**ARTICLE**

# The Populist Backlash Against Globalization:
# A Meta-Analysis of the Causal Evidence

Gábor Scheiring[1] (ID), Manuel Serrano-Alarcón[2] (ID), Alexandru Moise[3] (ID), Courtney McNamara[4] (ID)
and David Stuckler[5] (ID)

[1]Georgetown University Qatar, Doha, Qatar, [2]Dondena Research Centre on Social Dynamics and Public Policy, Bocconi
University, Milan, Italy, [3]Department of Political and Social Sciences, European University Institute, Florence, Italy,
[4]Population Health Sciences Institute, Newcastle University, Newcastle, UK and [5]Department of Social and Political Sciences,
Bocconi University, Milan, Italy
**Corresponding author:** Gábor Scheiring; Email: gs1098@georgetown.edu

## Abstract

The literature on populism is divided on whether economic factors are significant and robust causes of
populism. To clarify this, we performed the first systematic review and meta-analysis of the evidence of
a causal association between economic insecurity and populism. We combined database searches with
searching the citations of eligible studies and recently published reviews. We identified and reviewed
thirty-six studies and presented a concise narrative summary and numerical synthesis of the key findings.
Although we found significant heterogeneity in several dimensions, all studies reported a significant causal
association. A recurrent magnitude was that economic insecurity explained around one-third of recent
surges in populism. We tested for publication bias by conducting a funnel-plot asymmetry test and a dens-
ity discontinuity test of the distribution of *t*-statistics. We found significant evidence of publication bias;
however, the causal association between economic insecurity and populism remains significant after con-
trolling for it.

The explosive growth of populism has transformed politics to its core. In the post-Second World
War era, dominated by liberal consensus, populism seemed primarily a Latin American phenom-
enon. Then, in the 2000s, populism – especially its right-wing variety – began to spread in
Europe, as Fig. 1 shows. Its prominence jumped significantly with the 2014 European
Parliament election, elevating UKIP and the Front National to the centre of attention, followed
by the election of populist governments in Greece and Poland in 2015. Then, in 2016, populism
reached the heartlands of the current world order with the Brexit referendum and the election of
President Donald Trump.

The causes of populism are at the heart of the most significant political and social science
debates. One narrative contends that economic globalization resulted in real suffering among
less-educated working-class voters, catalyzing populism (Colantone and Stanig 2018c; Rodrik
2021). Another narrative contends that populism is an adverse reaction to cultural progressivism
and that economic factors are not relevant or only relevant symbolically through perceptions of
loss of cultural status (Margalit 2019; Norris and Inglehart 2019; Sides et al. 2019). Even though
the evidence suggests that the generational change argument suggested by the canonical book of
Norris and Inglehart (2019) does not hold empirically (Grasso et al. 2019; Schäfer 2022), the
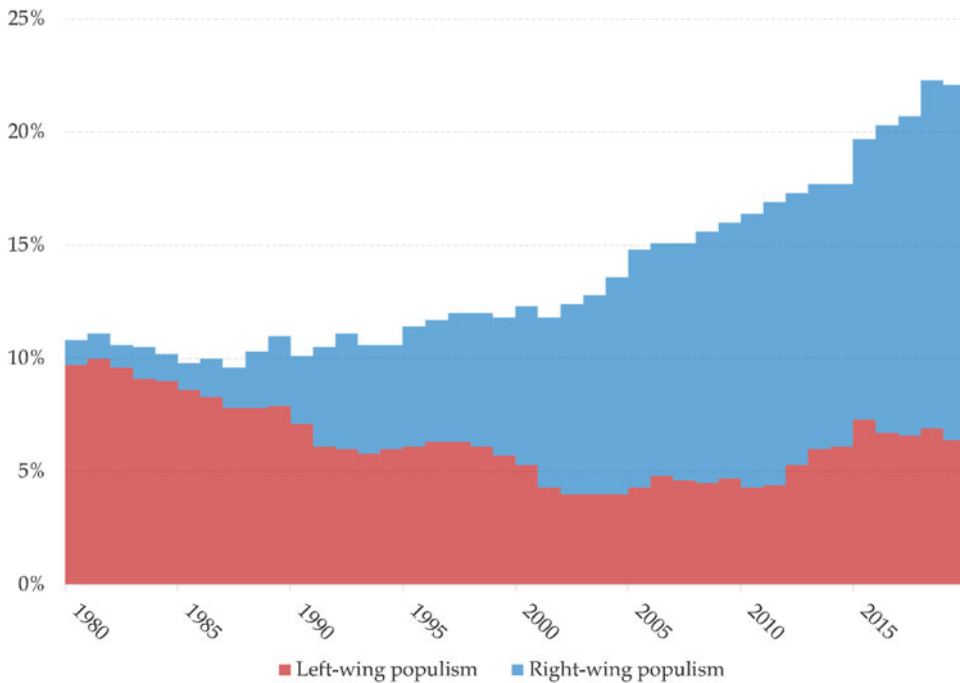
**Figure 1.** The explosion of populism in Europe.
*Notes:* Aggregated populist votes 1980–2019 in 33 European countries, including the 28 members of the EU plus Iceland, Norway, Switzerland, Serbia, and Montenegro. Non-democracies and semi-authoritarian countries are excluded. Source Timbro Authoritarian Populism Database, https://populismindex.com/ (accessed 18 February 2023)

cultural narrative nevertheless seems to be particularly influential. In their review of the structural determinants of right-wing populist voting, Amengay and Stockemer (2019, 35) concluded that the 'results do not lend support to the economic hardship breeds extremism hypothesis'. Walter (2021, 9) concluded her review by saying, 'the evidence that those directly hurt by globalization are more likely to vote for anti-globalization parties or proposals is mixed'. Finally, Berman (2021, 75–76) reached a similar conclusion, highlighting that 'the evidence linking individual economic grievances to populist voting is not particularly strong'.

We enter the debate on the populist backlash against economic globalization by presenting the first systematic review and meta-analysis of the causal evidence that economic insecurity significantly catalyzes populism. A key factor contributing to the widespread perception concerning the lack of association between economic factors and populism is the lack of clarity about the definition of economic factors. This vagueness manifests in the one-sided focus on individual-level economic hardships in the past and the neglect of contextual-level economic shocks and the temporal dimension; that is, changes in economic factors as opposed to their level.

However, a lack of clear, cross-sectional, static association between past material deprivation and populism does not imply that economic factors are unimportant. As Mughan and Lacy (2002, 513) noted, economic insecurity is 'a novel form of economic discontent that is distinctive … from retrospective evaluations of short-term economic performance'. We need a more fine-grained approach to the economics of populist demand, such as economic insecurity. Recent studies have shown that economic insecurity 'has risen steadily since the mid-1980s for virtually all sub-groups of Americans' (Hacker et al. 2014, S5). Insecurity is also widespread in Europe, extending 'across income groups and occupational classes, reaching into the middle classes' (Ranci et al. 2021, 539). Therefore, we focus on economic insecurity as the causal factor.

Systematic reviews (Dacombe 2018) and meta-analyses (Borenstein et al. 2009) are increasingly popular in political science; they explore topics such as immigration and support for anti-immigration parties (Cools et al. 2021), the effect of framing on political attitudes and behavior (Amsalem and Zoizner 2022), and the effect of the size of the legislature on public spending (Freire et al. 2023). Although the term systematic review is sometimes used to denote narrative reviews conducted with a systematic intent (Obinger and Petersen 2017), systematic reviews differ from narrative reviews in important ways. A systematic review 'locates existing studies, selects and evaluates contributions, analyzes and synthesizes data, and reports the evidence in such a way that allows reasonably clear conclusions to be reached about what is and is not known' (Denyer and Tranfield 2009, 672). By conforming to the standards expected of primary research, systematic reviews can significantly reduce bias, improve transparency, and be subject to replication (Dacombe 2018, 149). They also help accumulate knowledge and 'provide a basis for planning and interpreting new primary research' (Gough et al. 2012, 3).

We combine an academic database search with a backward citation search of the eligible studies and a review of recently published narrative reviews. We include studies focusing on populist political behavior as the outcome and economic insecurity as the treatment and follow a research design dedicated to identifying causality. We reviewed and synthesized thirty-six studies fulfilling our inclusion criteria, extracting 144-point estimates and studying their precision. We numerically summarize the main results and formally test for publication bias. All thirty-six quasi-experimental studies report a robust causal link between economic insecurity and populism. We also identify significant geographic, methodological, and disciplinary divisions in the literature; the association is robust across these dimensions of heterogeneity. We find significant evidence of publication bias, but the causal association between economic insecurity and populism remains significant after controlling for it.

Our paper proceeds as follows. First, we describe our systematic search strategy and define populism, economic insecurity, and quasi-experiments. After describing the process of inclusion and exclusion, we present the results in three sections. First, we overview the main results and the geographic, methodological, and disciplinary divisions. Second, we synthesize the key results using the 'vote counting' approach, which is suitable for an exploratory numerical summary of findings from widely heterogeneous studies. Third, we perform formal statistical tests for publication bias. We start by testing the density discontinuity of the distribution of t-statistics. Next, we estimate a partial correlation coefficient to handle heterogeneity in the treatment and outcome variables and conduct a funnel plot asymmetry test. Finally, we present the conclusions.

