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Q2. Does inequality affect support for the welfare state?

Introduction

Rising inequality in high-income countries has been coupled with a steady decline in the support for welfare states. I will focus here on Western Europe. Support for welfare in most Western European countries has steadily declined over the years. Even after the most severe and long-lasting recession in living memory, five years of a government intent of reducing welfare expenditure, and an increase in poverty among working-age people with no children, the UK public have remained relatively unsympathetic to spending on welfare (Clery 2016): further, there is a "clear pattern of convergence among Western countries" (Iversen and Soskice 2015).

At first blush, this pattern seems paradoxical: why are the poor and working public not clamouring for welfare when they need it most? In this essay, I first examine the seminal median voter model of Meltzer and Richards (1981), as well as a related model by Dimick et al. (2018) that examines income-dependent altruism. Both models predict that increasing inequality should *increase* the demand for welfare. I provide theoretical and empirical criticisms and reject both models. I then synthesise three models by Iversen and Soskice (2015), Lupu and Pontusson (2011), and Barth et al. (2018) into a single, "coalitionist" model, showing that the post-industrialisation of Western Europe led to a breakdown of social affinity, an increase in inequality, and a decrease in welfare support as a result. My conclusion is a nuanced one. It's not that inequality affects welfare state *in and of itself*, but that changes in the global work order simultaneously increase inequality and decrease welfare support, which explains the negative correlation.

A problem of measurement: it is difficult to disentangle cause and effect

First, a methodological aside. It is difficult to determine whether inequality affects support for the welfare state because of the possibility of *reverse causality*. Roughly speaking, we would like to find the coefficient on Inequality in the following causal model.

$$WelfareSupport = \beta_0 + \beta_1 Inequality + Controls + u$$

But here we have a problem, because support for the welfare state obviously affects welfare provision—which in turn reduces inequality. The possibility of reverse causality introduces endogeneity and poses a threat to the reliability of our results. Worse yet, Campbell (2012) shows that existing welfare provision (a function of welfare support) can cause a positive "feedback loop" by mobilising and crystallising the interests of groups of welfare recipients, further increasing welfare support. Thus the problem of reverse causality poses a strong problem to the robustness of our results.

There are several ways to get past this: using time-series data is one, and controlling for the degree of redistribution is another. I will be evaluating several of these models by how well they control for this endogeneity.

I reject median voter and inequality-dependent altruism models which predict an increased support for welfare

Now that we've gotten issues of measurement out of the way, we can proceed to the analysis proper. Here I analyse two theories that suggest that inequality should increase support for welfare, and reject them.

Meltzer and Richard's (1981) median voter model

A long and storied strand of political science literature has revolved around the median voter. It was therefore natural that Meltzer and Richards (1981)'s (MR1981) seminal model did too. In MR1981, the key variable is the ratio of mean income to the income of the median, or "decisive" voter. The median voter chooses the tax share that balances the government budget, while being fully aware that his choice affects everyone's decision to work and consume. Under this model, rising inequality should increase the size of the welfare state. This is because rising inequality effectively causes a movement from a normally-distributed income distribution to a bimodal one, which causes the median voter to have an income much lower than the mean income. The ratio of $\frac{MeanIncome}{MedianIncome}$ thus increases, and the median voter demands more redistribution.

Should we accept this explanation? MR1981's model asks both too little and too much of the median voter. It is patently unrealistic to expect the median voter to know the effect of his desired tax rate on leisure-labour substitution and consumption, seeing as even economists have trouble modeling this! Also, median voters do not decide on an optimal tax rate: rather, they have preferences over certain specific welfare programs, which depend on their own circumstances and their beliefs about the "deservingness" of the recipients. For instance, the British public largely agrees that we should increase welfare payments to single parents with children, but not unemployed couples with no children. Attitudes towards welfare are inextricably tied to altruism, and the MR1981 model cannot reflect that.

Dimick et al's (2018) income-dependent altruism model

These are significant blows to MR1981. Might we salvage it? Dimick, Rueda and Stegmueller 2018 (DRS2018) attempt to do this by modelling "income-dependent altruism". DRS2018 address both of the theoretical criticisms I levy on MR1981. They relax the unrealistic assumption of any individual knowing the effects of a tax on *everyone's* consumption and leisure — now each agent is modeled knowing only their own budget constraint and labour-leisure tradeoff. They also relax the "median voter" assumption, correctly pointing out that welfare spending requires that the middle-income and rich be at least *somewhat* sympathetic to redistribution. As such, this model is much more robust in its microfoundations.

