(When) Do Parties Affect Economic Inequality? A Systematic Analysis of 30 Years of Research

Martin Haselmayer and Alexander Horn

Despite 30 years of research on economic inequality, the question of whether government ideology affects inequality remains unresolved. As rising inequality poses a major challenge to contemporary democracies, we ask: (when) do parties matter regarding inequality? Our *systematic analysis* finds that research is divided, with a tendency toward a pessimistic "no." We decipher the factors that account for this split in theoretically predictable ways. We assess the roles played by the type of inequality, the time horizon, and the impact of policy channels. Bivariate and multivariate analyses of 393 TSCS-regression findings show how the type of inequality and a neglect of top incomes, a focus on short- rather than long-term effects, and the inclusion of policy channels that absorb the effects of parties strongly codetermine the results. Effects *septuple* depending on how these factors are combined. We draw three lessons that, when combined, foster a shift toward a more optimistic perspective on the latitude of politics.

Can we find a cure for inequality that isn't worse than the disease?
—Milanovic (2017)

Martin Haselmayer (martin.haselmayer@univie.ac.at, Austria) is a senior research fellow in the Department of Government at the University of Vienna and an associated postdoctoral researcher in the Varieties of Egalitarianism project (voe-project.org) at the University of Konstanz. His research focuses on political communication, elections, and inequality. His work has been published in journals such as the British Journal of Political Science, European Journal of Political Research, *and* Political Communication.

Corresponding author: Alexander Horn (Alexander.horn@uni-konstanz.de, Germany) is the head of the Emmy Noether Group's Varieties of Egalitarianism project (www.voe-project.org) at the Politics of Inequality Cluster at the University of Konstanz and interim Professor for Comparative Analysis of Political Systems at the Humboldt University of Berlin. He was Assistant Professor at Aarhus University and Kennedy Fellow at Harvard. His work on parties, inequality, welfare states, and reforms has been published in journals such as Comparative Political Studies, Journal of European Public Policy, European Journal of Political Research, *and* Socio-Economic Review, *where he recently won the 2024 Best Article prize. He is the author of the book* Government Ideology, Economic Pressure, and Risk Privatization *(2017).*

In 2013, then US president Barack Obama delivered a poignant speech in which he acknowledged that "[economic] growth has flowed to a fortunate few" and declared economic inequality "the defining challenge of our time." To address this challenge, it would be necessary "to set aside the belief that government cannot do anything about reducing inequality" (Obama 2013). In 2014 and 2020, Thomas Piketty published his bestselling books *Capital in the Twenty-First Century* and *Capital and Ideology*, in which he posits that inequality tends to increase evermore without political countermeasures. Economists such as Piketty (2020) and Atkinson (2015) have discussed what such predistributive and redistributive measures could look like—and have even tried to quantify the effects of different measures on inequality.

From a consequentialist perspective (Jensen and van Kersbergen 2017), high and rising inequality has largely adverse effects on societal and political outcomes (Solt 2008). Cagé (2020), Gilens (2012), and Wilkinson and Pickett (2009) suggest that increasing material inequality undermines the promise of political equality and eventually democracy itself. It is, thus, all the more noteworthy that voices publicly questioning the idea that inequality can be fought politically have gained momentum. For instance, Walter Scheidel (2017, vii) has argued that aspects such as partisanship, social democracy, and broader legislative reforms are not a viable cure. Historically, Scheidel claims, it is violence, rather than political reform,

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that has led to economic leveling (9). But is it true that we cannot vote or legislate our way to greater equality? What cumulative knowledge do we have on the impact of democratic governments on economic inequality? Does the political composition of government affect economic inequality, and how? Given widespread concern about inequality and its negative political and societal externalities, the extent to which political choices can slow or reverse its rise is an existential question for the vitality of representative democracy. Economists who study inequality are both optimistic (Atkinson 2015) and pessimistic (Milanovic 2016), but do not provide large-N tests. Fortunately, exploring the impact of parties and the steering capacity of governments is central to the aims of political science and political economy. Unfortunately, the evidence remains inconclusive even within this specialized group. While there is no shortage of excellent studies, there are no consolidated findings either.

The standard theoretical argument is that "parties matter" because left parties are more economically egalitarian than right parties so they can appeal to their less affluent core electorate. This assumption that left parties represent their below-median income core constituency and fight against inequality vis-à-vis more equality-averse right parties has been the point of departure for many studies (Pontusson and Rueda 2010; Tavits and Potter 2015). However, assumptions about the alignment of social groups and parties have come under pressure as the focus of electoral politics has shifted away from economic conflicts around redistribution (Häusermann and Bornschier 2023) and as midrange theories suggest less affluent voters are hard to mobilize in contexts of high inequality (Gallego 2015; Solt 2008).

Based on a systematic review (and a complementary meta-analysis) of 43 papers and 393 regression results, we show that existing research (Bradley et al. 2003; Huber, Huo, and Stephens 2019; Iversen and Soskice 2006; Pontusson and Rueda 2010 vs. Huber, Petrova, and Stephens 2022; Lee, Kim, and Shim 2011; Wallerstein 1999) is almost evenly split regarding the effect of government parties on economic inequality.[1] The ratio is 63:37, tilted against party effects.[2] This raises the question of whether it is possible to find theoretically relevant dimensions that account for mixed results and systematically bias the conclusions.

We discuss and present three such dimensions or bundles of choices that explain—in theoretically predictable ways—whether authors will find party effects on inequality. It matters what type of inequality we look at and if income concentration at the top is captured; it matters if we look at short- or long-term effects of the composition of government; and it matters how analyses account for policy channels that absorb the explanatory power of partisanship.

As we will show, depending on these decisions, the likelihood of analyses reporting party effects *septuples.* For representative democracy, and in light of the resurgence of interest in inequality after the 2008 financial crisis, the results can be read as positive news. If certain theoretical considerations are accounted for—and we think they should—it still matters who is in office. Our analysis shows that the effects of partisanship on inequality are often underestimated and explains why that is the case.

Contribution and Structure

The contributions of the paper are fourfold and are reflected in the structure of the paper. First, we discuss and theorize factors that should skew analyses either in favor of, or against, finding partisan effects on inequality. Second, we take stock of the inconclusive results from the first systematic review of government effects on inequality, summarizing over three decades of research. Third, our key findings speak directly to the heated debate about the steering capacity of political parties in the politics of inequality. We show that regression analyses that leave out the position of the most affluent, that focus on annual effects rather than medium- and long-term effects, and that include various policies through which partisanship exerts an influence are much less likely to yield positive results. Vice versa, analyses that look at inequality at the top, consider mid- and long-term effects, and avoid policy controls are very likely to show that the composition of governments has a significant effect on inequality.

By contrast, the status of partisanship as an explanatory or control variable plays a less important role. We have included it because we were concerned about publication bias, especially at top journals. Fortunately, this idea was disconfirmed in a variety of tests. Likewise, the expectation —inspired by realignment and welfare research—that party effects are characteristic until the 1970s and then fade away proves to be descriptively correct, but is not robust in the multivariate analyses.

In a fourth step, we discuss the substantive implications for the politics of inequality and summarize what researchers can learn from our results for future studies on the (party) political determinants of inequality. Most importantly—and despite the limitations that we acknowledge—if done right, analyses of party effects show that governments affect inequality.

Theory-Guided Systematic Analysis, 1990–2023

To make sense of competing findings, we build a comprehensive dataset capturing key study characteristics and employ bivariate analysis and meta-regression of over 30 years of research. A meta-regression (Littell, Corcoran, and Pillai 2008) uses statistical tests (in our case, results from time-series cross-sectional [TSCS] regressions) as units of analysis, study features as explanatory variables, and estimation results as dependent variables. This enables researchers to assess how individual choices in the research

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process affect the results. It thereby synthesizes empirical evidence, which makes it a key technique for systematic literature reviews that aim for systematic selection, collection, evaluation, and synthesis of research (Dacombe 2018).

Our point of departure is the inconclusiveness of results found in 43 peer-reviewed articles and 393 results from TSCS models that look at party ideology effects in OECD countries. We describe the three-tier search and filter strategy we used below. What is crucial for now is that only two out of every five results indicate that partisanship has an effect on inequality—and that these studies vary strongly in how they assess this relationship. While the existing literature offers little as to what explains this puzzling variation, we unpack it in two empirical steps —using group comparisons and using multilevel logistic regression. As a robustness test, we complement these analyses with a multilevel meta-analysis based on available (standardized) regression coefficients. Before we conduct any meaningful analyses, two questions are logically prior. First, we need to outline why governments should have an effect. Second, to decipher the inconclusive results in *theoretically predictable ways*, we theorize how a few key choices affect the findings. Both aspects are addressed hereafter.

Why Should Government Parties Affect Inequality?