## Systematic Review Methodology

### Search Strategy

We follow the updated 2020 version of the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines (Page et al. 2021). These guidelines set standards for robust, transparent, and replicable systematic reviews. Here, we summarize the key search parameters and present a detailed overview of the search strategy in Supplement S1.

We searched three academic databases: Web of Science, Scopus, and EBSCO Discovery Service. The searches occurred between 26 January and 6 February 2022 and were repeated between 19 and 25 July 2022. We included a combination of keywords covering the outcome of interests (populism), the treatment (economic insecurity), and the research design (causal inference). Table S1.1 in the Supplement (p.1) shows the whole search strategy verbatim.

We searched for studies that analyze populist political behavior in terms of outcome. Much of the culture versus economy debate is centred on far-right or right-wing populism. However, left-wing populism is also important, especially in Latin America and Southern Europe. Some posit that different forms of populism might be competitive alternatives. For example, the availability of left-wing populism might reduce the demand for right-wing populism. Therefore, we

focus on populism in general, not just right-wing populism. This approach allows us to analyze whether the literature identified significant differences in the determinants of left-wing and right-wing populism.

The most conventional approach in the literature identifies *populism* as a 'thin-centred ideology' that frames the fundamental political cleavage between the elite and the pure people (Mudde 2007). The strengths of this approach are that it is agnostic about the causes and the specific policy content of populism; is broad enough to capture a wide range of the phenomenon, such as left- and right-wing; inclusionary or exclusionary populism; and specific enough to exclude non-populism. Despite the ubiquity of this definition, empirical researchers often include parties influenced by the notion of *anti-establishment politics*, a term closely related to populism (Barr 2009; Mudde 2007).[1] Our aim is not to clarify the concept of populism or decide between different approaches but to capture a wide variety of populist outcomes to reflect the heterogeneity of the literature. Thus, our definition includes the radical left, the radical right, and historical precedents of populism, such as fascism in Europe or Latin-American import-substitution populism, such as Peronism (Guriev and Papaioannou 2022, 11–13).

We started from a 'nearest neighbor' systematic review and extracted the search string from Hunger and Paxton (2022), whose search strategy relied on the word stem 'populis*'. In addition to populism, we included keywords that cover anti-establishment politics and different versions of the radical right and the radical left to cover different valances of populism, even if the word populism is not mentioned in the abstract or title.

Economic insecurity captures multiple dimensions of vulnerability and relies on a dynamic focus to analyze change across time (Whelan and Maître 2005). We define economic insecurity as the 'risk of economic loss faced by workers and households as they encounter the unpredictable events of social life' (Western et al. 2012, 342). Western et al. argue that these changes are linked to adverse events or external shocks (originating in the labor market, the housing market, the financial market, over-indebtedness, health events, etc.). Thus, we include search terms to cover various forms of material hardship and other sources of economic insecurity, such as 'economic uncertainty', 'financial uncertainty', or 'labor market uncertainty'. Because 'citizens' economic worries largely track exposure to substantial economic shocks' (Hacker et al. 2013, 23), we also used keywords that capture upstream shocks, such as 'trade shocks', 'import exposure', austerity, or robotization.

Studies following a causal inference research design (also called 'natural experiments' or 'quasi-experiments') 'aim to make causal inferences about the effects of an exposure or intervention[2] of interest on outcomes by exploiting exogenous variation in treatment assignment' (Glanville et al. 2017, 68). Studies following a causal inference research design try to identify causality and mimic experimental designs by identifying an exogenous ('as good as random') source of variation in treatment assignment. However, 'quasi-experiments' are not experiments; they rely on several identifying assumptions that are sometimes credible and sometimes more demanding.[3] Cools et al. (2021) conducted a systematic review of the causal evidence on populist right voting but did not provide an exact search string, so we extracted the search string from El Soufi and See (2019). We extended their string with other keywords based on causal inference textbooks (Cunningham 2021; Huntington-Klein 2022) – instrumental variables, difference-in-differences,

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[1]According to Barr's (2009, 44) widely cited definition, 'anti-establishment politics refers to a rhetorical appeal based on opposition to those who wield power within the state'. Relying on this concept of anti-establishment politics, he defines populism as a 'mass movement led by an outsider or maverick seeking to gain or maintain power by using anti-establishment appeals and plebiscitarian linkages'.

[2]We refer to these exogenous exposures or interventions as 'treatment'.

[3]We do not provide a systematic quality assessment of study designs. However, we flag designs that require more demanding assumptions to isolate causality. The more demanding these assumptions, the more threat the identification strategy faces.

regression discontinuity, synthetic control designs, survey experiments, field experiments, and randomized controlled trials.[4]

Constructing the search this way runs the risk of missing studies that do not refer to their causal design in the title or abstract. We minimized this risk by relying on two additional search strategies described below. First, we conducted a backward citation search based on the bibliography of the seventeen studies we identified after screening and eligibility. Second, we reviewed the bibliographies of recently published narrative reviews on populism.

### Inclusion and Exclusion

The PRISMA flow diagram in Fig. 2 describes the screening process. Our initial database search identified 1,234 studies: 483 through EBSCO Discovery, 461 through Scopus, and 290 through Web of Science. Of these, we identified 446 duplicates using automated identification in EndNote. This left 788 studies for screening. In addition, we identified 625 records through citation searching (553 on Scopus, 62 on Web of Science, and 6 through review of reviews) and 4 further studies through Google Scholar. These 625 studies were included in the second round of screening based on the inclusion and exclusion criteria detailed below. We included studies if they fulfilled the following inclusion criteria:

(a) the outcome variable is directly related to populist political behavior or orientation,
(b) the treatment is related to economic insecurity broadly defined,
(c) the study identifies a causal association through a quasi-experiment, field experiment, or survey experiment,
(d) the study relies on original, primary empirical research.

Screening the titles and abstracts of the 788 studies identified through primary database search, we excluded 702 studies. This left 86 studies whose full texts were retrieved and assessed for eligibility by reading the whole study. At this stage, we excluded 69 studies. However, we identified 17 eligible studies during the first round.

As mentioned above, we also identified 625 studies for screening through a backward citation search and a review of reviews. After screening and eligibility, we searched the bibliographies of the 17 studies we identified. We used Scopus and Web of Science to extract the references cited and found 553 further studies in Scopus and 62 in Web of Science. We reviewed the bibliographies of recently published reviews on populism (Berman 2021; Guriev and Papaioannou 2022; Hunger and Paxton 2022; Mansfield et al. 2021; Rodrik 2021; Scheiring 2021; Tuğal 2021; Walter 2021) and identified 6 further studies for screening. Finally, we identified 4 studies through a Google Scholar citation search. Of these 625 studies, we excluded 541 during screening on abstract and title. We retrieved 84 full-text studies for a detailed analysis of eligibility. Of these, we excluded 65. This way, the citation search and review of reviews yielded 19 eligible studies for inclusion.

We extracted the main study parameters into a summary table, which included author(s), title, countries analyzed, sampled units, sample size, study time, primary outcome, treatment, level of

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[4]We include difference-in-differences and synthetic control designs because they are textbook examples of quasi-experiments. Both provide plausible causal inferences under a transparent and indirectly testable identification assumption – the parallel trends assumption. However, we excluded studies that analyzed panel data with continuous treatment and outcome variables using fixed effects approaches. Such continuous indicators make it difficult to reliably separate pre- and post-treatment periods as well as treatment and control groups while not providing a transparent identification assumption. This makes it more difficult to rule out confounding by unobserved heterogeneity. Although causal inference textbooks discuss matching, we did not include studies that only used matching. Matching generates treated and untreated groups ex-post using observed characteristics for assignment while leaving any potential unobserved heterogeneity unaddressed. Thus, they do not significantly move beyond multivariate regressions.
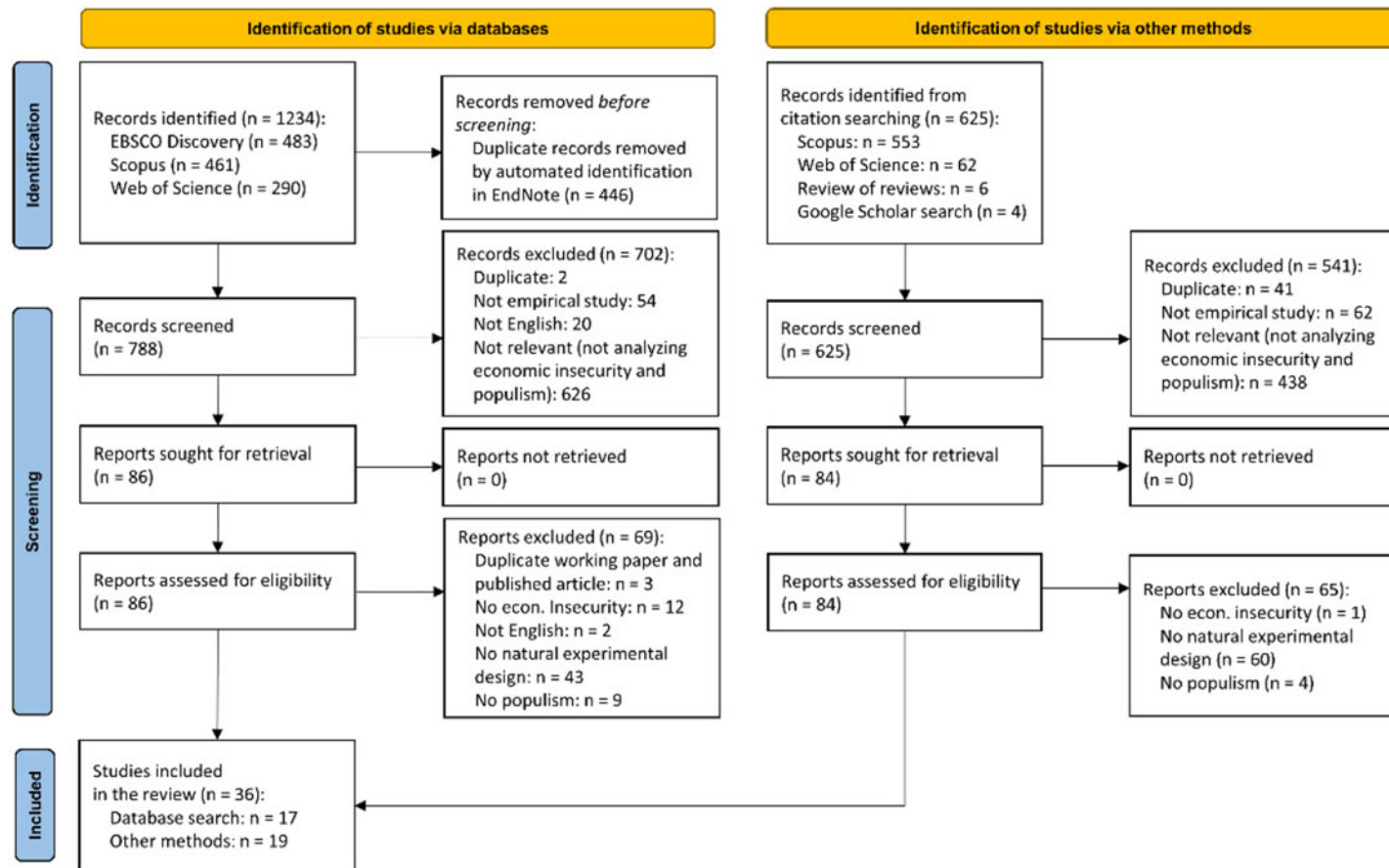
**Figure 2.** PRISMA flow diagram.