The DRS2018 model works on the principle of diminishing marginal returns on income. As the rich get richer, each individual unit of income means less to them; they feel less pain in giving it away. As the poor get poorer, each individual unit means more to them; they become more deserving. This "income-dependent altruism" has the following conclusion: increased inequality makes the rich more willing and able to give.

DRS2018 improves upon MR1981 significantly. It gives the same predictions as MR1981 while relaxing many of the problematic assumptions. They also give some empirical support for their model. They run a multivariate regression and find that the rich are more supportive of redistribution in countries with high inequality, compared to countries with low inequality. More formally, they run

$$WelfareSupport = \beta_0 + \beta_1 Income + \beta_2 Inequality + \beta_3 Income \times Inequality$$

and find that β_3 is positive. However, they have fallen prey to the endogeneity I mentioned in the beginning! They operationalise inequality as the Gini of disposable income, which is *post-tax Gini*, but didn't control for pre-tax Gini.

Why is this problematic? Suppose that people's preferences about redistribution are affected in *precisely the opposite way*: rich people get very angry when they pay a lot (see Fernandez and Castillo 2017 for evidence). Then, welfare states that redistribute to a greater degree will have both low Inequality and less WelfareSupport amongst the rich, and welfare states that don't redistribute will have high Inequality and higher WelfareSupport. This will mean that we get a *positive* coefficient on $Income \times Inequality$, even with the exact opposite scenario. They should have controlled for pre-tax Gini to measure the *degree* of redistribution, which would prevent this error.

Indeed, more careful analysis by Beramendi and Rehm (2015) find no relationship between post-tax Gini and the attitudes of the rich toward welfare after controlling for pre-tax Gini. This deals a huge blow to the model, as its core prediction—that greater inequality softens the rich's attitudes towards redistribution—is unsupported by the evidence.

I synthesise three models of social affinity, bargaining and coalition-forming

We have examined two models that predict that welfare support should rise with inequality, and found them wanting theoretically and empirically. I now present a synthesis of three models that I believe best explains the data. The key insight here is that **welfare requires the support of the middle class and rich**, because the median voter is actually a net contributor to welfare. Welfare support critically depends on *social affinity*: how much net contributors empathise and see themselves in welfare recipients.

Firstly, Lupu and Pontusson (LP2010) show convincingly that it is not inequality per se, but *social affinity*–that most affects attitudes towards welfare provision. They show that when the middle class's income is close to the poor's, they identify with the poor rather than the rich—and form a bargaining coalition with them to redistribute from the rich. On the other hand, when the middle class identifies more with the rich, they oppose redistribution.

Secondly, Barth et al. (BFM2018) build a model of policy bargaining between left- and right- wing parties. They argue that as income decreases, people actually become *less* supportive of welfare as they have less to give away: welfare is a "normal good". In their model, increased inequality causes left-wing parties to decrease welfare provision because their electorate becomes less supportive.

Lastly, increased immigration into Western Europe has increased racial heterogeneity and decreased social affinity, which severely depresses welfare provision. As Alesina and Glaeser (2004) write: "the homogeneity in Europe has made it difficult for opponents of welfare to demonise the poor as being members of some hated minority. . . .on the other hand, [highly heterogeneous societies make] it easy for welfare opponents to use racial and ethnic divisions to attack redistribution".

I now combine the three models and demonstrate that it better fits how welfare provision has decreased in Europe.

Deindustralisation and globalisation in Western Europe increased inequality at the same time as it decreased social affinity, leading to decreased welfare support

I argue that the deindustrialisation and globalisation of Western Europe led to a decrease in social affinity between the new middle class (salariat) and the working-class even as it increased inequality, which decreased support for welfare states amongst the new middle-class.

The deindustrialisation of Western Europe resulted in large changes in worker composition. A move towards the service sector increased the ranks of the salariat, and dwindled semi-skilled blue-collar labourers. This dealt a triple blow to welfare states:

Firstly, social affinity between the middle-class and working-class decreased. Skilled blue-collar workers became the salariat, and the semi-skilled worker—whose skills were in low demand—became the low-skilled worker. Rather than a coalition of labourers on a spectrum of skill level, we now had a dichotomy of salariat vs. low-skill workers. The white-collar salariat ("new middle class") saw more of themselves in common with rich working professionals, which decreased social affinity and support for welfare (Lupu and Pontusson 2010).