The impact of government ideology on economic inequality remains a contested issue. There is ample empirical evidence supporting and rejecting the argument that parties and cabinets can do something about it. Yet before we decipher the evidence further, we need to assess the arguments that speak in favor of, or against, party effects on economic inequality. We find three main perspectives. First, two influential approaches in political science suggest that government partisanship should indeed affect inequality: the power resources approach and partisan theory. Second, the predictions derived from these approaches contrast with three sets of midrange theories that suggest that parties—even left ones—do not strive for equality in unequal contexts. Third, outside political science, a broad literature, discussed in academia and outside it, suggests that governments are either unable (Scheidel 2017, but cf. Atkinson 2015) or not committed enough (Piketty 2020) to fight inequality via legislation.

First, starting with the theoretical workhorses and meta-approaches in political science, it is clear that these would suggest a classic partisanship pattern: while the Left is expected to fight inequality, the Right is more indifferent or accepting of material inequalities. Partisan theory (Hibbs 1977) and power resources theory (PRT) (Esping-Andersen 1990; Korpi 1978; 1983; Stephens 1979) suggest that the distribution of societal and political power resources and the political coloring of governments are decisive for egalitarian outcomes and income equalization.[3] Hibbs (1977, 1468) claims that "the macro-economic policies pursued by left- and right-governments are broadly in accordance with the objective economic interests and subjective preferences of their class-defined core political constituencies." Likewise, Korpi (1983, 107) argues that "the relationship between social position and party preference is generated by the fact that the social structure gives various groups in society partially divergent interests, which the political parties then may attempt to promote and safeguard" in the "democratic class struggle." With regard to inequality, the rationale is that "[a]ctors relying primarily on economic resources can be expected to favor market distribution, while categories of citizens relatively disadvantaged in terms of economic resources and relying primarily on their labor power are likely to combine in the sphere of politics to modify outcomes of, and conditions for, distributive processes on markets" (Korpi and Palme 2003, 427). Variations of the argument that left parties strive for economic equality because their constituency demands it can still be found in many of the articles we surveyed. By contrast, the transformation of party systems and new electoral cleavages (Häusermann and Bornschier 2023) are acknowledged, but do not crowd out the focus on the old class conflict.

Second, there are a number of midrange approaches in political economy that call into question the assumption that left parties fight high inequality. This literature suggests that inequality should be self-reinforcing irrespective of the distribution of political power between left and right parties, either because it is shown that in contexts of higher inequality the (below-median) core constituency of the Left is demobilized and does not vote (Gallego 2015; Solt 2008), which renders it less attractive to strive for more equality, or because inequality levels have become part of the status quo perception of voters, who have consequently internalized inequality outcomes as meritocratic (Mijs 2021; Trump 2018). Alternatively, it could be that neither parties nor voters are sufficiently cognizant of high inequality (McCall 2013; McCall et al. 2017).

Third, outside political science, the recent debate about the politics of inequality has been shaped by studies that provide pessimistic answers regarding the impact of parties. The instruments parties have to fight inequality, like reforms and legislation (Atkinson 2015), do not compensate for the absence of war, pandemics, and state failure—the historical levelers of the economic playing field (Scheidel 2017). "More benign influences" (2017, 22), such as reforms and legislation, in that view, do not work. There is no way to regulate or vote ourselves to significantly greater equality (9). Or, as Piketty (2020) argues, left parties are no longer especially focused on economic equality because their constituencies are

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increasingly influenced by the highly educated (the "brahmins"), who care less about economic equality than the old left electorate.

Why Studies Underestimate Party Effects

The aforementioned inconclusiveness brings up two key questions: What is it—assuming that the steering capacity of governments is viewed as desirable—that distinguishes optimists from pessimists? And what are the theoretically relevant dimensions that help us to better understand when government partisanship matters? In answering these questions, we rely on a close reading of 43 studies we identified as relevant and of the rationales that their authors provide.

Moreover, we draw on discussions from a field that has successfully agreed on answers to the question of whether and how parties matter. Comparative welfare state research has reduced the uncertainty surrounding the causes of conflicting results regarding the impact of parties. They have identified three factors that help to explain *seemingly* incompatible results.[4] First, different periods of welfare state evolution have been identified. The effect of partisanship has become less pronounced after a "golden age" of welfare state expansion was surpassed by an era of retrenchment in which competitive and fiscal pressures undermined the autonomy of parties (Green-Pedersen and Haverland 2002; Horn 2017; Pierson 1994; Starke 2006). Second, a long debate about the *dependent variable problem* (Clasen and Siegel 2007; Green-Pedersen 2004) has shown that spending ratios are less susceptible to political control than social rights and welfare benefits. Finally, with regard to government partisanship, a fair test of the impact of parties should complement the use of static party labels with dynamic and issue-specific conceptualizations of ideology (Döring and Schwander 2015; Horn 2017). As a result, it is now common to motivate choices regarding (1) time frame, (2) choice of the dependent variable, and (3) the way party effects (the independent variable) are conceptualized.

In the following, we theorize these three dimensions with regard to inequality effects.

Time/Period Effects

First, as in welfare state research, there is an assumption in many of the 43 inequality studies we surveyed that party effects on inequality declined when the economy stopped growing faster than budgets (Brady 2009; Huber, Huo, and Stephens 2019; Nam 2020). As is also the case in welfare state research, a closely related justification for period effects is that party politics have become increasingly marginalized due to pressures exerted by globalization and postindustrialization (for Pierson [1996], the latter is the more serious constraint). In this reading, the leeway for different (re)distributive approaches via wage policy, transfers, or fiscal measures was constrained by the exit option of capital and lower productivity gains. Similarly, and partly induced by these structural changes, realignment and changing cleavages would suggest that economic polarization between left and right parties over predistribution and redistribution have become less important (Häusermann and Bornschier 2023). Therefore, our expectation is that analyses that start earlier are more likely to find that partisanship has an effect.[5]

Type of Inequality

Second, although monolithic conceptions of economic inequality per se can be found, many authors are well aware that this is simplistic. Studies conceive of inequality as either overall inequality (Gini coefficients), a ratio of two specific income groups, or as the share held by top income groups relative to the "rest" ("top shares"). Gini measures are typically sensitive to changes in the middle of the income distribution (Atkinson 1970, 256–57), but have a weaker impact on public perceptions compared to the fortunes of top income groups (Franko 2017). Thus, while political actors can mobilize on the latter (e.g., "the 99 percent" versus "the 1 percent"), it may be harder to embed inequality captured via a Gini coefficient in a campaign slogan. Accordingly, inequality indicators that capture the share of high earners (the 1 percent) or ratios that capture the difference between high-income and middle- or low-income groups (e.g., a 90:10 percentile ratio) are more susceptible to political control than broad Gini indicators (Piketty 2020). Regarding the importance of the effect of parties on inequality, inequality at the top provokes public outcries (like the Occupy movement's "99 percent" and "1 percent" slogans) and the concentration of gains at the top concerns those who fear unequal responsiveness due to influence relying mostly on affluence (Cagé 2020; Gilens 2012). In terms of the potential for mobilization and political steering capacity, we would thus expect effects for inequality measures that tap into the opposition of "the rich" or "the well-off" versus "the rest" rather than for those measures that tap into a Gini coefficient or mean-to-median ratios (Franko 2017; Kevins et al. 2018).

Partisanship

Third, regarding partisanship, we expect three factors to make a difference in terms of party effects on inequality: (1) the use of short-term versus long-term effects, (2) the status of parties in the analysis as a core explanatory or control variable, and (3) the role of policy channels. A question that is relevant for all three aspects is what instruments parties have at their disposal to affect inequality. Here, we must *try* to distinguish between predistribution and redistribution. Under redistribution, we subsume all fiscal measures and all more or less generous and more or

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less progressively financed welfare policies. These social programs serve as "automatic stabilizers" against inequality shocks or lead to vertical redistribution. Redistribution means that inequalities in market incomes are leveled via taxes and transfers, leading to a more compressed distribution of disposable income. Yet there is neither pure market income equality or inequality nor are policy instruments always purely redistributive *or* predistributive. While predistributive measures in favor of, or against, wage-setting policies, minimum wages, and employment protection affect market inequality, welfare benefits such as unemployment insurance or assistance function as a reservation wage that strengthens the bargaining power of collective labor and individual workers vis-à-vis "capital" (Korpi 1983).