the treatment and outcome variable, experimental design, data structure, modelling details, approach to missingness, and summary results, among others. This detailed review table is available as part of the replication data. Altogether, we found thirty-six eligible studies, as summarized in Table 1.

## Overview of the Literature

We found significant heterogeneity in several dimensions. Most studies analyze Western Europe and North America, where most authors are located. Eastern Europe outside the EU, Latin America (except Brazil), Asia, and Africa are neglected, as Tuğal (2021) and Mansfield et al. (2021) noted.

This geographic skew leads to a neglect of specific sources of economic insecurity. Shocks related to robotization are concentrated in the most advanced economies (Anelli et al. 2021; Frey et al. 2018). Eurozone countries were more exposed to import shocks than non-Eurozone European countries (Guiso et al. 2019). However, this does not mean that other forms of globalization shock were not crucial in emerging economies. Ahlquist et al. (2020) and Gyongyosi and Verner (2022) show that foreign currency debt shocks were important factors behind the rise of right-wing populism in Eastern Europe, while Barros and Santos Silva (2019) show that male-specific labor market insecurity fuelled support for Bolsonaro.

Most studies analyze the populist radical right in terms of the outcome variable. A smaller group of studies explores the whole populist spectrum (both left and right), some studies analyze populist attitudes, but only one concentrates on the populist left. Instrumental variables dominate the field in primary causal inference design, but difference-in-differences and regression discontinuity designs are also popular. Economists strongly prefer contextual-level measurements of populism, while political scientists are more open to individual-level variables. These disciplinary differences might influence substantive findings. Berman (2021, 75–6) recently noted that economic grievance-based explanations are stronger at the contextual level, while cultural explanations are stronger at the individual level. Supplement S2 (pp.3–6 of the online Supplement) presents a detailed overview of the studies' geographic, methodological, and disciplinary heterogeneity.

To synthesize the substantive results of the review, Fig. 3 presents a conceptual overview of the multi-channel causal relationship between economic insecurity and populism based on the reviewed studies (with the number of studies assessing the specific treatment in parentheses). Regarding the experimental treatment, the source of economic insecurity and import exposure ('the China shock') stands out in the number of studies that analyze it. Labor market uncertainty is the second most often analyzed cause. Other treatment variables focus on austerity, redistribution, automation, foreign currency debt shocks, subjective economic hardship, bank failure, and housing demand shock. The literature also identified important moderators, such as class, education, age, sex, race/ethnicity, political supply, and social security policies. These moderators influence how the lived experience of economic shocks translates into populism. Supplement S3 (pp.10–16) presents a detailed narrative overview of the literature focusing on the main causes of the populist backlash.

All thirty-six studies report a robust causal link between economic insecurity and populism. Several studies report a strong effect. A recurring effect is that economic shocks and economic insecurity explain around one-third of recent surges in populism. Several studies also showed that higher economic security through redistribution dampens voters' enthusiasm for populism (Albanese et al. 2022; Crescenzi et al. 2020). As Guiso et al. (2019, 101) concluded, 'if one wants to defeat populism, one must first defeat economic insecurity'.

Some studies suggest that changes in political supply influence how economic insecurity translates into political outcomes. For example, once the Five Star Movement appeared in Italy, it successfully attracted the votes of trade victims previously channeled by radical-right parties. Thus, when presented with an alternative, victims of trade shocks do not necessarily choose a radical

**Table 1.** Overview of the studies

| Study | Setting | Outcome & operationalization | Treatment | Design | Summary of results |
|---|---|---|---|---|---|
| Ahlquist et al. (2020) | Poland, 2015 | Populist radical right; Vote choice; Binary | Foreign currency debt shock | Survey experiment; Matching; | Individuals exposed to the 2015 surprise revaluation of the Swiss franc were more likely to demand government support and vote for the largest opposition party, the national-populist PiS. |
| Albanese et al. (2022) | Italy, 2008–2013 | Populist radical right & Left; Political orientation; Score | Redistribution | Reg. discont.; Instrumental variable | Higher EU financing in the 2008–2012 period led to lower support for populist parties in the 2013 general election. |
| Algan et al. (2017) | European Union, 2000–2016 | Populist radical right & left; Vote share; Change | Labor market uncertainty | Instrumental variable; Diff-in-diff | There is a strong relationship between increases in unemployment and voting for non-mainstream parties, especially populist ones. |
| Anelli et al. (2021) | Western Europe, 1999–2015 | Populist radical right; Vote choice; Binary | Automation | Instrumental variable | Individuals exposed to automation display higher support for the radical right, independently of cultural factors. Automation also increases nativism, status threat, and cultural traditionalism. |
| Autor et al. (2020) | US, 2000–2016 | Populist radical right; Vote share; Change | Import exposure | Instrumental variable | Import exposure increased the campaign contributions by more ideologically extreme donors, the popularity of conservative media, conservative viewpoints, Republican nominees after 2010, and Trump in 2016. |
| Baccini and Sattler (2021) | Western Europe, 1991–2018 | Populist Radical Right & Left; Political orientation; Score | Austerity | Difference-in-Differences | Austerity increases support for radical-right populism in economically vulnerable regions. Furthermore, it catalyzes a cultural backlash: negative attitudes against migrants and minorities and higher support for conservative values. |
| Baccini and Weymouth (2021) | US, 2004–2016 | Populist Radical Right; Vote share; Change | Labor market uncertainty | Instrumental variable | White manufacturing layoffs led to a decline in the Clinton vote and an increase in the Trump vote, while Non-White manufacturing layoffs had the opposite effect during the 2016 Presidential election. |
| Barone and Kreuter (2021) | Italy, 1992–2013 | Populist radical right & left; Vote share; Change | Import exposure | Instrumental variable | Trade globalization increased support for populist parties, invalid votes, and abstentionism. Redistribution mitigated the political consequences of the trade shock because it alleviated economic distress. |
| Barros and Santos Silva (2019) | Brazil, 2014–2018 | Populist radical right; Vote share; Change | Labor market uncertainty | Instrumental variable | Male-specific labor market shocks increase support for Pres. Jair Bolsonaro, while female-specific shocks have the opposite effect. |
| Caprettini et al. (2021) | Italy, 1946–1992 | Populist radical left; Vote share; Change | Redistribution | Reg. discont.; Instrumental variable | The large-scale land redistribution led to persistent electoral benefits for the Christian Democrats (DC), the party that promoted the reform, and persistent electoral losses for the Communist Party (PCI). |
| Caselli et al. (2020) | Italy, 1994–2008 | Populist radical right; Vote share; Change | Import exposure | Instrumental variable | Exposure to import competition from China has positively contributed to the electoral outcomes of far-right parties. |

(*Continued*)