This decrease in social affinity meant that traditional coalitions between semi-skilled labourers and professionals could no longer be sustained. The new salariat no longer benefited from employment protection and conjoint collective bargaining with low-skilled workers, weakening the insider-outsider divide present in left labour parties (Rueda 2005). We therefore witnessed a clear pattern of convergence toward less regulation of temporary and part-time employment, and the lack of strong advocates for employment protection legislature, further decreasing welfare provision (Iversen and Soskice 2015).

Finally, the swelling ranks of the new middle class and increased economic inequality present in the knowledge economy encouraged traditional labour parties to move to the right through the party bargaining mechanism outlined in BFM2018. This was exacerbated by a relatively liberal immigration policy: since immigrants are disproportionately welfare recipients, they easily became targets for political attacks by the right.

Conclusion

In sum, the move towards a knowledge economy in Western Europe simultaneously diminished traditional social affinities and increased inequality, which led to a decrease in support for welfare.

Q6. Are the forces that have driven increasing social liberalism in Western countries in decline?

Introduction

Are the forces that have driven increasing social liberalism in Western countries in decline? To answer this question, I first define what social liberalism is and show how it has increased over time. Then we need to agree on what the forces actually are. I give two competing theories: formative affluence (Inglehart 1981) and "elite cueing" via education and media. If it is formative affluence, then a decade of flagging growth (and the Great Recession) in Western European countries should arrest the growth in social liberalism. But if it is elite cueing, then the growth in social liberalism should continue given that higher education enrollment has been steadily increasing. I evaluate both theories and rule in favour of the socialisation theory, due to several methodological and empirical flaws of the formative affluence account. Overall, therefore, the forces driving increasing social liberalism are not in decline.

What is social liberalism?

Social liberalism describes progressive stances on socio-political issues like abortion and same-sex marriage as opposed to social conservatism. Under this definition, the Western world has grown more affluent and more socially liberal. Attitudes towards homosexuality, abortion, and gender equality have significantly liberalised over the past thirty years (Anderson and Fetner 2008, BSA 34).

What are the forces driving increasing social liberalism?

Why has Western society liberalised? I first consider the formative affluence theory, developed by Inglehart.

According to Inglehart (1981), material scarcity during one's formative years leads one to be socialised into holding "materialist" views, which are traditional views closely associated with survival, law and order, and stable economic growth. But because young people grow up in relative abundance, they will shift their thinking towards "postmaterialist" values that emphasise self-expression and personal freedoms, rather than "materialist" values that emphasise stability and economic growth. Furthermore, this adolescent socialisation is *persistent*: social attitudes ossify, and entire cohorts maintain the values developed during their adolescent years. The effects of improvements in economic conditions will be delayed and will only gradually have an impact through generational replacement.

What does Inglehart's theory predict in this case? A decade of flagging growth (and the Great Recession) in Western European countries should arrest the growth in social liberalism. We should therefore expect to see a "levelling-off" in liberal attitudes amongst the young.

Inglehart's formative affluence hypothesis cannot be the force driving increasing social liberalism

The materialist-postmaterialist dichotomy makes little theoretical sense

In the first instance, I believe Inglehart's (1981) characterisation of "materialist" and "postmaterialist" values is too reductive. Brooks and Manza (1994) got people to rank four values in order of importance: two 'materialist' and two 'postmaterialist', and fitted several loglinear models on them. If Inglehart's materialism-postmaterialism dichotomy is correct, then we should see two clear clusters representing the dichotomy. However, Brooks and Manza found that a model which allowed any clusters of beliefs to be correlated fit better than a model that restricted preferences to only cluster in the dichotomy. This suggests that people's beliefs don't track the postmaterialist-materialist dichotomy cleanly. This deals a big blow to Inglehart's account, as **even if formative affluence explains values related to stability and economic growth, this may have no relation at all with social liberalism!**

Using data from the 8-nation Political Action Study, de Graaf and Evans (1996) run a regression of postmaterialist attitudes against wartime severity and education, with several control variables:

$$PostMaterialism = \beta_1 YoB + \beta_2 WarSeverity + \beta_3 Education + \beta_4 FathersJob.$$

They find that after controlling for education and degree of post-war severity, the coefficient on $FathersJob$ (a proxy for formative affluence) becomes insignificant. Again, this suggests that Inglehart's postmaterialism is actually measuring values related to progressive liberalism due to education, not formative affluence. And while the significant coefficient on post-war severity could be taken as support for Inglehart's scarcity thesis, they could just as well be interpreted as influencing liberal values rather than postmaterialism, as "liberal values have been thought to positively related to psychological security".