The Use of Short-Term versus Long-Term Effects. Since ideology and inequality are *sluggish* variables, we juxtapose models with short-term effects and models with long-term effects. Studies that assume immediate or short-term effects operate at the time of observation $t0$ or at $t - 1$ (Kwon 2018; Sjöberg 2009; Wallerstein 1999). By contrast, studies that assume that the political effects on inequality unfold over the long term use cumulative measures that sum up the share of left or right parties in cabinet over a long time period ahead of the measurement of inequality (Beramendi and Cusack 2009; Bradley et al. 2003; Huber and Stephens 2014). A fair test of partisan effects should take long-term effects into consideration. As short-term changes in inequality are typically of modest size (an average Gini coefficient varies by 0.1 per annum), it is optimistic to detect partisan effects on inequality over the course of one or two years. While the slow-changing nature of our dependent variable and the lagged influence of partisanship are not "new" (Bradley et al. 2003), the trend toward cabinet periods or other cumulative measures marks another interesting parallel to welfare research.

The Status of Parties in the Analysis as a Core Explanatory or Control Variable. We contrast studies that assess partisanship as a key explanatory variable with studies that treat it "only" as a control variable. Compared to all other aspects discussed here, our concern here is less deductive, more explorative, and more precautionary. One may even say that this is an atheoretic concern. When reading the 43 studies, we were under the impression that the papers with null findings were published in journals with lower impact factors and rarely in what are considered the "top" journals. This reminded us of the possibility of conscious or unconscious *p*-hacking (which *could* be more widespread if partisanship is the key explanatory variable), and inspired us to compare otherwise *largely* similar models that differ as to whether partisanship is the independent variable or a control variable. We must be careful not to overinterpret the importance of the differences we find here. We include this differentiation cautiously and acknowledge that it is not, in general, instructive to interpret control variables substantively. However, in a systematic analysis or review and/or a meta-analysis, we can control for *most* of the variation (in terms of variables and the specification) between models. We *know* how similar the models are. In any case, excluding otherwise similar models because of the status assigned to the variables in the paper (as "mere" control) would violate the idea of a systematic analysis and lead to bias.

The Role of Policy Channels. Finally, and surprisingly given the prominence of this long-standing debate, we found little overlap in the way papers discussed and modeled the role of policy context. We see the risk that policy channels, such as social spending or welfare generosity, condition or even absorb party effects. Controlling for a laundry list of these channels that is not theoretically proven will lead us to underestimate party effects. In other words, we endogenize or absorb party effects when we add policy instruments to party shares. After all, we know that left parties are more inclined than right parties to use social welfare and fiscal measures to fight inequality. The same problem applies to more distant causes of welfare and wage policy, such as corporatist bargaining structures and unionization, which are themselves subject to party effects. As Jensen (2014) has shown, right governments rarely risk a frontal attack on the welfare state. Instead, they first try to undermine and erode corporatism and the power of unions. In sum, no matter whether we consider welfare policies, spending, or attempts to weaken corporatism, using the right and/or left share of parties in government alongside lists of instruments/channels as independent variables creates a *race of the variables* that leaves unresolved the question as to which means, other than these instruments/channels, parties can use to exert their influence. The inclusion of policy channels such as welfare or wage policy means that we test the capacity of parties to steer inequality in ways that go beyond using said policy channels. If many such channels are included, parties do not exert an effect.

We have discussed three potentially relevant dimensions. Because the conceptualization of the third, partisanship, has been split into three separate *sub*dimensions, this leaves us with five parameters or choices.

Foreshadowing the results, and although any kind of period effects are not robust enough in multivariate analyses to include them in our model, the remaining four choices theorized here explain whether party effects are found or not. Depending on these four choices, the probability of finding significant party effects varies between 11% and 78%.

How Did We Select and Summarize Studies?

This analysis covers all *relevant* articles published in English-language peer-reviewed journals between 1990

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and 2023. The selection criteria for relevance were that studies (1) conduct a direct test of partisan effects on inequality, (2) focus on OECD countries, (3) include at least 10 countries, and (4) use TSCS analyses with a minimum time-series length of 10 years. These criteria ensure a minimum level of conceptual and methodological comparability across studies, which facilitates systematic analyses (Cooper 2019). The OECD focus has the advantage that the political dynamics and scope conditions for party effects are similar and that functionally equivalent data and measures are available. Part 1 of the supplementary material provides all details on the selection and coding of articles. While a focus on published work could be susceptible to publication bias, empirical evidence suggests that broad criteria, including unpublished work or gray literature, could distort results due to a lack of quality control for such studies (Egger et al. 2003). Moreover, we include studies whose main aim is to assess the impact of parties as well as studies that merely control for partisanship. This design allows us to assess empirically whether studies that primarily test hypotheses of partisan effects on economic inequality are more likely to report that government ideology matters.

To retrieve and select relevant articles, we use three search strategies. While these approaches yield similar results, we deliberately chose this conservative and time-consuming strategy to confirm that we did not miss a study that meets our criteria. First, we ran comprehensive keyword searches in the most important databases covering published peer-reviewed work from disciplines related to the topic: Web of Science and ProQuest. A very broad keyword list generated 2,692 unique hits. We manually inspected all articles and, based on a close reading of titles and abstracts, we excluded irrelevant articles (e.g., single-county studies, non-OECD country samples, and studies on subfields of inequality or welfare) and identified potentially relevant ones. Second, we further checked whether the latter contained results from TSCS regressions with inequality as a dependent and partisanship as an independent or control variable. Third, once this step was completed, we applied two complementary strategies for each relevant article: (1) we checked all articles citing it and (2) inspected its references. We repeated these steps for each new article we discovered and only stopped when we could not retrieve additional ones. Based on our selection criteria, we had to exclude valuable cross-sectoral studies (with a focus on corporatism and the Kuznets curve) and research that did not account for partisanship from the 1990s (Gottschalk and Smeeding 1997; Gustafsson and Johansson 1999; Nielsen and Aldersen 1995). In light of the data constraints scholars faced until the late 1990s, the absence of studies from the 1990s is plausible. Finally, consultation of five authors who have published extensively on the party politics of inequality confirmed that we had not overlooked any relevant studies.

Data and Variables

The dataset contains 43 studies from three disciplines: political science (25), sociology (13), and economics (5). We collected information from 330 regression models presenting 393 coefficients on the relationship between partisanship and inequality in the main text of an article. The different numbers of coefficients stem from studies including variables on left and right partisan shares in the same model. On average, we collected nine coefficients from each article. All studies were published between 1999 and 2023; three-quarters after 2008. The fact that almost one in three studies were published in leading journals, such as *World Politics* (4), *Socio-Economic Review* (4), *American Political Science Review* (3), and *Comparative Political Studies* (2) attests to the high general relevance of this research field. However, while all leading political science, subfield, and sociology journals are represented as well, the articles appeared in a wide range of journals (e.g., judging based on the journals' impact factors).

Dependent and Independent Variables

Our primary interest is to examine whether the partisan composition of governments (still) affects economic inequality. Therefore, the dependent variable of our analysis is an indicator of whether a regression result reported a significant partisan effect on inequality. We assign a value of one to models reporting a significant relationship of parties on inequality and zero to null findings. While it would be preferable to compare coefficients across studies to evaluate the difference in effect sizes, a direct comparison is not feasible, because studies use different measures of inequality and partisanship. Whereas standardization of coefficients could solve this problem, it introduces other well-known problems. These coefficients combine the strength and the variation of effects, are sensitive to sample composition, and are hard to interpret (Achen 1977; Blalock 1961; King 1986). Moreover, several studies lack some information necessary for standardizing regression coefficients (e.g., the total number and levels of all variables included in a model). On average, two in five results indicate that partisanship has an effect on inequality (37%).[6] However, as studies cover different time periods and use different conceptualizations of inequality and partisanship, we try to decipher whether the consideration of these three bundles of factors yields more consistent patterns regarding partisan effects. While we limit explanations of aggregation rules to consequential cases regarding time period effects, type of inequality, and conceptualization of partisan effects, we provide a full overview of all our coding decisions in part 1 of the supplementary material.

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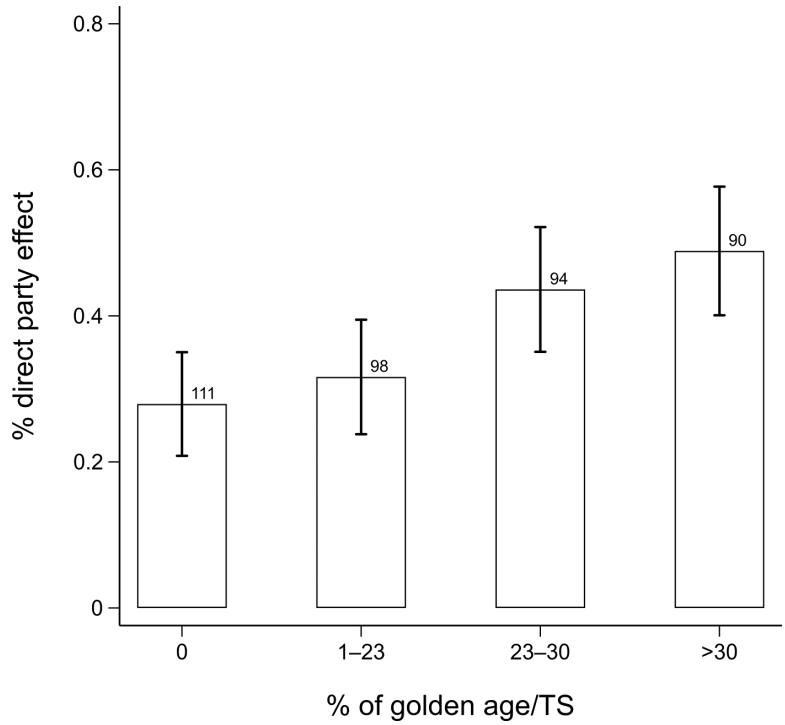
Figure 1.
Time Periods and Partisan Effects on Inequality



Notes: Number of observations on top of bars. Whiskers indicate 90% confidence intervals. Y-axis shows the share of models that indicate direct and significant party effects (0.8 = 80%). TS: time series.