**Table 1.** (*Continued.*)

| Study | Setting | Outcome & operationalization | Treatment | Design | Summary of results |
|---|---|---|---|---|---|
| Caselli et al. (2021) | Italy, 2001–2013 | Populist radical right & Left; Vote share; Change | Import exposure | Instrumental variable | Import exposure, robotization, and immigration increased the votes for far-right parties between 2001 and 2008. After 2008, only robotization had such an impact, while immigration increased the votes for the 5 Star Movement. |
| Cavaille and Ferwerda (2023) | Austria, 2002–2006 | Populist Radical Right; Vote share; Change | Housing demand shock | Difference-in-Differences | Municipalities most exposed to public housing congestion induced by the extension of public housing benefits to non-EU residents led to a significant increase in radical-right votes. |
| Chen (2020) | US, 2006–2014 | Populist radical right & Left; Vote share; Level | Labor market uncertainty | Diff-in-diff; Matching | Unemployment during the Great Recession increased distrust in companies, the preference for redistribution, and voting for Sanders in primaries. The immigrant influx increased anti-immigration attitudes and voting for Trump. |
| Colantone and Stanig (2018a) | Western Europe, 1988–2008 | Populist attitudes; Political orientation; Score | Import exposure | Instrumental variable | Import shocks lead to less democratic, less liberal, and more authoritarian attitudes. People in regions with stronger import shocks are more concerned with the 'cultural threat' posed by immigrants. |
| Colantone and Stanig (2018b) | UK, 1990–2007 | Populist radical right; Vote share; Level | Import exposure | Instrumental variable | The 'Leave' share in the Brexit referendum was systematically higher in regions more exposed to the Chinese import shock. Import exposure also led to an increase in anti-immigration attitudes. |
| Colantone and Stanig (2018c) | Western Europe, 1988–2007 | Populist radical right; Political orientation; Score | Import exposure | Instrumental variable | A stronger import shock leads to (1) an increase in support for nationalist and isolationist parties, (2) an increase in support for radical-right parties, and (3) a general shift to the right in the electorate. |
| Crescenzi et al. (2020) | UK, 2016 | Populist radical right; Vote share; Level | Redistribution | Regression discontinuity | Citizens living in areas eligible for the highest amount of EU Structural Funds and experiencing improvements in their labor market were more inclined to express a pro-Europe ('Remain') vote in the referendum on Brexit. |
| Dehdari (2022) | Sweden, 2006–2010 | Populist radical right; Vote share; Change | Labor market uncertainty | Instrumental variable | The study finds that one layoff notice among low-skilled native-born workers increases, on average, support for the Swedish radical-right party, the Sweden Democrats, by 0.17–0.45 votes. |
| Dippel et al. (2021) | Germany, 1987–2009 | Populist radical right; Vote share; Change | Import exposure | Instrumental variable | Exposure to imports from low-wage countries increased the support for the far-right between 1987 and 2009. Low-skilled manufacturing workers were the most affected. There is no robust association with the far-left vote shares. |

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|---|---|---|---|---|---|
| Doerr et al. (2022) | Germany, 1930–1932 | Populist radical right; Vote share; Change | Bank failure | Diff-in-diff; Matching | Danat Bank's failure (targeted by anti-Jewish propaganda) led to declining incomes & increased Nazi votes. Dresdner Bank's failure (no Nazi propaganda) had a similar income effect but no increase in Nazi votes. |
| Ferrara (2022) | US, 2008–2016 | Populist radical right; Political orientation; Score | Import exposure | Instrumental variable | Voters in districts more exposed to Chinese imports are more likely to favor racial and religious in-groups (Whites and Christians) against ethnic, religious, and sexual minorities and vote for the Republican Presidential candidate. |
| Fervers (2019) | Germany, 2003 | Populist radical right & left; Vote choice; Binary | Austerity | Regression discontinuity | The Harz IV reforms led to decreasing satisfaction with democracy and political participation and increased support for non-established right-wing parties and non-established left-wing parties less robustly. |
| Fetzer (2019) | UK, 2000–2016 | Populist radical right; Vote share; Multiple | Austerity | Diff-in-diff; Event study | Electoral district and individual-level results suggest that those exposed to welfare cuts experienced large and precisely estimated increases in their tendency to express support for UKIP and support 'Leave' in the 2016 Brexit referendum.[14] |
| Fetzer et al. (2019) | UK, 2008–2016 | Populist radical right; Vote share; Level | Austerity | Diff-in-diff; Matching | Housing benefits cuts led to an affordability shock and significant housing insecurities. It also decreased the registration and turnout in the 2016 EU referendum and increased the support for 'Leave.' |
| Frey et al. (2018) | US, 2011–2015 | Populist radical right; Vote share; Change | Automation | Instrumental variable | Victims of the robot revolution have a higher propensity to opt for radical political change: electoral districts with higher exposure to robots were significantly more likely to support Trump. |
| Galofré-Vilà et al. (2021) | Germany, 1930–1933 | Populist radical right; Vote share; Change | Austerity | Instrumental variable; Reg. discontinuity | Areas more affected by austerity had higher Nazi vote shares. Most of the effect of austerity is driven by health and housing expenditure cuts. Mortality is a significant mediator of the impact of austerity on Nazi Party support. |
| Gozgor (2022) | European Union, 1980–2020 | Populist radical right & left; Vote share; Change | Labor market uncertainty | Instrumental variable | Higher economic uncertainty measured by the World Uncertainty Index increases the support for total populism, right-wing populism, and left-wing populism to a smaller degree. |
| Guiso et al. (2019) | European Union, 2000–14 | Populist radical right; Vote share; Level | Import exposure | Instrumental variable | Import exposure and the financial crisis have boosted populism in Eurozone (EZ) countries but not in non-EZ countries. The policy straitjacket imposed by the EZ increases economic insecurity, which fuels populism. |
| Gyongyosi and Verner (2022) | Hungary, 1998–2014 | Populist radical right; Vote share; Multiple | Foreign currency debt shock | Instrumental variable; Diff-in-diff | Household debt increase due to the foreign currency exchange rate shock was a significant cause behind the electoral success of the radical-right Jobbik party in Hungary in 2010. |
| Lechler (2019) | European Union, 1994–2014 | Populist radical right; Vote share; Change | Labor market uncertainty | Instrumental variable | Declining employment increases Eurosceptic voting. This effect is particularly strong for unemployed and low-skilled workers in regions with a high share of migrants from other EU member states. |

(Continued)

**Table 1.** (*Continued.*)

| Study | Setting | Outcome & operationalization | Treatment | Design | Summary of results |
|---|---|---|---|---|---|
| Lin and Xi (2022) | European Union, 2010–2014 | Populist Radical Right; Vote share; Change | Import exposure | Instrumental variable | Exposure to the import shock from developing countries increases the support for right-wing parties and economically far-right parties but decreases the support for culturally far-right parties. |
| Malgouyres (2017) | France, 1995–2012 | Populist radical right; Vote share; Change | Import exposure | Instrumental variable | Industrial decline generated by import exposure increases far-right vote share. The effect increased, benefitting the far-right mainly from 2007 to 2012. |
| Milner (2021) | Western Europe, 1990–2018 | Populist radical right & left; Vote share; Level | Import exposure | Instrumental variable | Import exposure and automation are associated with growing vote shares for extreme right parties. The financial crisis enhanced support for populist right parties and eroded support for mainstream left parties. |
| Rhodes-Purdy et al. (2021) | US, 2020 | Populist attitudes; Political orientation; Score | Subjective economic hardship | Survey experiment | Negative economic priming induced cultural discontent. Anger mediates the effect of economic decline on cultural discontent. Cultural discontent, in turn, catalyzes populism. |
| Siedler (2011) | Germany, 1990–2004 | Populist radical right; Vote choice; Change | Labor market uncertainty | Sibling differences | Parental unemployment during late childhood has a significant positive effect on right-wing party affinity later in life. |
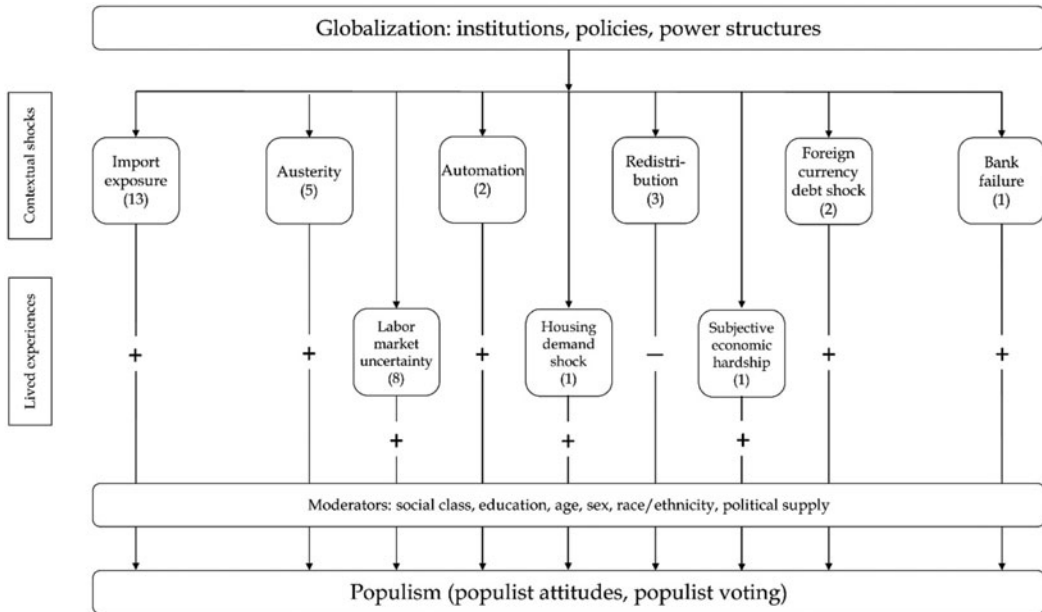
**Figure 3.** The multi-channel causal relationship between economic insecurity and populism.

right populist party (Caselli et al. 2021). The United States offers an opposite example. Here, the Democrats benefited from the discontent of trade victims during the presidential elections before 2012. However, since 2012, Republicans have become more successful in attracting the victims of trade shocks, leading to Trump's 2016 victory (Autor et al. 2020; Che et al. 2016). This aligns with the extant literature showing that centre-left parties are particularly vulnerable to moving toward the right in economic policy (Bagashka et al. 2022; Fervers 2019; Mudge 2018; Snegovaya 2024). Without left-wing progressive policies and narratives, radical-right populists can mobilize dissent through culturally symbolic issues, using the nation as an imagined solidarity community that protects against globalization (Berman and Snegovaya 2019; Scheiring 2020).