In conclusion, liberal values are effectively orthogonal to economic issues. There is then no necessary trade-off between materialistic beliefs due to formative affluence, and those pertaining to liberal principles.

Social attitudes are not ossified and can change significantly post-adolescence

Secondly, according to Inglehart, social attitudes should not change very much after adolescence, because formative affluence determines one's outlook. But this is not true. There are many examples of attitudes changing significantly amongst all age groups over time. For instance, Andersen and Fetner (2008) use surveys of the US and Canada over three decades and find that—contrary to the theory that values are ossified during adolescence— attitudes towards homosexuality in the US and Canada have liberalised from 1980s–2000s for all cohorts equally, due to the increased (sympathetic) portrayal of lesbian and gay people in the media, and the contested politics over gay rights.

I have one methodological criticism to make of Anderson and Fetner. The work suffers from the well-known underdetermination (conflation) of age, cohort and period effects. They claim that $\{A : 0, P : +, C : +\}$, that is, there is a contemporaneous liberalisation in attitudes on all cohorts over time. But the same data is also consistent with the $\{A : +, P : 0, C : +\}$ interpretation: that is, rather than a contemporaneous effect, people get more tolerant as they age *and* younger cohorts are more tolerant. Of course, there is no real reason to believe that people get more tolerant of homosexuality as they age. So this does not really impugn the validity of the work. Nonetheless, I would have liked to see them use social aging effects as an *instrumental variable* for age as a robustness check. Crucially, this refutes Inglehart's claim that adolescent socialisation ossifies social attitudes: social attitudes can change very much even in adulthood.

The elite cues account better explains social liberalism

If formative affluence is not the answer, what is? A second theory, that of "elite cues", runs as follows: Elites (thought leaders) drive shifts in attitudes. They promulgate their messages in two ways: through socialisation in higher education and dissemination in venues like churches and mass media. Because elites disproportionately control discourse in these venues, they are able to shape the attitudes of society. One mechanism stands out in particular: students who enter higher education are socialised by their professor and peers to hold more liberal attitudes. As Tilley (2005) writes, "over the course of the twentieth century up to the end of the 1970s, elite political opinion had been broadly committed to *entrenching and extending libertarian ideas, from enfranchisement to abortion liberalisation.*"

Under this theory, the key driver of social liberalism is the growth in higher education, and the ability for messages to be disseminated through the mass media. We should not expect to see a decline in the growth of social liberalism, as higher education enrollment has not declined and the reach of mass media has only expanded over time.

There is strong empirical evidence to support the elite cues account

The elite cues theory has a lot of evidence supporting it. In *Culture War in America*, Jacoby (2014) develops a spatial vector model to show that value priorities different stem primarily from party partisanship, and secondarily from education. Critically, one's party identification affects value priorities **independent of demographic factors**: this supports an elite cueing mechanism such as the one described in Levendusky's (2009) partisan sort.

Egan and Mullin (2017) give another example of social attitudes being shaped by elite cues—but this time not in an unambiguously liberal direction. Climate change started off as a relatively unknown issue that both parties were equally ignorant about. However, a strategic misinformation campaign conducted by Republicans in the 1980s has made global warming the issue with the most partisan difference in issue priority today (Guber 2012). This is a prime example of an "issue evolution" in which "attitudes among partisans in the electorate have come to mirror the divided positions taken by partisans in government (Lindaman & Haider-Markel 2002)."

The ability of elites to shape social attitudes also extends to religious institutions. Paterson (2018) employs mixed methods to analyse the discourse of the Church of England. He finds that greater exposure to "elite cues" of "desecuritising messages"—measured via attendance at Anglican religious services—is consistently related to more positive immigration attitudes. This strongly suggests that religious elite actors have the capacity to shape secular, liberal attitudes.

Finally, Surridge (2016) uses panel data from the 1970 British Cohort Study and finds that there are "large and linear" education effects of education on social liberalism. Additionally, the coefficients for people who study a social science and humanities subject are double that compared to people who study science, business or medicine, which strongly suggests that education liberalises through socialisation and not by cognitive sophistication or psychodynamic feelings of security. And while one might be concerned with a self-selection effect (youths who are already more socially liberal self-select into studying social science degrees), Surridge controls for this with social attitudes of the cohort at age 16. Social liberalism at age 16 is strongly related to social liberalism at age 30—so there is certainly a selection effect here—but significant educational effects remain after controlling for values at age 16.