Bivariate Analysis

Time Period

Let us start with the question of whether the prevalence of partisan effects is associated with a certain period. Historians and scholars of the welfare state and inequality often hold that the "golden age" of capitalism (1960s–1970s) coincided with welfare state expansion, followed by an era of retrenchment. During the latter, governments thought they had little choice but to distribute losses rather than gains (Esping-Andersen 1990; Hobsbawm 1996; Pierson 1994). Studies focusing on the impact of globalization or international trade also tend to see the late 1970s and early 1980s as a watershed moment, after which the political choices of elected governments were increasingly constrained. This was caused by tighter fiscal conditions (after two oil crises) due to lower growth and deeper economic integration, which provided companies and capital with an exit option (Nam 2020; Scharpf 2000). This was perceived as a threat to tax-and-spend policies, predistribution and redistribution (often used to pacify social conflicts), and the postwar compromise between capital and labor (regarding wage bargaining, training, or welfare state schemes) more broadly. As decade or period dummies are not available for all studies, we account for the relative importance of the golden age. On average, this period accounts for 18% of the time series. For graphical presentation, we divide all observations into quartiles and plot the percentage share of models reporting a partisan effect across these groups.

According to figure 1, only one in four results report that parties or cabinets affect economic inequality when the golden age is not covered in the time series. This fraction increases with the growing weight of this time period and peaks at one in two results, suggesting that parties' capacity to shape economic inequality has continuously declined since the golden age.

Type of Inequality

Measures of inequality vary in terms of focus and granularity (Atkinson 2015; Jensen and van Kersbergen 2017). Though a few studies try to convey that they study economic inequality as a whole despite a narrow focus on one specific kind, many of the 43 studies acknowledge that different political incentives and logics apply to the measures at hand. Consider the Occupy Wall Street slogan "we are the 99 percent," directed against the increasing share of income held by the 1 percent; discussions about

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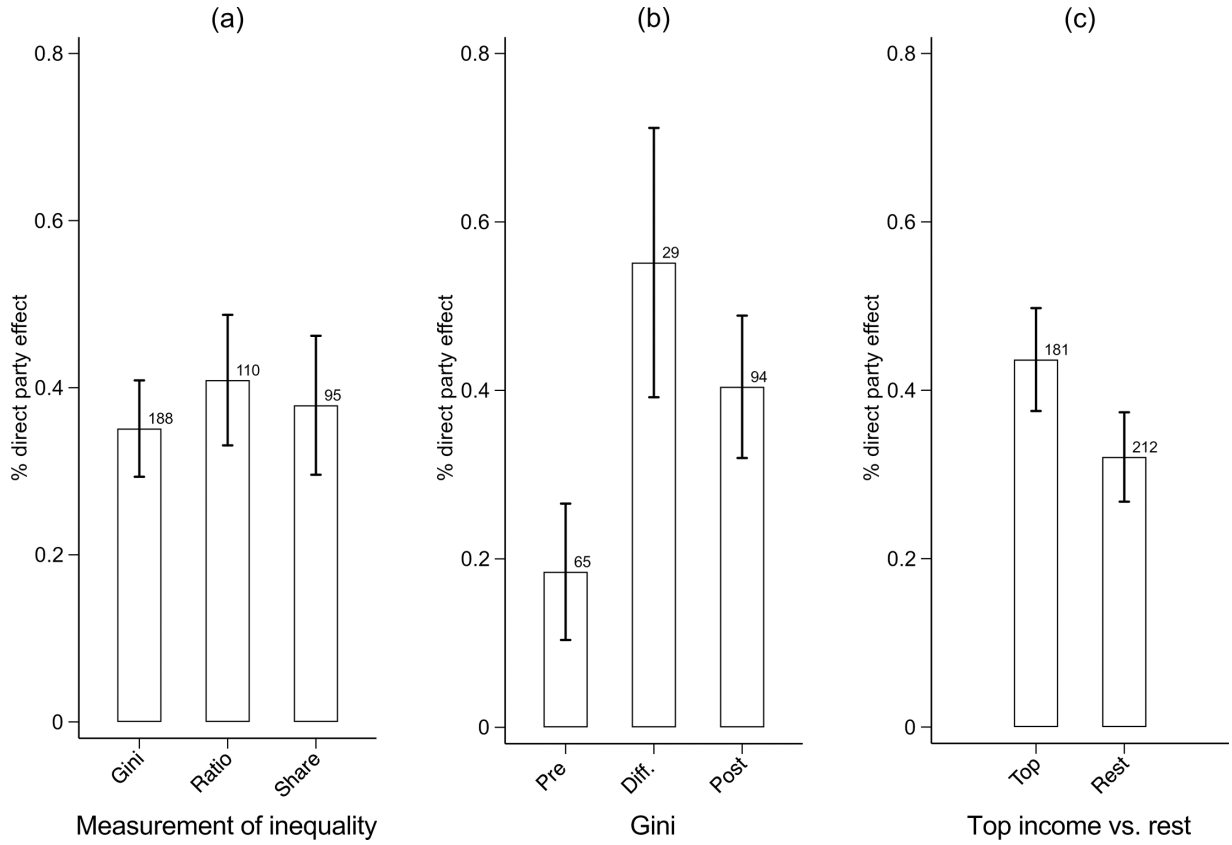
the backsliding of the middle class vis-à-vis the economic elites; and concerns about growing overall inequalities according to Gini coefficients. Top-share inequality, ratios between income groups, and measures that summarize the inequality of income distribution have different political implications. In terms of political steering capacity, we expect effects for inequalities that tap into the opposition of "rich" or "well-off" versus "the rest" rather than those that tap into a less observable Gini coefficient or the mean-to-median ratios.

To assess whether a dependent variable pattern applies, we summarize inequality measures into three broader categories reflecting their operationalization and data sources:[7] (1) Gini coefficients, (2) income ratios, and (3) top income shares. As panel (a) of figure 2 shows, Ginis are the most commonly used indicator, yet they are also the least likely to yield partisan effects (35%) compared to income ratios (42%) or top income shares (38%).

As the three categories are quite heterogeneous, we further explore variation within and across indicators. First, we compare (1) pre-tax, (2) pre- versus post-tax, and (3) post-tax Ginis. The former only picks up income inequality based on wages and is less susceptible to direct political influence than post-tax measures, which account for parties' and cabinets' capacity to redistribute wealth based on taxes or spending. As shown in panel (b) of figure 2, party effects were up to four times more likely when Ginis account for redistribution (55%) compared to pre-tax and transfers Ginis (18%). As we argue that top incomes (including those of the more reliably measured well-off) might be more susceptible to partisan effects than broader indicators, we consolidate our data into measures pertaining to top incomes versus the rest (panel [c]). The former contains top income shares (top 1%, top 10%) and ratios including this group (90th–10th or 80th–20th percentile). The latter summarizes the Gini coefficients and income ratios that contrast middle- and lower-income groups (50th–10th percentile). The figure's right panel confirms that partisan effects are more likely for top income groups (44%) than for the residual category (32%). Hence, the influence of parties appears to be greater for measures relating to high- and top income groups that are channeled through redistributive policies.

Figure 2.
Measure of Inequality and Partisan Effects on Inequality



Notes: Number of observations on top of bars. Whiskers indicate 90% confidence intervals. Y-axis shows the share of models that indicate direct and significant party effects (0.8 = 80%).

