robustness check for the 2019 European Parliament election that examines the vote share for parties classified as populist, instead of parties classified as either populist or far-right.

TABLE A7 — ROBUSTNESS CHECK FOR 2019 EU ELECTION, OUTCOME VARIABLE CHANGED

	(1)
IGE	0.374*
	(0.220)
Observations	19
R^2	0.377

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A8 presents a robustness check for the 2019 European Parliament election where the change in the share of immigrants in each country from 2000–2015 is examined.

TABLE A8 — ROBUSTNESS CHECK FOR 2019 EU ELECTION, MIGRATION FLOWS

	(1)
Change in Immigrants	0.255
	(0.413)
Observations	19
R^2	0.208

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A9 presents a robustness check for the 2019 European Parliament election that swaps in alternative intergenerational income elasticity data from Corak (2013) where possible.

TABLE A9— ROBUSTNESS CHECK FOR 2019 EU ELECTION, IGE DATA CHANGED

	(1)
IGE	0.498***
	(0.195)
Observations	19
R^2	0.397

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A10 presents a robustness check for the 2019 European Parliament election that swaps in alternative income inequality data from the Luxembourg Income Study where possible in specification (1), and uses wealth inequality data from the World Economic Forum in specification (2). Note that the latter data is unavailable for Austria, and the country is thus dropped in (2)

TABLE A10 — ROBUSTNESS CHECK FOR 2019 EU ELECTION, INEQUALITY DATA CHANGED, VARIABLES OF INTEREST

	(1)	(2)
Inequality	0.138	-0.419*
	(0.238)	(0.245)
Observations	19	18
R^2	0.196	0.273

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A11 presents a robustness check for the 2019 European Parliament election that restricts the analysis to countries with GDP per capita levels above $30,000 USD.

TABLE A11 — ROBUSTNESS CHECK FOR 2019 EU ELECTION, RICH COUNTRIES ONLY

	(1)
IGE	0.490**
	(0.209)
Observations	15
R^2	0.560

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A12 presents a robustness check for 2016 confidence in national government, where each alternative hypothesis is included in a specification together, one at a time, with social mobility.

TABLE A12 —ROBUSTNESS CHECK FOR 2016 CONFIDENCE IN GOVERNMENT, SOCIAL MOBILITY & ALTERNATIVE HYPOTHESES

	(2)	(3)	(4)	(5)
IGE	-0.265*	-0.279*	-0.267*	-0.262**
	(0.145)	(0.151)	(0.144)	(0.127)
Income Inequality	0.049	-	-	-
	(0.182)			
Immigrants	-	0.279	-	-
		(0.201)		
Seniors	-	-	-0.028	-
			(0.144)	
Social Media	-	-	-	-0.061
				(0.156)
Observations	25	25	25	25
R^2	0.597	0.634	0.596	0.599

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A13 presents a robustness check 2016 Confidence in National Government that swaps in alternative intergenerational income elasticity data from Corak (2013). Switzerland is dropped as an outlier.

TABLE A13 — ROBUSTNESS CHECK FOR 2016 CONFIDENCE IN GOVERNMENT, IGE DATA CHANGED

	(1)
IGE	-0.278*
	(0.143)
Observations	24
R^2	0.614

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation. Switzerland dropped as outlier.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A14 presents a robustness check for 2016 Confidence in National Government that swaps in alternative income inequality data from the Luxembourg Income Study where possible in specification (1), and uses wealth inequality data from the World Economic Forum in specification (2). Austria is dropped in (2) due to the unavailability of data.

TABLE A14 — ROBUSTNESS CHECK FOR 2016 CONFIDENCE IN GOVERNMENT, INEQUALITY DATA CHANGED

	(1)	(2)
Inequality	-0.020	0.136
	(0.172)	(0.138)
Observations	25	24
R^2	0.541	0.558

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Table A15 presents a robustness check for 2016 Confidence in National Government that restricts the analysis to countries with GDP per capita levels above $35,000 USD.

TABLE A15 — ROBUSTNESS CHECK FOR2016 CONFIDENCE IN GOVERNMENT, RICH COUNTRIES ONLY

	(1)
IGE	-0.369**
	(0.171)
Observations	20
R^2	0.604

Notes: Regression coefficients reported. White-corrected standard errors reported in parentheses. Coefficients for controls and intercept not reported. All variables are demeaned and divided by their standard deviation.

*** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.