Six studies find that the cultural backlash is not an alternative mechanism but a mediator. For example, Anelli et al. (2021) find that robotization increases nativism, status threats, and cultural traditionalism in Europe. Autor et al. (2020) find that import exposure in the United States (US) in the 2000s increased the popularity of the conservative media and conservative viewpoints. Using an innovative survey experiment, Rhodes-Purdy et al. (2021) show that economic decline induces cultural discontent mediated by economic anger, while cultural discontent, in turn, catalyzes populism. Colantone and Stanig (2018a) find that economic insecurity through import exposure leads to a shift towards anti-liberal values in the domain of politics but not in the extra-political domain, such as child-rearing. This result implies that the populist backlash is neither a result of a persistent authoritarian outlook nor an expression of a general shift in the direction of the authoritarian personality. Instead, Colantone and Stanig (2018a) argue that scepticism towards liberal values and liberal democracy is a political manifestation of distress driven by economic insecurity. Of course, the existence of such mediation, where economic insecurity is the primary cause, does not mean that cultural factors are not important; they can be independent primary causes, too. It means that juxtaposing economy and culture is less productive than exploring their interactions.

Studies following causal inference designs are considered among the highest quality studies in the universe of observational studies. Therefore, we did not perform a systematic quality assessment. However, some studies employ less effective identification strategies that require more demanding assumptions. The field is riddled with methodological controversies and frequent

innovations. Ultimately, the credibility of causal inference designs depends on the authors' ability to prove the exogeneity of the treatment. As consumers of empirical research, we should always consider these design limitations.

It is rare for a systematic review to yield such conclusive results, with all studies pointing in the same direction. This unanimity might reflect publication bias. Publishing only results that find a significant effect disturbs the quality and reliability of the overall picture emerging from the literature (Brodeur et al. 2020; Thornton and Lee 2000). To maximize the chances of finding null results, we searched the available grey literature, including working papers and PhD theses that are more likely to report null results than published articles or conference papers (Thornton and Lee 2000, 211). However, non-published papers also all report a significant positive effect of economic insecurity on populism.

## Meta-Analysis

Because of the considerable heterogeneity in study designs, populations, treatments, and outcomes, comparing the effect sizes across studies relying on a standard meta-regression is not appropriate. To tackle this challenge, we rely on two different approaches. First, we quantify the results using the widely used but somewhat naïve 'vote counting' approach, which counts the number of significant and insignificant associations (Amengay and Stockemer 2019; Bushman and Wang 1994; Geys 2006). While this method has important limitations (Stanley and Doucouliagos 2012, 43–5), it allows for an exploratory numerical summary of results across a wide range of heterogeneous studies.

Second, to formally test for publication bias and further analyze the heterogeneity in the results, we rely on a standardization procedure borrowed from recent meta-analysis literature by calculating a partial correlation coefficient (Borenstein et al. 2009; Brodeur et al. 2020; Filomena and Picchio 2023; Gechert and Heimberger 2022; Jin et al. 2015; Stanley and Doucouliagos 2014). We evaluate publication bias by conducting a funnel-asymmetry and precision-effect test and analyzing the density discontinuity of t-statistics.

We constructed a meta-dataset by including the main estimates reported in the thirty-six studies, which included 144 estimates from the studies (see Supplement S4 for the inclusion criteria). The number of estimates per study ranged between a minimum of 1 and a maximum of 10. For each estimate, we extracted the t-statistic and normalized their sign so that a positive sign means an increase in populism derived from increased economic insecurity.[5]

### 'Vote Counting' Approach Statistics

We counted the number of significant positives (sometimes called 'successes'), significant negatives (sometimes called 'anomalies'), and insignificant associations (also called 'failures') and calculated the share of positives (sometimes called the 'success rate' of the specific variable). The higher the share of positives, the more confident we can be in uncovering a 'true' association. We summarize the results of this 'vote counting' approach in Table 2. While each study reports at least one model that shows a significant[6] positive association, some estimates diverge from this overall picture. Of the 144 estimates, 96 show a significant positive association, 7 show a significant negative association, and 41 show no significant association. The negative anomalies come from various sub-samples, revealing interesting patterns of heterogeneity.

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[5]For example, if the outcome variable was the Remain vote share (the opposite of populism) during the Brexit referendum, we changed the sign of the coefficient so that it reflected the logic of economic insecurity, which positively correlated with populism. Similarly, if the treatment was redistribution, we changed the sign of the coefficient, because redistribution is the opposite of insecurity.

[6]Here we consider significant only those estimates where the *t*-value is equal to or bigger than 1.96. In some cases, the authors reported p < 0.1 as significant.

Table 2. Summarizing the effect of economic insecurity on populism

|  | No link | Positive | Negative | No of estimates | Share of positives (%) |
|---|---|---|---|---|---|
| Austerity | 4 | 21 | 0 | 25 | 84 |
| Automation | 2 | 3 | 0 | 5 | 60 |
| Bank failure | 2 | 3 | 0 | 5 | 60 |
| Foreign currency debt shock | 1 | 4 | 0 | 5 | 80 |
| Housing demand shock | 0 | 2 | 0 | 2 | 100 |
| Import exposure | 17 | 28 | 2 | 47 | 60 |
| Labor market uncertainty | 13 | 24 | 5 | 42 | 57 |
| Redistribution (reversed) | 2 | 3 | 0 | 5 | 60 |
| Subjective economic hardship | 0 | 8 | 0 | 8 | 100 |
| Total | 41 | 96 | 7 | 144 | 67 |

For example, Guiso et al. (2019) find that import exposure boosts populism in Eurozone countries but find a significant negative link in non-Eurozone countries (mainly Eastern Europe).[7] Another interesting example is Baccini and Weymouth (2021), who find that non-white voters' exposure to deindustrialization led to higher support for the Democrats in 2016, while white voters exposed to deindustrialization turned to the Republicans to support Trump. These differences show that identity politics and economic dislocations interact in producing support for populism. Barros and Santos Silva (2019) also find that gender identity interacts with labor market insecurity in Brazil: men exposed to labor market insecurity were significantly more likely to vote for Bolsonaro. The opposite is true for women.

Table 2 also shows that each treatment has a share of positives exceeding 50 per cent, with the average share reaching 67 per cent. In other words, the modal category is 'success' for each treatment. The sample size is small because the studies/treatments were not randomly selected. Consequently, the differences are significant across treatments, and we must be cautious when comparing the share of positives across the treatment categories. The variability across the treatments could reflect a mix of true effect heterogeneity, publication bias, specification bias, or differences in sampling. Automation, bank failure, import exposure, labor market uncertainty, and redistribution (reversed) seem more ambiguous predictors of populism than economic insecurity on 'average'. Austerity, foreign currency debt shock, housing demand shock, and subjective economic hardship seem less ambiguous predictors to predict populism more robustly.

Table 3 shows the results broken down by the type of populism. Altogether, 15 models were estimated for left-wing and 100 for right-wing populism. The remaining 29 estimates do not differentiate between left and right. Overall, economic insecurity seems to predict right-wing populism more robustly (percentage of positives 65 per cent) than left-wing populism (40 per cent). Automation, labor market uncertainty, and redistribution might seem most relevant for left-wing populism. Austerity, foreign currency debt shocks, housing demand shocks, and import exposure seem more relevant for right-wing populism. The same caveat applies: we must be careful when comparing the share of positives across the treatment categories.

## Publication Bias

Figure 4 plots the density function of t-statistics of the estimates included in the meta-analysis. If there is no publication bias, there should be no significant discontinuities in the distribution at critical values such as $t = 1.96$ (when the p-value would equal 0.05). Looking at Fig. 3, we can infer a discontinuity in the distribution of t-values around these critical points.

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[7]Although they do not provide an explanation, this might be related to the foreign-investment-dependent model prevalent in Eastern Europe. Sub-regions with high share of transnational manufacturing plants might be more vulnerable to import shocks, but overall, still offer better living conditions than other regions.