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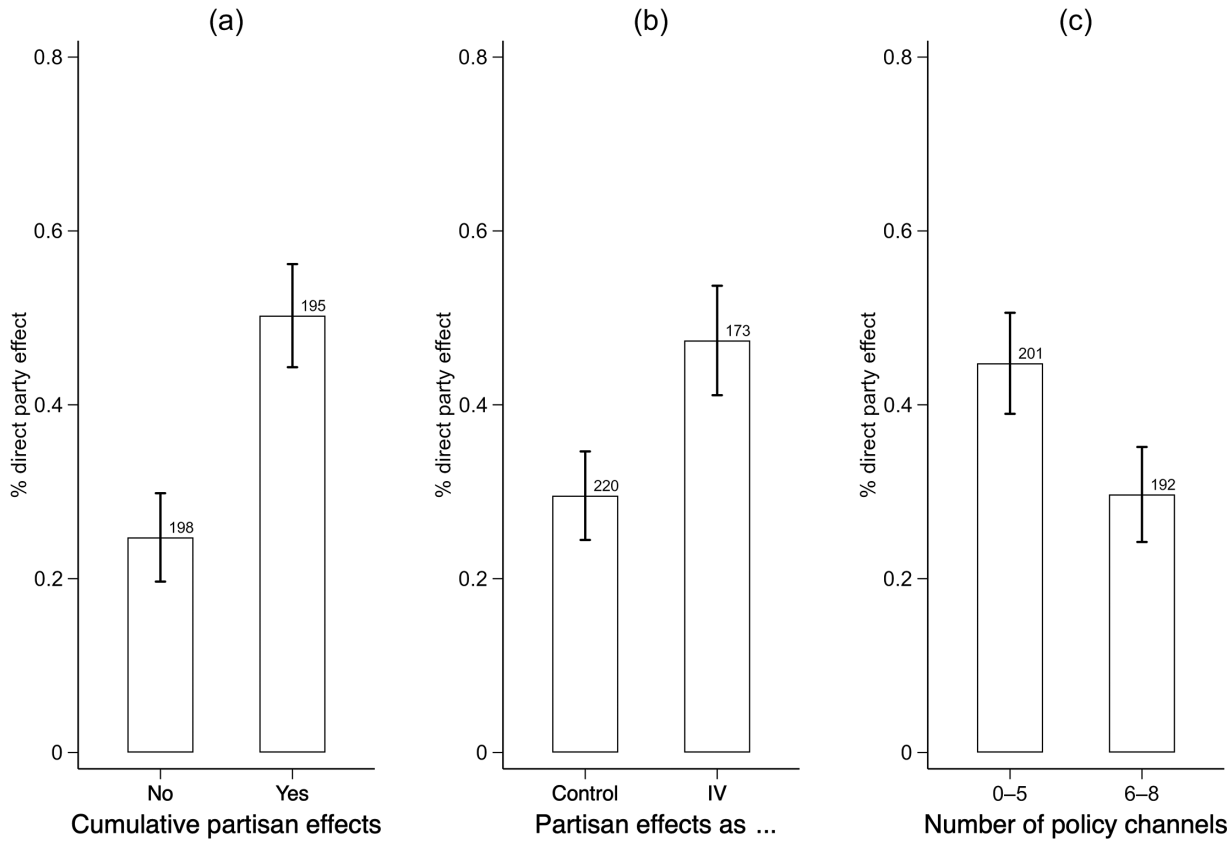
Partisanship

Research on partisan effects has argued that the measurement of partisanship will affect the ability of researchers to detect such effects (Döring and Schwander 2015; Horn 2017). Typically, these studies make a strong case that fine-grained (e.g., dynamic and issue-specific) measures are more likely to capture meaningful variation than simpler approaches relying on party labels (e.g., left versus right shares measured via historic expert judgments or party family affiliation). Yet thus far, research on partisan effects on inequality has almost exclusively used party labels (e.g., ordinal measures based on expert judgments). Only two (Neal 2013; Rueda 2008) out of 43 studies incorporate variation across parties using a more specific measure derived from the quantitative content analysis of election programs (Volkens et al. 2021). Yet even these studies rely on rather broad left–right scores, which encompass many categories that are unrelated to a party's stance on inequality. This neglect of dynamic and issue-specific preferences is in itself a striking finding, but the lack of variation in the measurement of party profiles does not allow us to include this aspect in our analysis.

First, we look at short-term versus long-term effects of government partisanship. Studies that assume immediate or short-term effects operate at the time of observation $t0$ or at $t - 1$ (Kwon 2018; Sjöberg 2009; Wallerstein 1999). By contrast, studies that assume that the political effects on inequality unfold over the long term use a cumulative measure that sums up the share of left (or right) parties in cabinets over a long time (Beramendi and Cusack 2009; Bradley et al. 2003; Huber and Stephens 2014). As shown in panel (a) of figure 3, studies using a cumulative measure were twice as likely to report partisan effects (50%) than those looking at short-term effects (25%).

Likewise, articles with an explicit focus on partisan effects are more likely to report effects. We compare whether partisanship was presented as an independent or a control variable. The former group of studies formulate explicit hypotheses regarding partisan effects and reflect on the importance of this variable in the title, abstract, introduction, or conclusion; the latter merely treat it as a control. Panel (b) of figure 3 shows that half of the studies hypothesizing about party effects on inequality report such effects (47%), a figure that drops below a third for studies

Figure 3.
Conceptualization of Partisan Effects on Inequality



Notes: Number of observations on top of bars. Whiskers indicate 90% confidence intervals. Y-axis shows the share of models that indicate direct and significant party effects (0.8 = 80%). IV: independent variable.

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with another research focus. These differences could suggest publication bias or point to the importance of including other variables that absorb or condition party effects. While supplementary analyses indeed suggest that high-impact journals are less likely to publish nonfindings, this effect vanishes once we account for the inclusion of policy channels (see section S-B in part 2 of the supplementary material), which alleviates our concerns about publication bias.

Finally, we turn to the role of direct and indirect policy channels through which parties may not exert an impact on inequality. Though not all studies consider policies, and the ones that do look at widely different policies, we group these variables into four groups: (1) policies (e.g., public spending, taxes, and decommodification); (2) corporatism (e.g., unions and wage bargaining); (3) postindustrial transformation (e.g., changes in the labor market or the education sector); and (4) globalization (e.g., financial and economic openness).[8]

We also sum these variables up to account for analyses incorporating variables such as wage inequality, welfare expenditures, tertiarization of the labor market, and trade openness in the same models. Some of the channels are theoretically related to cabinet ideology (policies), reflect a country's political power relations (corporatism), or represent more general scope conditions for party action (financial openness). As outlined above, these variables can moderate or absorb party effects (Pontusson, Rueda, and Way 2002; Rueda 2008). On average, studies accounting for a multitude of policy channels should be less likely to detect partisan effects. The right panel of figure 3 presents evidence based on a median split of the number of policy channels (evidence for separate policy channels is available in figure S-B1 in the supplementary material). Including a moderate/low number of such variables means that studies are more likely to report partisan effects compared to studies where parties and cabinets had to "compete" with up to eight policy channels. Thus, controlling for policy channels can reduce direct effects of parties and cabinets. As these channels include policy interventions (e.g., social policies, taxes) or reflect power relations (corporatism), this finding is not surprising.

In sum, there is strong bivariate evidence that the explanations that we identified determine partisan effects on inequality. To be sure, some of these variables are correlated, and our observations are not independent of each other (we have multiple results from the same studies). To conduct an authoritative test, we proceed with multilevel logistic regressions.

Other Variables

Before we turn to multivariate analyses, we want to point to variables that should be taken into consideration, though we do not have theoretical priors or a pronounced substantive interest in them. Our first such variable, the journal's impact factor, is a proxy for a study's quality. While only a crude indicator, it is the most "objective" measure to approximate variations in study quality. We collect these values from Clarivate Journal Citation Reports.[9] Finally, studies vary in scope in terms of the length of the time series and the number of countries included. Due to the modest number of observations, we cannot include both variables in our models, but we use the number of observations reported for a regression model as proxy. On average, a result is based on 271 country-time units.

Multivariate Analysis

To examine whether the strong bivariate patterns hold in multivariate analyses, we specify logistic regression models. Our dependent variable indicates whether a result reports a significant partisan effect on inequality (1) or not (0). As multiple results from the same article are not independent of each other, we add random effects at the level of articles. The number of clusters (43) is rather high for the total number of observations (393), and some clusters have few observations, which can be challenging for multilevel models as estimates may be less precise (Raudenbush 2008). Yet the dependency of observations and model diagnostics suggests that the heterogeneity between studies requires multilevel modeling (the random effects compose approximately 70% of the total residual variance). As a consequence, some standard errors are somewhat large, yet results are robust across alternative specifications (such as ordinary least squares [OLS] multilevel models and logit models with article-clustered/robust standard error) or if we rerun our estimation with observations from articles with at least five or 10 results per cluster.