Conclusion

The main force driving increasing social liberalism in the West is indeed due to affluence, but not quite in the way Inglehart explained: rather, the increased affluence of the West meant a greater uptake of higher education, whereupon people were influenced by a liberal shift in elite political opinion. As higher education enrollment has not declined and the reach of mass media has only expanded over time, we should therefore not expect to see a decline in the growth of social liberalism.

Q9. Does the rational choice model of turnout help explain why turnout levels vary so much across countries?

Introduction

Whether or not the rational choice model helps to explain variation in turnout across countries depends on how exactly you define "rational choice". The simplest rational choice models like Lipset's (1954) "simple economic self-interest" cannot explain turnout in any country, much less the variation between countries. If we extend the model to allow for subjective benefits such as expressive voting (Hamlin and Jennings 2011) or information-gathering and decision-making costs (Verba 1995), then the rational choice model can better explain variation in turnout. But this raises the question whether or not a rational choice which includes purely subjective preferences can *really* be called a "rational" choice model. I suggest that while some elements of rational choice are certainly present in voting, the variation between and within countries can be better explained by a combination of socialisation and policy/social alienation.

The simple rational choice model doesn't have enough explanatory power

Rational choice theory contends that at the individual level, the decision to vote depends on the rational weighing of costs and benefits. In the simplest model, the benefit of voting is the differential utility from the individual's preferred election outcomes compared to the next most likely option, weighted by the probability of the individual's vote being pivotal. The costs are the time, effort or resources required to cast a vote.

Problematically, this version of the theory predicts that rational individuals should generally not vote, because the chances of casting a pivotal vote are infinitesimally small. This would predict that countries without compulsory voting should have near-zero turnout rates—producing a paradox of participation. This is of course problematic: we can forget about explaining variation within countries if the model can't even explain voting.

Finally, a seminal paper by Weakliem and Heath (1994, henceforth WH) deals a big blow to any rational choice theory contingent on income alone. Using BSA data, WH run multivariate regressions of $Vote$ on $Class$ controlling for both income and economic policy preferences, and show that neither can explain people's voting behaviour fully:

$$Vote = \beta_0 + \beta_1 Class + \beta_1 Income + \beta_2 EconPrefs + u$$

On Lipset's account, economic interest is the sole determinant of voting behaviour: the coefficient on Class should drop to zero once income and economic preferences are controlled for. But WH's findings show that this can't explain the data.

Improved rational choice: Riker and Ordeshook's subjective costs and benefits

To fix the issues of the simple rational choice models, Riker and Ordeshook (1968) extend the model. Hamling and Jennings (2011) point out that voting behaviour is never "instrumental", but rather "expressive". Rather than trying to change the direct eventual outcome of the election, voting behaviour is motivated by expressing one's ideologies or party affliation, and this possesses value to the individual in and of itself.

Riker and Ordeshook thus incorporate subjective benefits from voting into the model. The equation for the overall utility of voting can be expressed as:

$$R = BP\check{\ }C + D.$$

where BP is the benefit of having one's preferred party come to power multiplied by the probability that your vote will be pivotal, C are your subjective costs from voting, and D are your subjective benefits such as the satisfaction of complying with norms, satisfaction from affirming partisan preferences of allegiance to the system, enjoyment of the process, and so on. This model still suggests that P will be very small, and the relative size of C and D will almost always dominate decision-making. This fits with the finding that the effects of closeness on turnout, while significant, are small in magnitude (Blais, 2000).

Under this model, therefore, we can say that turnout differences between countries are due to differences in the subjective costs and benefits to voting. For instance, a country with compulsory voting increases the benefit of voting (to avoid having to pay a fine). We can also incorporate Verba et al.'s "resource model" (1995): education and socioeconomic status can reduce the costs (C) associated with understanding politics and policy positions of the parties.

While this 'extended' rational choice model works quite well, is it really a model of rational choice? It seems like this model kicks the can down the road. Apart from things like compulsory voting, whether or not it rained during election day, and so on, *how* can we explain the large differences in C and D between different countries? The rational choice model is silent on this front.

. . .