**Table 3.** The effect of economic insecurity on left-wing and right-wing populism

|                               | No link | Positive | Negative | No of estimates | Share of positives |
|-------------------------------|---------|----------|----------|-----------------|--------------------|
| **Left-wing populism**        |         |          |          |                 |                    |
| Austerity                     | 2       | 1        | 0        | 3               | 33%                |
| Automation                    | 0       | 1        | 0        | 1               | 100%               |
| Bank failure                  | na      | na       | na       | 0               | na                 |
| Foreign currency debt shock   | na      | na       | na       | 0               | na                 |
| Housing demand shock          | na      | na       | na       | 0               | na                 |
| Import exposure               | 7       | 0        | 0        | 7               | 0%                 |
| Labor market uncertainty      | 0       | 3        | 0        | 3               | 100%               |
| Redistribution (reversed)     | 0       | 1        | 0        | 1               | 100%               |
| Subjective economic hardship  | na      | na       | na       | 0               | na                 |
| Total                         | 9       | 6        | 0        | 15              | 40%                |
| **Right-wing populism**       |         |          |          |                 |                    |
| Austerity                     | 1       | 14       | 0        | 15              | 93%                |
| Automation                    | 2       | 2        | 0        | 4               | 50%                |
| Bank failure                  | 2       | 3        | 0        | 5               | 60%                |
| Foreign currency debt shock   | 1       | 4        | 0        | 5               | 80%                |
| Housing demand shock          | 0       | 2        | 0        | 2               | 100%               |
| Import exposure               | 8       | 22       | 1        | 31              | 71%                |
| Labor market uncertainty      | 13      | 18       | 5        | 36              | 50%                |
| Redistribution (reversed)     | 2       | 0        | 0        | 2               | 0%                 |
| Subjective economic hardship  | na      | na       | na       | 0               | na                 |
| Total                         | 29      | 65       | 6        | 100             | 65%                |

However, using the non-parametric local polynomial density estimator with robust bias-correction of Cattaneo et al. (2018), we cannot reject the null hypothesis of no discontinuities at a value $t = 1.96$ (p-value of the test = 0.607). Following Filomena and Picchio (2023), we ran a two-sided binomial test for the null hypothesis of equal mass across different windows around the $t = 1.96$ cut-off. In this case, all tests reject the null hypothesis and point toward significant publication bias (see Table S4.1 on p.17 of the Supplement).

Does publication bias invalidate the overall association? The funnel plot test can provide a statistical answer. However, as already noted, the causal effect of economic insecurity on populism is not directly comparable across the models used in each study. Therefore, we calculate the partial correlation coefficient as a standardized 'effect size' to make the estimates comparable. This measure has been used in previous social science meta-analyses (Filomena and Picchio 2023; Stanley and Doucouliagos 2014) and is calculated as follows:

$$p_i = \frac{t_i}{\sqrt{t_i^2 + dk_i}} \tag{1}$$

where $t_i$ is the $t$-statistic from the causal estimate, and $dk_i$ is the degrees of freedom in the model from which the estimate is derived. Since the number of covariates is not reported in most of the models, we substitute $dk_i$ by the number of observations minus one, following Filomena and Picchio (2023). The partial correlation coefficient $p_i$ varies from −1 to 1. A $p_i$ greater (lower) than zero means that populism increases (decreases) because of economic insecurity. $p_i$ measures the strength of the statistical significance of the effect, although it does not have a direct quantitative meaning of the effect size (Filomena and Picchio 2023; Gechert and Heimberger 2022; Stanley and Doucouliagos 2014).[8]

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[8]The partial correlation coefficient is also a measure of the strength and direction correlation between two variables but holds other variables constant. The value of the Pearson Correlation Coefficient (PCC) implies a stronger correlation, but it carries no substantive meaning; i.e., it is not a regression coefficient and does not imply a bigger effect in the sense of a steeper regression slope. Instead, it implies that observations are distributed more or less closely around the regression line representing the 'true'
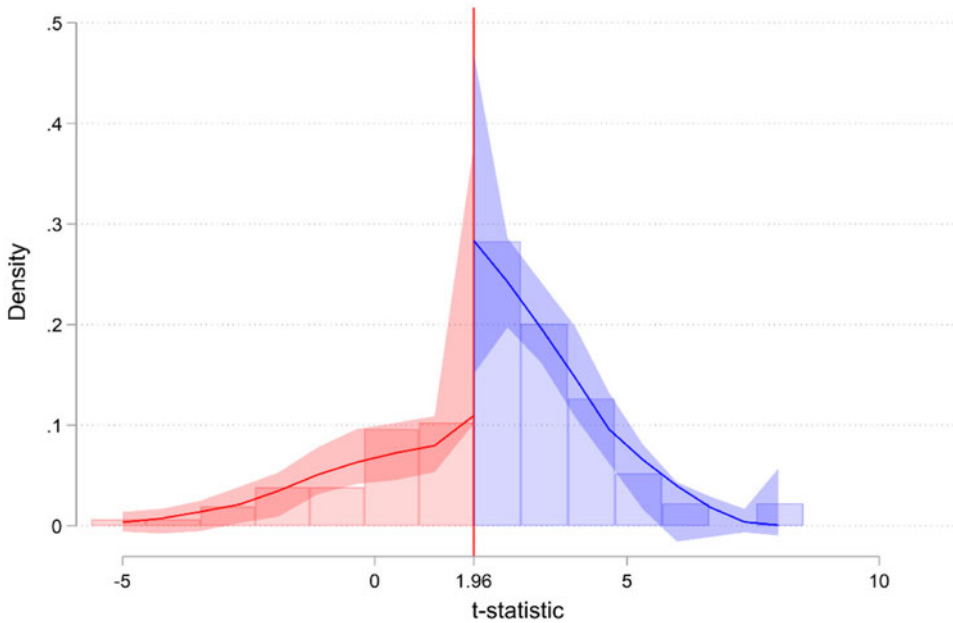
**Figure 4.** Density discontinuity of the *t*-statistics.
*Notes:* The solid lines represent the non-parametric local polynomial density estimator of Cattaneo et al. (2018). The colored areas around are 95 per cent confidence intervals. The null hypothesis of no discontinuity at *t* = 1.96 cannot be rejected at a 5 per cent significance level (p-value = 0.607). The top and bottom 1 per cent of the *t*-statistic distribution were excluded from the graph for illustrating purposes but not from the density estimator and the test.

The inverse funnel plot in Fig. 5 serves as a visual inspection test for publication bias. The funnel plot in Fig. 5 does not seem to be fully symmetric.[9] Whereas more precise estimates (at the top) seem closer to zero, less precise estimates (at the bottom) are concentrated on the right, showing a stronger effect of economic insecurity on populism. This could indicate publication bias.

The Funnel Asymmetry Precision Effect test (FAT-PET) formally tests for a correlation between the partial correlation coefficient and its standard error. It is based on the following regression:

$$p_{i,j} = \propto_0 + \propto_1 \text{SE}(p_{i,j}) + e_{i,j} \qquad (2)$$

where $p_{i,j}$ is the partial correlation coefficient of the estimate I from study *j*, SE ($p_{i,j}$) is the standard error, and $e_{i,j}$ is the error term. If $\propto_1 = 0$, there is no evidence for publication test (Funnel Asymmetry Test, FAT). At the same time, if $\propto_0 \neq 0$, we can conclude that there remains a 'true' effect after accounting for publication bias (Precision-Effect test) (Gechert and Heimberger 2022; Stanley and Doucouliagos 2014). To account for heteroskedasticity, we estimate equation (2) by weighted least squares (WLS) using the inverse of the squared standard errors as weights (Gechert and Heimberger 2022; Stanley and Doucouliagos 2014). We cluster standard errors at the study level j to account for correlation between estimates from the same study.

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underlying association. Because the PCC has the significance of the regression coefficients in the numerator and the sample size in the denominator, it measures the significance of the association correcting for the sample size.

[9]The funnel plot shows the standardized effect size; in our case, the partial correlation coefficient (*x*-axis), against a measure of precision, the inverse of its standard error (*y*-axis). If there is no publication bias, standardized 'effect sizes' (i.e., partial correlation coefficients) should not be correlated with the measure of precision (i.e., the inverse of standard errors). Estimates should vary randomly around the mean, while those that are more precise are closer to the mean; those less precise form a wider range around the mean. In such cases, the funnel plot should be symmetric.
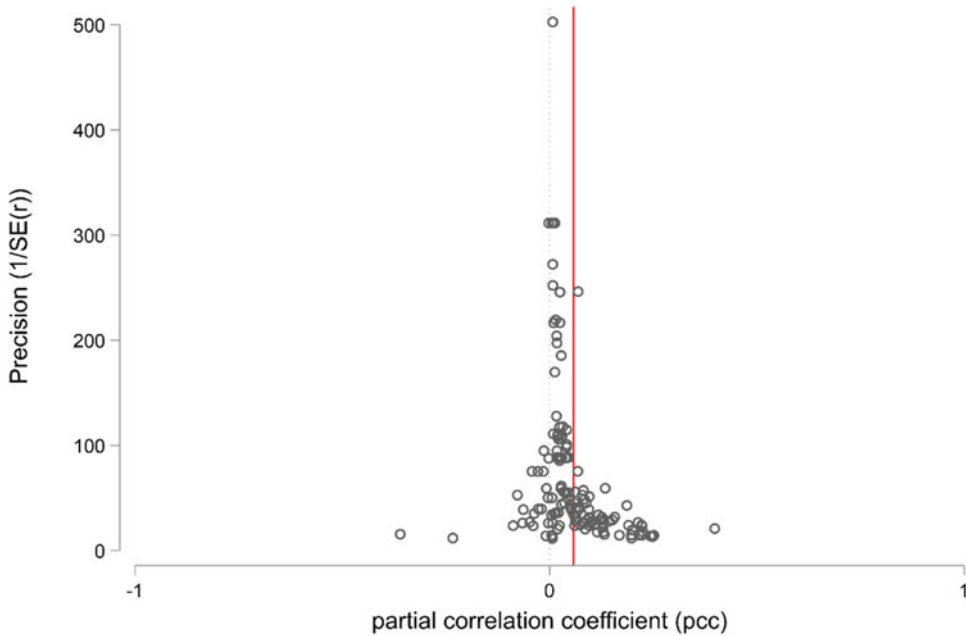
**Figure 5.** Inverse funnel plot.
*Notes*: Each circle represents the different estimates included in the meta-analysis ($n = 144$). The red vertical line represents the mean of the partial correlation coefficient ($p = 0.0579$).