The models jointly include the explanatory factors outlined above. As some of the variables are correlated, we present separate models for each of them in the supplementary material. We further add the variables outlined in the previous section (journal impact, logged number of observations) as controls. The supplementary material provides additional robustness checks, including different model specifications (OLS multilevel models and logit models with article-clustered/robust standard errors) accounting for different data sources of inequality, and presents results for samples of observations from articles with at least five or 10 results per cluster. As discussed in greater detail in the robustness section, we also present results based on pooling an overall effect for each study and measure of inequality. We examine whether party effects differ for studies on inequality and redistribution, or if market inequality or disposable income inequality (after taxes and transfers) yield different results. We also use a more conservative aggregation rule for top-level inequality

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and control for studies using country-fixed effects. We also reproduce the core results based on standardized regression coefficients. All of the additional analyses corroborate the results reported below.

Results

What determines partisan effects on inequality? Table 1 and figure 4 confirm most of the bivariate patterns presented above except for the share of the "golden age." While the time-period effect has the expected positive effect in the separate model (see table S-C1, model 1, in the supplementary material), the insignificant coefficient even turns negative with a large standard error once we add the remaining explanatory variables. Thus, the strong bivariate pattern of the golden age and partisan effects is explained away by more substantive factors.

Table 1.
Explaining Partisan Effects on Inequality

	Model 1	Model 2	Model 3
Time			
% of golden age	−3.38	−4.55	−2.26
	(3.05)	(4.24)	(2.91)
Measure of inequality			
Income ratios	0.46	—	—
	(0.37)		
Top income shares	2.31**	—	—
	(0.92)		
Gini: Post-tax	—	0.71	—
		(1.27)	
Gini: Pre vs. post	—	1.00	—
		(0.71)	
Top vs. rest	—	—	2.34**
			(1.08)
Conceptualization of partisan effects			
Cumulative partisan effect	3.17***	2.07	3.22***
	(1.04)	(1.46)	(1.08)
Partisan effect as control	−1.98**	−3.61*	−1.97*
	(0.98)	(1.87)	(1.07)
Number of policy channels	−0.27*	−0.32	−0.34**
	(0.15)	(0.21)	(0.17)
Controls			
Journal impact factor	−0.67*	−0.40	−0.66*
	(0.35)	(0.86)	(0.35)
N (model)	0.00*	0.01*	0.00*
	(0.00)	(0.00)	(0.00)
Constant	−0.03	0.15	−0.37
	(1.36)	(2.07)	(1.44)
Sigma based on article clusters	7.18**	3.14	8.25*
	(3.61)	(2.37)	(4.61)
Observations	393	188	393
AIC	354.74	179.89	338.26
BIC	394.47	212.26	374.02

Notes: Robust standard errors in parentheses.
* $p < 0.10$
** $p < 0.05$
*** $p < 0.01$

Turning to the role of different types of inequality, models 1–3 and figure 4 corroborate and nuance our conclusions from the discussion above. For model 1, positive coefficients of income ratios and top income shares confirm that the choice of dependent variable matters. Results from these indicators are more likely to report partisan effects (p-value below 0.05 for top income shares). To provide a more intuitive interpretation of the effects, we plot the predicted probabilities based on model 1. Figure 4 shows that studies using top income shares as a dependent variable were 16 percentage points more likely to report partisan effects than studies relying on the Gini coefficient. Compared to Gini indicators, drawing on income ratios increased the rate of positive findings by 7 percentage points. Thus, researchers using data on top income shares (top 1%, top 10%) or ratios comparing the better-off to other groups (90:10, 80:20) were considerably more likely to find that partisanship has an effect on economic inequality.

Regression coefficients in model 2 and predicted probabilities in figure 4 confirm strong variation for research relying on different Gini indicators. While standard errors are somewhat larger due to the smaller sample size, predicted probabilities in the central panel of figure 4 show differences in reporting partisan effects when comparing pre-tax Ginis (22%) and post-tax Ginis (34%) to pre- versus post-tax Ginis (55%).

Looking into differences in partisan effects for studies focusing on top income groups or other segments of the population, model 3 reports a positive effect (coefficient of 2.34, p-value below 0.05). This suggests that the political leaning of cabinets is more relevant when studies examine high- and very high-income groups (via top shares or vis-à-vis middle and/or lower income groups). Predicted probabilities in figure 4 attest to a substantial difference: on average, partisan effects were 20 percentage points more likely for measures including high and top incomes compared to the residual category.

Turning to the conceptualization of partisan effects, the temporal distinction of short-term versus cumulative effects shows positive coefficients in models 1 and 3 (p-values below 0.01). Studies arguing that ideology operates over the long term were thus more likely to capture partisan effects on inequality than those focusing on more immediate effects. Predicted probabilities in figure 4 show a sizable effect: studies relying on a long-term measure of partisanship were 19% more likely to report that partisanship matters for economic inequality than those using a short-term indicator (based on model 3, $p < 0.01$).

For the status of partisanship in a study, both models 1 and 3 show a negative coefficient, suggesting that studies merely controlling for partisanship were less likely to report that parties and cabinets matter for inequality.

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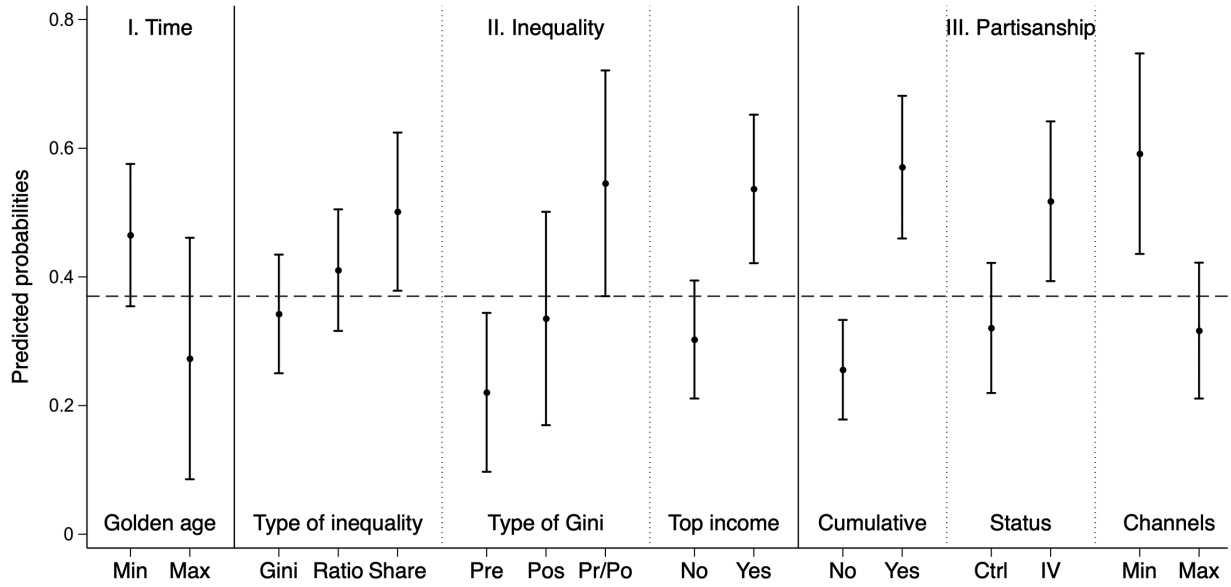
Figure 4.
Predicted Probabilities Plot



Notes: Y-axis shows predicted probabilities along with 90% confidence intervals. Dashed line indicates average effect. IV: independent variable. Results are based on table 1. All remaining variables are held constant at their observed values.

Figure 4 shows that studies using partisanship as a control variable were roughly 18 percentage points less likely to report effects than those treating it as an independent variable (predicted probabilities based on model 3). While the effect is substantive, it is not entirely robust: adjusting for other variables of interest reduces the effect and turns it insignificant (see also table S-C1, model 5, in the supplementary material). Thus, despite sizable differences resulting from the focus of a study on partisanship (or not), other explanatory variables account for much of the variation that stems from it.

Finally, the inclusion of (too) many direct or indirect policy channels, such as social spending, reduces the ability of parties to shape inequality. In figure 4, predicted probabilities for partisan effects drop by 27% across the empirical range (from 59% for models adjusting for no policy channels to 32% for models including eight such variables). To be sure, there is a sensitive balance between omitting important variables and the overdetermination of models that goes beyond the scope of a secondary analysis. However, including a multitude of policy channels[10] obviously decreases the rate of partisan effects.[11]

Joint Effects of Predictors: When Do We Find Party Effects on Inequality?

Our results point to a number of individual factors that can shape partisan effects on inequality. Yet the choice of the dependent variable and the conceptualization of partisan effects reflect distinct choices by researchers. To further consolidate our understanding of partisan effects on inequality, we need to understand the joint effect of various *combinations* of the factors outlined above. Therefore, we estimate predicted probabilities based on the co-occurrence of the key variables identified in our previous analyses (regression results are based on table 1, model 3). Figure 5 presents the results based on all empirical combinations from the studies. It provides evidence that the conceptualization of inequality and partisan effects massively *codetermine* the likelihood that a study reports that partisanship matters.