Differences in socialisation can explain some variation in turnout

Franklin's seminal socialisation thesis claims that voting is a habit that is learned during adolescence. Franklin (2004) found that the competitiveness of elections in early adulthood crystallises turnout habits. He runs a regression and finds that the interaction term

$$Age \times ClosenessOfElection$$

is highly positive and significant. While there is strong evidence to support the claim that voting behaviour is learned through socialisation, however, I disagree that closeness of election explains the huge changes in turnout. This is because Franklin has committed an econometric mistake. Franklin ran the regression with the interaction term $Age \times ClosenessOfElection$ but didn't include those terms on their own. This means a **high likelihood of Type I error** as if the individual constant terms are significant at all (as we know they are, from Blais), the interaction term is likely to be (falsely) significant.

In any case, however, there is strong evidence to support the claim that turnout is affected greatly by socialisation. Here I give three pieces of evidence. First, Heath (2007) found that women who came of age before full franchise was extended were much less likely to vote in subsequent elections than women who came of age with equal rights, whereas men were unaffected (as they already had the franchise).

Second, Ojeda (2018) found that there is a long-term effect of formative income on turnout: people who grew up poor (as measured by family income at age 16) are less likely to vote for decades afterwards, even if they go on to earn high incomes in the future. This effect lasts until they are in their 40s, which suggests a strong effect of formative income on long-term voting patterns, which could explain variation in turnout across countries. This also lends credence to the adolescent socialisation mechanism: if one is poor and doesn't vote, one is less likely to vote in the future.

A large part of adolescent socialisation works on a proximate social level: if your parents are voters, you're more likely to vote. Fieldhouse and Cutts (2012) find that young people's participation is highly sensitive to the presence of other voters in the household. They run a multivariate regression with an interaction term, showing that 1) the turnout of first-time electors is higher for those who live with other voters, and 2) the impact of living with other voters is stronger for first time electors than for other electors.

The evidence strongly suggests that adolescent socialisation plays a big role in long-term voter turnout. **If this is the case, then differences in adolescent socialisation between countries can explain the variation in turnout.** But how can we explain the difference in adolescent socialisation between countries? This is what I turn to next.

Policy social alienation explains another part of turnout

Apart from socialisation, I suggest that the variation in turnout can be explained by the different degrees of policy and social alienation of each political landscape.

In particular, in countries where there was a convergence of the center-left and center-right, the working class became disillusioned and exited the political process. Evans and de Graaf (2013) use CMP data to measure the L-R positions of left wing-party blocs and to measure party polarisation based on a weighted sum of standard deviations from the mean. They find that party platforms in many Western European countries have not systematically converged. Between countries that did and countries that didn't, however, large variations in turnout can result.

How policy and social alienation cause lowered turnout

I will give here the example of the UK. Blair's move towards new Labour in the 90s meant a huge convergence in party positions and descriptive representation. Heath (2016) and Grady (2018) examine the composition of working-class Labour MPs and find that the proportion of working-class MPs in Labour have declined from 30% to less than 10%.

This had effects on turnout, in particular amongst the working class. Evans and Tilley (2012) use BES panel data and find that the gap between the working-class and middle-class in terms of support for Labour has fallen from around 30% in 1983 to around 10%. While part of this is certainly due to Labour becoming more attractive to middle-class voters, a larger part of this is due to working-class abstention. Most notably, the proportion of working-class voters who identified with "No Party" has increased tremendously, from 15% in 1983 to about 35% in 2010. The proportion of people who perceived a great difference between the parties plummeted from 85% in 1987 to 17% in 2001. Turnout fell too, to become the lowest across all Western European countries.

This is substantiated by cross-national work. Spoon and Kluver (EJPR 2019) run a multilevel logit regression on nearly 15,000 vote choices of individual voters. They find that that mainstream party voters across Western Europe are more likely to switch to non-mainstream parties the more similar the policy positions of the mainstream parties are: "[party] convergence reduces voters' ability to distinguish among the parties... and may then turn to another party that presents a more divergent position that may be closer to their ideal position." They write:

> The recent success of the populist right-wing AfD [German Far Right party] is, for example, often attributed to its ability to mobilise non-voters who previously felt under-represented by the established mainstream parties.

Conclusion

While the rational choice theory can explain some cross-national variation in turnout (most notably compulsory voting), a large part of the variance can be traced to socialisation and policy convergence. Where left and right parties converged, turnout fell the most, and stayed low, thus explaining the variance.