Table 4 reports the results of the regression. Model (1) reports the simple model. Model (2) reports an instrumental variable estimation using the square root of the number of observations as an instrumental variable since SE ($p_{i,j}$) might be endogenous due to measurement error (Filomena and Picchio 2023). In Model (3), we substitute SE ($p_{i,j}$) by its square to account for non-linearities, as this was the preferred model in the presence of publication bias (Stanley and Doucouliagos 2014).

All model results point toward the presence of publication bias; they report a significant association between the partial correlation coefficient and its standard error ($\alpha_1 > 0$). However, crucially, they also show a remaining 'true' effect after accounting for publication bias ($\alpha_0 > 0$). After adjusting for publication bias, the 'true' partial correlation coefficient would be 0.016,[10] compared to the mean of 0.058. While the partial correlation coefficient does not have a quantitative interpretation, these results point to a 'true' positive effect of economic insecurity on populism after accounting for publication bias. However, it is not as strong as the one we would get from a simple average of the standardized 'effect sizes'.

### Heterogeneity Analysis

Finally, following Gechert and Heimberger (2022) and Filomena and Picchio (2023), we test whether different study characteristics influence the likelihood of reporting a significant effect. We do this by extending Models 1 and 3 in Table 4, including the explanatory categorical variables measuring study characteristics that we also used in the vote-counting approach. These are the populism outcome variable, primary treatment variable, region, discipline, and primary experimental design. The omitted baseline categories represent the categories with the largest

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[10]Stanley and Doucouliagos (2014) recommend using the PET from the model including the square of the standard error since the linear model may report biased estimates of the 'true' effect if there is publication bias.

**Table 4.** Funnel-asymmetry precision-effect test results

| Dependent variable | Partial Correlation Coefficient (PCC) | | |
| --- | --- | --- | --- |
| | Model (1) | Model (2) | Model (3) |
| $\propto_1$SE ($p_{i,j}$) | 1.8389*** | 2.1325*** | |
| | (0.2774) | (0.2778) | |
| $\propto_1$SE ($p_{i,j}$)$^2$ | | | 34.3744*** |
| | | | (6.6833) |
| Constant ($\propto_0$) | 0.0072** | 0.0053* | 0.0158*** |
| | (0.0034) | (0.0028) | (0.0031) |
| Observations | 144 | 144 | 144 |
| $R^2$ | 0.2324 | 0.2265 | 0.1707 |

*Notes:* Model (1) reports results from equation (2). Model (2) is a two-stage least square (2SLS) model using the square root of the number of observations as an instrumental variable. The *F*-test of the 1st stage is equal to 24. Model (3) substitutes SE ($p_{i,j}$) from equation (2) by its square. All models are estimated through Weighted Least Squares (WLS) using as weights the inverse of the squared SE ($p_{i,j}$). Standard errors are clustered at the study level in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

sample size in each case. Suppose a study characteristic significantly correlates with the Partial Correlation Coefficient (PCC). In that case, it can be interpreted as statistical evidence that studies with the characteristic in question are more likely to report a significant effect between economic insecurity and populism compared to the omitted baseline category regardless of the sample size (Stanley and Doucouliagos 2012).

Table 5 presents the results. First, the type of populism outcome seems to make no difference to the likelihood of finding an effect from economic insecurity. In alignment with the results based on the vote-counting approach, we find that studies testing the effect of austerity, bank failure, foreign currency debt shock, housing demand shock, and subjective economic hardship are significantly more likely to report a significant effect on populism than studies testing the effect of import exposure, net of the effect of publication bias. Studies from different regions are similarly likely to report significant effects. Studies published in political science journals are more likely to report a more significant effect than studies published in economics journals. Finally, studies relying on difference-in-differences and survey experiments to identify causality are statistically less likely to report a significant effect of economic insecurity on populism. The constant is significant in each specification, which reflects the presence of a significant 'true' effect of economic insecurity on populism in a 'typical' study,[11] net of publication bias.

## Concluding Discussion

We presented the first systematic review and meta-analysis of the causal evidence of economic insecurity as a catalyst of populism. We conducted a two-pronged systematic search to identify eligible studies, combining a database search with a search for citations of eligible studies and recently published narrative reviews. We included studies that focused on populist political behavior as the outcome of economic insecurity as treatment and followed a causal inference research design. Although we found significant heterogeneity in several dimensions, all thirty-six studies report a significant association between economic insecurity and populism. A recurring effect size is that economic insecurity explains around one-third of recent surges in populism. Tallying the number of successful, positive significant associations across 144 causes tests conducted in the thirty-six studies, we found that the overall share of significant positive associations (successes) was 67 per cent, with each causal treatment exceeding the 50 per cent threshold.

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[11]A study using right-wing populism as the outcome; import exposure as treatment; analyzing Europe, published in an economics journal; and using an instrumental variable for causal identification.

**Table 5.** Heterogeneity test results

| Dependent variable | Partial Correlation Coefficient (PCC) | | | | | |
| --- | --- | --- | --- | --- | --- | --- |
| | 1 | 2 | 3 | 4 | 5 | 6 |
| $\alpha_1 SE\,(p_{i,j})$ | 1.5037*** | 1.5914*** | 1.5728*** | | | |
| | (0.3521) | (0.3810) | (0.3802) | | | |
| $\alpha_1 SE\,(p_{i,j})^2$ | | | | 27.8737*** | 29.4920*** | 30.6112*** |
| | | | | (7.4627) | (8.1156) | (7.7099) |
| Estimate-level outcome | | | | | | |
| Left-wing populism | −0.0075 | −0.0078 | −0.0074 | −0.0049 | −0.0051 | −0.0056 |
| | (0.0075) | (0.0077) | (0.0080) | (0.0065) | (0.0066) | (0.0069) |
| Populism | −0.0057 | −0.0058 | −0.0049 | −0.0056 | −0.0057 | −0.0048 |
| | (0.0062) | (0.0064) | (0.0053) | (0.0070) | (0.0072) | (0.0053) |
| Primary treatment | | | | | | |
| Austerity | −0.0066 | −0.0065 | 0.0878*** | −0.0088 | −0.0088 | 0.0981*** |
| | (0.0074) | (0.0078) | (0.0156) | (0.0080) | (0.0082) | (0.0180) |
| Automation | −0.0136 | −0.0133 | −0.0031 | −0.0176** | −0.0176** | −0.0068 |
| | (0.0089) | (0.0095) | (0.0082) | (0.0080) | (0.0085) | (0.0076) |
| Bank failure | 0.0212 | 0.0172 | 0.1180*** | 0.0118 | 0.0073 | 0.1182*** |
| | (0.0296) | (0.0299) | (0.0383) | (0.0318) | (0.0328) | (0.0408) |
| Foreign currency debt shock | 0.0000 | −0.0004 | 0.1000*** | 0.0040 | 0.0038 | 0.1173*** |
| | (0.0079) | (0.0083) | (0.0170) | (0.0080) | (0.0085) | (0.0188) |
| Housing demand shock | 0.0784** | 0.0770** | 0.1655*** | 0.0898** | 0.0890** | 0.1891*** |
| | (0.0363) | (0.0365) | (0.0415) | (0.0366) | (0.0369) | (0.0423) |
| Labor market uncertainty | −0.0138 | −0.0123 | −0.0195 | −0.0123 | −0.0104 | −0.0213 |
| | (0.0087) | (0.0098) | (0.0138) | (0.0088) | (0.0097) | (0.0135) |
| Redistribution | 0.0104 | 0.0095 | 0.0152 | 0.0136 | 0.0128 | 0.0196 |
| | (0.0183) | (0.0180) | (0.0185) | (0.0185) | (0.0182) | (0.0180) |
| Subjective econ. hardship | 0.0474*** | 0.0464*** | 0.1471*** | 0.0589*** | 0.0592*** | 0.1617*** |
| | (0.0135) | (0.0176) | (0.0261) | (0.0132) | (0.0167) | (0.0269) |
| Region | | | | | | |
| US | | −0.0012 | 0.0015 | | −0.0019 | 0.0034 |
| | | (0.0095) | (0.0110) | | (0.0091) | (0.0106) |
| Brazil | | −0.0615* | −0.0482 | | −0.0574* | −0.0418 |
| | | (0.0346) | (0.0383) | | (0.0346) | (0.0369) |
| Discipline | | | | | | |
| Interdisciplinary and sociology | | | −0.0049 | | | −0.0040 |
| | | | (0.0063) | | | (0.0063) |
| Political science | | | 0.0117* | | | 0.0134** |
| | | | (0.0059) | | | (0.0059) |
| Primary experimental design | | | | | | |
| Diff-in-Diff | | | −0.0944*** | | | −0.1073*** |
| | | | (0.0156) | | | (0.0175) |
| Regression discontinuity | | | −0.0873*** | | | −0.0925*** |
| | | | (0.0175) | | | (0.0198) |
| Sibling difference. | | | 0.0124 | | | 0.0248 |
| | | | (0.0204) | | | (0.0184) |
| Survey experiment | | | −0.1101*** | | | −0.1165*** |
| | | | (0.0182) | | | (0.0205) |
| Constant | 0.0175* | 0.0170* | 0.0117* | 0.0256*** | 0.0256*** | 0.0188*** |
| | (0.0089) | (0.0097) | (0.0065) | (0.0075) | (0.0080) | (0.0055) |
| Observations | 144 | 144 | 144 | 144 | 144 | 144 |
| $R^2$ | 0.3255 | 0.3341 | 0.4053 | 0.3036 | 0.3111 | 0.4014 |