The x-axis of figure 5 simply indicates how many variables are at values that were associated with partisan effects in our previous analyses, with values ranging from zero to four predictors. On the left-hand side of the x-axis, a value of zero represents results that (1) do not focus on top income groups, and (2) use a short-term measure of partisan effects (3) as control variable while (4) adjusting for six to eight policy channels (above median). For such studies/models, the rate of party effects is a mere 11 percentage points. This very low value *septuples* to 78% of the results for studies (1) with a focus on top incomes, (2) with a cumulative measure of partisan effects, (3) with partisanship as an independent variable, and (4) with few (zero to five) policy channel controls.

In sum, the choices individual researchers make with respect to their dependent, independent, and control

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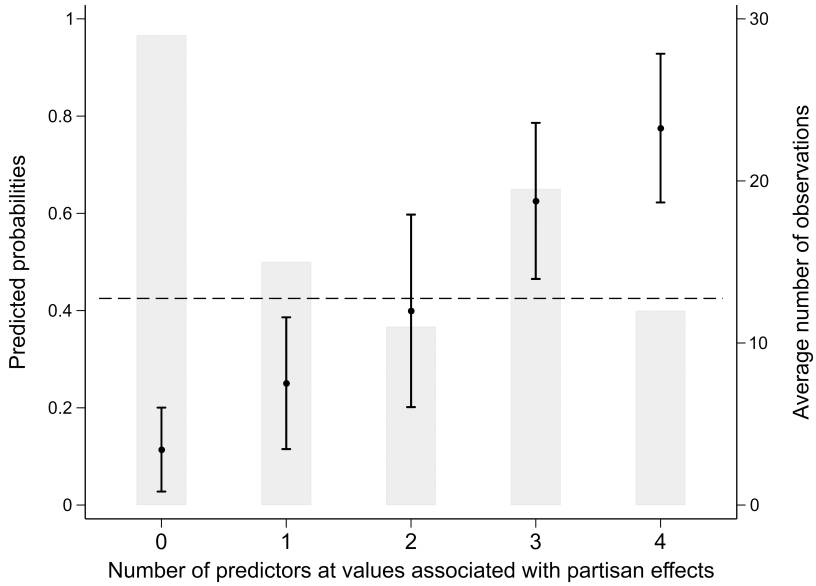
Figure 5.
Combined Predicted Probabilities for Partisan Effects on Inequality



Notes: Y-axis shows predicted probabilities with 90% confidence intervals. Results based on table 1 (model 3). All remaining variables are held constant. Bars show the number of observations, dashed line indicates the average predicted probabilities. In case of multiple combinations, values represent average effects.

variables massively codetermine the likelihood of reporting partisan effects on inequality—and they do so in theoretically predictable ways. Our systematic analysis (cf. Haselmayer and Horn 2024 for replication files) provides strong evidence that we can consolidate our understanding of the capacity of parties and cabinets to shape economic inequality if we consider the explanatory factors we have identified.

Additional Considerations and Robustness Tests

Beyond the robustness tests discussed above, we present further considerations and tests to make sure that the results presented above are robust. The results can be found in table S-C11 to table S-C17 in the supplementary material. We (1) examine whether party effects differ for inequality and redistribution, (2) show that our results hold for studies that focus on market inequality, (3) examine whether our results on top-level inequality are driven by how we aggregate and merge data into the top-versus-rest, (4) reproduce the main results based on the standardized regression coefficients, and (5) examine the impact of original studies using country-fixed effects, as well as the impact of differences in the number of cross-sectional units and the length of time series (numbers of countries divided by the number of years).[12] These are outlined in more detail below.

1. We run our original models and include a dummy indicator to capture whether the result is driven by inequality or redistribution. We then rerun the models for studies of inequality only, and run a model for the small number of models with redistribution as a dependent variable. If anything, these additional results show that the effects reported in the main analyses appear to be somewhat *stronger* for inequality alone. However, due to the small number of models with a redistribution dependent variable, we do not emphasize this result.
2. In principle, the arguments we discussed for left–right gradients apply to predistribution and redistribution. Yet, to tease out potential differences in partisan effects between inequality before and after taxes and transfers, a dummy variable is employed to capture whether inequality before or after taxes and transfers is used. This has no effect on our results. Likewise, rerunning the analyses with market inequality effects alone reproduces our findings.
3. In our analyses of inequality measures that pitch top earners versus the rest, we pool top shares with ratios that involve groups that may also be regarded as upper middle class (e.g., the 80th percentile). While this approach seems justified in light of the modest case numbers and extant research on yardsticks of inequality (Kevins et al. 2018), it is still a fair question to ask if a narrower focus on the rich makes a difference. Thus, we further disaggregate and reaggregate our measure of "top versus the rest" inequality. As the aim is to work with a narrower measure of top inequality, we only use the shares or ratios that include the top 1% to top 10%

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(compared to other segments of income distribution). Additional regression models include a "full model" with all results, as well as a reduced model where we only include studies that use income shares or ratios as the dependent variable. For both models, the results are basically the same.

4. We reproduce the core results based on standardized regression coefficients. As we pointed out above, this strategy has a number of drawbacks, to which we now add a few feasibility concerns. Most studies neither report standardized coefficients nor provide replication material or descriptive information needed to calculate them (i.e., standard deviations of inequality and partisanship). Due to nonresponses from original study authors, often for understandable reasons (some are dead, some have left academia), we could only gather results for four out of 10 regression results. As a result, the sample is not "representative" as it mostly includes newer studies, and there is a particularly low number of results from studies using the Gini coefficient as a dependent variable (eight papers). We discuss these constraints and quantify the biases in section S-C15b of the supplementary material. For all of these reasons, we are extremely cautious when it comes to the substantive relevance of the results in table S-C15a. Despite these serious reservations due to the loss of roughly half of our previously analyzed papers and several biases, the multilevel meta-regressions largely confirm the initial results. All effects point in the expected direction and (with one exception: control variables) are statistically significant too.
5. Including a control for whether the models include country-fixed effects or the number of countries divided by the length of the time series does not affect our initial results.

Implications for Future Research and the Inequality Discourse

We first discuss three substantive implications and lessons for future inequality research. On this basis, we argue that our results can be seen as good news for the heated inequality debate.

First, our results suggest that scholars should question one-size-fits-all inequality measures for economic inequality, discuss different measures (as in Huber and Stephens 2014; Huber, Huo, and Stephens 2019; Huber, Gunderson, and Stephens 2020), and vary the measure to be explained accordingly (or at least carefully defend their choices, cf. Rueda 2008). If inequality debates focus increasingly on income concentration at the top, the price of top inequality, and the role of elite affluence and influence, this should be reflected in the use of measures that *try* to capture this income concentration. A relevant test of the impact of parties on inequality should, thus, also include either top shares or ratios that involve top income groups.

Second, for most instruments parties have at their disposal, robust short-term effects are unlikely. As argued already by Bradley and colleagues (2003), given how "sluggishly" inequality measures behave, a fair test of partisan effects must take into account medium- and long-term effects. In general, such effects are more plausible than short-term party effects. While more cumulative approaches are not new in the "politics matter" literature (e.g., Huber and Stephens [2001], in a book mostly concerned with welfare policies), it is the exception that this constraint is acknowledged in inequality studies with negative findings. Such temporally nuanced approaches could also help us to better understand the time horizons of different policy approaches. It is well documented that left and right governments hold different views and pursue different policies regarding the welfare state, the role of unions, and corporatism (Jensen 2014)—aspects that in the longer term have pro-rich or pro-poor consequences—because they concern automatic equality stabilizers, the reservation wage, or wage bargaining.

Right parties rarely attack the welfare state or equal-wage policies with risky frontal assaults. Anticipating a backlash against significant cutbacks, they instead limit further redistribution, slowly defund old programs, and complement them with private alternatives (*marketization*). In parallel, they try to undermine the institutional power base of unions and welfare states more broadly (*erode and attack*, as exemplified in the reform of the Danish Ghent system).

Third, to warrant a fair test of the impact of government partisanship on inequality, the role of policy channels and policies should, by default, be considered carefully to avoid the (frequent) underestimation of party effects. Ideally, the effects of policies and their links or interactions with partisanship should be discussed a priori—otherwise, policy effects absorb party effects. Policy contexts or channels should, if possible, be further distinguished along the lines of more direct and more indirect links to parties. For instance, we know that welfare and wage policy follow a left–right gradient. The inclusion of fiscal measures, social spending, minimum wages, welfare generosity, or taxation are likely to absorb short-term party effects. By contrast, the effects that left and right parties have on corporatism and unions have less direct effects. Still, corporatist bargaining structures and unions are likely to exert an effect on predistribution (Korpi 1983; Rathgeb and Klitgaard 2022) and, as discussed above, are decisive for whether parties take the risk to fight for more or less redistribution and welfare (Jensen 2014).