*Notes:* References categories: Estimate-level outcome = Populism, Primary treatment = Import exposure, Region = Europe, Discipline = Economics, Primary experimental design = Instrumental variable. All models are estimated through Weighted Least Squares (WLS) using as weights the inverse of the squared SE ($p_{i,j}$). Standard errors are clustered at the study level in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Economic insecurity seems particularly relevant for explaining right-wing populism, while the results for left-wing populism appear more ambiguous. Researchers have frequently analyzed import shocks, with robust causal evidence across thirteen studies. However, judging by the robustness of individual tests extracted from the studies, housing demand shocks, subjective economic hardship, austerity, and foreign currency debt shocks appear to be the most robust causes of populism.

Altogether, the existing scholarship presents enough evidence to conclusively disperse any doubts about the causal role of economic insecurity in the populist backlash against globalization. We explored whether the association reflects the underlying association or is contaminated by publication bias. We estimated a partial correlation coefficient to standardize the results, conducted a funnel-plot asymmetry test, and tested the density discontinuity of the distribution of t-statistics. These tests showed considerable evidence for publication bias. The underlying effect is likely smaller than the overall effect from the thirty-six studies. However, the association between economic insecurity and populism remains significant after controlling for publication bias.

The robustness of this causal association contrasts with the image emerging from some recent narrative reviews that raise doubts about the relationship between economic insecurity and populism. We can identify three factors to explain this difference. First, we defined economic security more broadly than political scientists using correlational designs with individual-level attitudinal data often do. We included measures of economic hardship, such as unemployment, and contextual-level shocks and forms of economic insecurity that did not lead to absolute material deprivation. At the individual level, political-cultural attitudes might cancel out the effect of economic hardship. By contrast, the effect of contextual-level shocks and economic insecurity on populism might be stronger.

Second, most casual designs require measuring temporal change, whereas correlational studies are often cross-sectional. Cross-sectional designs foreground the effects of slow-changing or stable attributes such as political culture. Right-wing populist attitudes are powerful predictors of right-wing populist voting. However, dynamic designs foreground change. These marginal effects might highlight the centrality of specific populations with specific experiences that change their voting behavior between elections, even if they remain a minority in the party's overall electoral base. In short, dynamic designs better capture the causes of the rise/change in support for populism. By contrast, cross-sectional designs can better capture the long-standing characteristics of populist supporters.

Third, systematic reviews and meta-analyses systematically map the available evidence. By contrast, narrative reviews are not systematic, leaving more room for the researchers' assumptions, pre-analytical visions, methodological preferences, and discipline-specific knowledge to influence the studies analyzed – as noted above. Different disciplines might also look at the causes of populism from different angles. Economists might devote more energy to analyzing economic factors, while political scientists might devote more energy to political supply or political culture. We controlled for several of these study characteristics, which did not influence the main association between economic insecurity and populism. In addition to methodological and disciplinary effects, the effect of prevalent views in scholarly publications and the public media could also influence non-systematic, narrative reviews. While our study establishes robust causal evidence between economic insecurity and populism, much remains to be done to systematize what we have learned so far. Our study is just a first step.

By focusing on causal designs, we bracket a significant body of literature that follows correlational designs. We do not think that causal designs are inherently superior to correlational designs. Correlational designs have several important advantages over causal designs. They are more versatile, their external validity is usually stronger, and they can be applied to analyze a wide range of phenomena for which it would be impossible to construct a credible causal design. However, our primary aim is to address the widespread perception in the literature that the association between economic insecurity and populism is weak. The strongest answer to this question emerges from quasi-experimental studies that establish a causal association and prioritize internal validity. We designed our search criteria with this primary aim in mind.

Despite this narrower focus on causal inference, the results of our systematic review fit well with the broader correlational literature. First, a central debate in political science has revolved around the changing political cleavage structures of advanced Western democracies. New

populist right parties gained increasing momentum by combining their traditional middle-class base with working-class voters; that is, the 'losers of modernization' (Betz 1993; Swank and Betz 2003) or the 'losers of globalization' (Hooghe and Marks 2018; Kriesi et al. 2006). In the age of organized Fordist welfare capitalism, working-class members had a chance to enter the middle class. This trend has stopped or even been reversed in advanced democracies.

Second, a more recent stream of studies, partly from the cleavage literature, has examined the effects of new labour market risks and economic insecurities, arguing that juxtaposing losers vs. winners might be misleading. Support for right-wing populism is linked to the growing sense of insecurity among formerly protected 'labor market insiders' who have a degree of material security to lose but are not among the biggest victims (Häusermann 2020; Kurer 2020). Researchers have highlighted several related channels, such as social decline (Engler and Weisstanner 2021), the geographical concentration of economic discontent (Rodríguez-Pose 2018), income inequality (Han 2016), housing prices (Adler and Ansell 2020), and relative group deprivation (Burgoon et al. 2018; Rico and Anduiza 2019).

Third, related to the notion of insecurity, status loss[12] (Gidron and Hall 2017), and status discordance[13] (Kurer and Van Staalduinen 2022) appear to be a stronger predictor of the populist vote than more direct measures of economic deprivation, such as unemployment. This underpins the importance of utilizing fine-grained measures of economic insecurity, as emphasized in our study. The notion of status loss or discordance also allows researchers to grasp the mutually reinforcing – economic and cultural – double devaluations of working-class skills, values, and lifestyles that are increasingly at odds with the demands of the globalized post-industrial economy (Friedman 2003; Kalb 2023). Recognizing the interrelations between cultural and economic devaluations allows researchers to identify how broad populist social coalitions emerge (Halikiopoulou and Vlandas 2020). These correlational studies shed light on crucial aspects of the economic insecurity-populism mechanism, in line with the conclusions arising from the present analysis.

Our systematic review has several policy implications. The literature shows that there is not one type of populist voter but several types, which means there might be several relevant policy channels to address the rise of populism. Classic welfare policies, such as a better social safety net and more redistribution, are crucial to protect the victims of globalization. New policies that protect against emerging insecurities and risks might reduce the appetite for populism among the downwardly mobile working-middle class. We do not see these policy choices as alternatives but as complementary solutions. These insights resonate with the policy conclusions emerging from the extant correlational literature. Swank and Betz (2003) have already reported that globalization is more robustly associated with support for the populist right in countries with lower levels of social protection. This association was later confirmed by Foster and Frieden (2019). In a related study, Vlandas and Halikiopoulou (2019) showed that unemployment benefits and employment protection limited the impact of unemployment on far-right party support.

Regarding newly emerging risks, such as automation, Gingrich (2019) found that while compensation for automation may help left-wing parties retain vote share among the exposed, it does not seem to forestall the rise of the populist right. By contrast, Vlandas and Halikiopoulou (2022) found that welfare state policies reduce the likelihood of supporting the far right among individuals exposed to high risks, including the unemployed, pensioners, low-income workers, employees on temporary contracts, individuals in large families, and individuals who are disabled/permanently sick. Our review confirms the relevance of multiple redistributive and protective welfare policies identified in the literature.

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[12]Stagnating real income or the relative decline of the income of working-class and lower-middle-class groups.

[13]The difference between expectations formed during childhood based on parental status and outcomes realized in adulthood.

Finally, our systematic review and meta-analysis uncovered several blind spots and open questions. First, while the study region does not affect the likelihood of reporting a significant effect, the literature shows significant substantive effect heterogeneity across geographical contexts. For example, some causes relevant in Western Europe might be less relevant in Eastern Europe. Thus, correcting the heavy geographical skew of the literature is essential. Focusing more on the experience of developing and emerging economies could yield new insights into how economic insecurity affects populism. Second, researchers should pay more attention to cross-study comparability. The disciplinary differences in design could strengthen disciplinary silos and lead to divergent results across studies and disciplines, which should be avoided.

Third, the results suggest that a strict juxtaposition of interest-based economic determinants vs. symbolic cultural factors is not fruitful. Focusing on the lived experience of economic insecurity, co-evolution, and the interaction of cultural-symbolic and economic factors allows researchers to bypass this culture/economy dichotomy. Finally, the role of political supply and redistributive policies in mitigating the effect of economic insecurity on populism remains an under-researched area. Politics can reduce economic insecurity, taking the wind from populists' sails. Future research should pay more attention to these questions, which have not received enough attention so far.

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