In sum, our results help to consolidate research on party effects in theoretically predictable ways: If income gains at the top are considered, if we avoid including policies that are a product of left and right cabinet parties, and if we move beyond short-term effects, we find that parties have

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an effect (a predicted probability of 78%). By contrast, if these aspects are not considered, we obtain a nonfinding (11% predicted probability). The ramifications for the heated equality debate are clear: we provide a rare dose of good news, backed by broad empirical evidence. Contrary to skeptical voices (Milanovic 2016; Scheidel 2017), political conflict still influences pro-rich/pro-poor outcomes in accordance with theories about party effects. Thus, we remain critical of attempts to *endogenize* political conflict as a result of globalization or skill-biased technological change, or to frame welfare reforms and cutbacks as *necessary* reactions.

Conclusion

Political scientists may be displeased with Piketty's disciplinary trespassing. Piketty (2020, 5–7) claims that ideologies (or ideas of social justice that justify inequality) vary mostly over the longue durée, and does not consider it a main source of within- and between-country variation in inequality. Yet thus far, the discipline offers no consolidated knowledge regarding whether and how government partisanship affects economic inequality trajectories. This systematic analysis takes stock of what we can learn from over three decades of research on party effects on inequality. Covering 393 coefficients from 43 peer-reviewed articles, we find that only 37% of existing models find party effects. Following the example of the (conclusive) debate of partisan effects in comparative welfare research, we have studied the impact of three bundles or dimensions of explanatory factors: the time period under study, the type of inequality, and the conceptualization of partisanship—with three underlying subdimensions.

Based on our multivariate analysis, we show that party effects are often underestimated. Accounting for the type of economic inequality, the differentiation of short- versus long-term effects, the status of partisanship in the analysis, and the number of policy channels explains a considerable share of the variation between negative and positive findings. When we take these factors into account, instead of a 63:37 split, this yields a spread in the predicted probability of finding party effects that ranges from a mere 11% to almost 80%.

Our main conclusion is that research into partisan effects on inequality needs to reflect more critically the conceptualization, operationalization, and combination of variables to carefully motivate and document all these choices. More specifically, we emphasize three key lessons. First, it is vital to question one-size-fits-all measures of economic inequality, to consider different indicators, and to vary the explanatory variable accordingly. Second, our findings suggest that a fair test of partisan effects should take long-term effects into consideration. As short-term changes in inequality are typically of modest size (an average Gini coefficient varies by 0.1 per annum), it is demanding to detect statistically significant partisan effects on inequality over the course of a year or one legislative term. Third, our results underscore that it is crucial to carefully justify the inclusion and exclusion of the policy channels through which parties may affect inequality. We cannot expect party effects when all or most of the strategies and means parties can draw on to affect inequality are included in a *race of the variables.* Instead, future analyses should follow in the footsteps of the studies that discuss the links between parties, policies, and inequality in interactive and sequential terms (Beramendi and Cusack 2009; Pontusson, Rueda, and Way 2002; Rueda 2008; Rueda and Pontusson 2000). In light of our findings, we presume that this would help to consolidate the idea that partisanship affects inequality.

Supplementary material

To view supplementary material for this article, please visit http://doi.org/10.1017/S1537592724001786.

Data replication

Data replication sets are available in Harvard Dataverse at: DOI: 10.7910/DVN/XEDQKF

Acknowledgments

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Notes

1 Notwithstanding the contributions of earlier work, the debate about the steering capacity of party politics only started to gain real traction when, in the late 1990s, the availability of inequality data compatible with TSCS analyses made it possible to assess the role of political determinants more rigorously. The data confirmed a sharp increase in inequality in the most advanced democracies since the 1980s. This was all the more important since scholars started to better understand the political and societal costs of economic inequalities —especially under conditions of (alleged) political equality (Cagé 2020; Gilens 2012; Solt 2008; Wilkinson and Pickett 2009). Recently, similar developments have revitalized the debate about the effect of parties on economic inequality: a diversification of the available time-series data on economic inequality, new increases in inequality in many advanced democracies,

and the publication of Piketty's *Capital and Ideology*, with its much-maligned absence of political science research on the politics of inequality. In his defense, we show that there are few *consolidated* findings on the effect of parties that he *could* have cited.

2 The split is 47:53 when we consider only studies that use partisanship as the core independent variable.

3 PRT extends beyond parties and constituency preferences to include the organization and alliances between organized labor, social movements, and left parties, which were crucial for long-term incumbency and the construction of welfare states with distinct distributive outcomes. Key works that inform our understanding of PRT include Korpi (1978), Stephens (1979), and Esping-Andersen (1990). Although they cannot be discussed here in detail, they demonstrate that PRT should not be reduced to party effects without acknowledging the crucial role of the political and organizational context (for an elaboration of this critique, see Campillo and Sola 2020)—for instance, the role of unions and red–green alliances in shaping the welfare state.

4 Bandau and Ahrens (2020) provide a succinct review.

5 We are not suggesting that time is a theoretical factor. Bundling structural changes under the banner of period effects has descriptive value and period effects are so frequently discussed that we have to test them.

6 In all except 15 of the 145 significant coefficients, these results were in line with the standard predictions derived from partisan theory and PRT (Hibbs 1977; Korpi 1983) that left parties have a negative effect on (i.e., reduce) inequality. We present robustness tests where we exclude these "unexpected" partisan effects in the supplementary material (table S-C5), which corroborate the results presented below.

7 The supplementary material (tables S-C9 and S-C10) provides information on the data sources of inequality measures used in the studies (e.g., Luxembourg Income Study, OECD, Standardized World Income Inequality Database, World Inequality Database) and presents analyses adjusting for such variation.

8 See table A2 in the supplementary material for information on the content of these categories.

9 https://clarivate.com/webofsciencegroup/solutions/journal-citation-reports. We take the value from the year preceding the publication of an article. If values are not available for a given year, we use the next available one.

10 For example, this applies to Aléman (2011); Kwon, Roberts, and Zingula (2017); Mahutga, Roberts, and Kwon (2017); Nam (2020); and Sjöberg (2009).

11 Additional analyses further suggest that effects of policy channels also depend on different lags/delays in policy implementation and/or outcomes. For example, controlling for more recent social spending efforts might *moderate* short-term partisan effects on inequality, which can be tackled empirically (by modelling interaction effects; Castater [2015]; Oskarsson [2005]; Pontusson, Rueda, and Way [2002]; Rueda [2008]; Rueda and Pontusson [2000]; and Wong [2017] all report positive interaction effects). Yet *absorption* could be susceptible to post-treatment effects (e.g., by capturing policy measures that have not yet affected inequality outcomes). In contrast, cumulative measures of partisanship intend to capture the effect of broader trends in a country's political power relations, which could be obscured by more recent policies (e.g., if they contradict established political power relations). In such research designs, accounting for (short-term) policies is advisable and should increase (long-term) partisan effects (see Beramendi and Cusack 2009). While we are cautious not to overemphasize this result, additional analyses in the supplementary material align with this reasoning (table S-C8, figure S-C3). They suggest that the inclusion of policy channels should also reflect the temporal logic of (immediate vs. delayed) partisan effects.

Hardly any studies use quasi-experimental designs to address endogeneity concerns in the study of policy and inequality. Cases such as the electoral system reform in New Zealand or reforms that only affected certain municipalities in Norway in 1919 are very rare (Górecki and Pierzgalski 2023; Paulsen 2022). Even in these interesting cases, one may reason about whether the quasi-random reforms have more distant historical roots.

12 Further qualifications could be discussed regarding the use of wage dispersion, which is generally measured for full-time employees only, so these measures underestimate inequality. On the other hand, wage measures exclude pensioners, whose exclusion can lead us to overestimate the level of inequality (cf. Lupu and Pontusson [2023] for the controversy). One of the problems is that we overestimate market inequality if we consider market income as low and pensions as a transfer, although pensions are largely a function of earlier market income.

Regarding coalitions, only seven of the 43 papers even discuss coalition dynamics. Of those, only two incorporate them into their model—three if we include Iversen and Soskice's (2006) more institutionalist argument. Overall, little attention is given to the effect that executive coalition dynamics ultimately have on income inequality policies. While a substantial fraction of the papers include control variables to account for competitive veto points arising from institutional separation, variables that control for the effect of collective government coalitions are conspicuously absent. Noteworthy exceptions include the studies by Crepaz (2002), who finds that a higher number of parties in

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government coalitions leads to greater redistributive capacities, and Schaltegger and Weger (2014), who find that coalitions reduce income inequality more effectively than single-party or minority governments. Many questions regarding coalition dynamics, such as the blackmailing potential of small coalition partners, the impact of policy dictators, or the impact of portfolio allocation, are not salient.

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