Automation Risk, Policy Mapping, and Policy Preferences*

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Abstract

Do political preferences change in response to automation risk? Existing observational and experimental studies provide conflicting evidence. This study reconciles these findings by proposing a two-stage framework in which individuals should first perceive automation risk and then map that risk onto concrete policy responses. I test this argument using two pre-registered survey experiments in the United States that vary whether automation risk is presented alone or linked to redistribution and regulatory policies. The results indicate that linking automation risk to these policies generally does not shift support, with the exception of increased support for slowing the adoption of new technologies in the workplace. These findings suggest that preference change depends on whether individuals connect risks to policy options, but that this process is conditional on specific policy domains and sensitive to the broader information environment.

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As a process of creative destruction, technological change improves overall standards of living but also poses risks to jobs that could be replaced by automation technologies, such as machines, computers, and artificial intelligence (Acemoglu and Johnson 2024). Historically, automation technologies like the power loom and threshing machine displaced labor in textiles and agriculture, which in turn provoked political unrest (Caprettini and Voth 2020; Hobsbawm 1952). More recent advances in automation have revived concerns about labor market disruptions and their broader social implications (Acemoglu and Restrepo 2020, 2022; Arntz, Gregory, and Zierahn 2016).

How does the public respond to the risks posed by automation technologies? Will the labor market disruption resulting from these technologies lead to political unrest, as it has in the past? Some observational research suggests that exposure to automation risk increases individuals' support for redistribution policies (Busemeyer et al. 2023; Gallego et al. 2022; Thewissen and Rueda 2019), as well as support for left-wing parties (Borwein et al. 2025; Gingrich 2019). In contrast, an increasing number of studies have found that exposure to automation risk instead drives voters toward populist right-wing parties (Anelli, Colantone, and Stanig 2019; Frey 2020; Im et al. 2019; Kurer 2020; Milner 2021). These mixed findings from observational studies underscore the complexity of automation's political effects, leading researchers to increasingly use survey experiments to identify causal relationships and clarify the underlying mechanisms. Yet, most experimental studies exploring these effects have reported null findings regarding the impact of automation risk on individuals' policy preferences and political behaviors (Borwein et al. 2024; Gallego et al. 2022; Heinrich and Witko 2022; Jeffrey 2021; Menon and Zhang 2025; Mutz 2021; Zhang 2022). The conflicting findings between observational and experimental studies raise a question: are the correlations observed in observational data spurious, or do experimental designs fail to capture the complexity of how automation risk influences political preferences and behaviors?

In this study, I develop a two-stage theoretical framework to explain when exposure to automation risk influences individuals' policy preferences. I argue that translating objective automation risk into policy preferences requires two interconnected stages: information acquisition and policy mapping. The first stage involves individuals perceiving the risks of automation, a process that may be hindered by limited information, the misattribution of economic hardship, or optimism about technological progress. However, awareness of risk alone is insufficient to shift policy preferences. For preferences to change, individuals must connect perceived risk to specific policy options. The second stage, policy mapping, provides this linkage by connecting automation risk to concrete policy responses, enabling individuals to translate general concern into evaluable policy choices. Without this step, perceived risk lacks a clear connection to policy options and is therefore unlikely to shape policy preferences.

To test my argument, I conducted two pre-registered survey experiments in the United States in March and August 2025. In these experiments, respondents were randomly assigned to one of five conditions: a no-information control group; an information-only group that received automation risk information; a policy mapping group that linked automation risk to redistribution and regulation policies; a co-partisan policy mapping group that presented the same linkage with an endorsement from a co-partisan elite; and a counter-partisan policy mapping group that presented it with an endorsement from a counter-partisan elite. This design allows me to isolate the effect of policy mapping beyond risk information alone and to assess how partisan cues condition individuals' willingness to translate perceived automation risk into policy preferences.

My analysis shows that, consistent with previous studies, receiving information about automation risk raises respondents' concern about general job loss. What distinguishes my findings is that policy mapping affects respondents' policy preferences. First, it consistently and significantly increased respondents' support for slowing the adoption of workplace automation across both experiments. Second, while it increased support for redistribution and regulation policies in the first experiment such as providing unemployment benefits and increasing corporate taxes, these effects did not replicate in the second. Lastly, partisan cues did not have an additional impact on the relationship between policy mapping and policy preferences.

This study makes three contributions. First, it develops a theoretical framework to reconcile the divergence between observational and experimental findings on the political effects of automation risk. Existing differences may arise either because observational studies capture spurious correlations or because survey experiments rely on simplified designs that do not capture the full process of preference formation. While some studies address the former concern using stronger identification strategies, such as instrumental variables (Gallego, Kurer, and Schöll 2022), this study focuses on improving experimental design to better reflect the underlying psychosocial mechanism. I propose a two-stage framework in which individuals first perceive automation risk and then connect that risk to relevant policy responses. By incorporating a policy mapping component—often absent in existing experiments—this framework captures the second stage and offers a plausible explanation for their frequent null findings.

Second, this study contributes to the literature on the political consequences of automation risk by identifying a missing link between risk perception and policy preferences. Prior work shows that specific ways of framing automation risk, such as fairness (Jeffrey 2021) or populist rhetoric (Borwein et al. 2024), can shape political attitudes. This study suggests that these frames may work, in part, because they help connect perceived risks to concrete policy responses. By examining redistribution and regulatory policies as policy mapping mechanisms, this study illustrates an additional way in which automation risk can be translated into policy preferences.

Third, this study contributes to the broader international political economy literature on preference formation under economic disruption by examining how preferences form when an issue is weakly politicized. Existing work on trade (Hicks, Milner, and Tingley 2014; Rho and Tomz 2017), immigration (Cavaille and Marshall 2019; Hainmueller and Hiscox 2007), and globalization (Hainmueller and Hiscox 2006) shows that public opinion often diverges from individuals' economic interests (see also, Margalit 2019), in part because individuals lack clear information about the consequences of these risks. In these contexts, however, both the distributional effects of economic shocks and the relevant policy responses are relatively well defined and embedded in political debate. By contrast, automation represents an emerging issue in which these linkages remain less developed. This study shows that, in such low-information environments, preference formation depends on whether individuals can connect perceived risks to concrete policy options, highlighting the role of policy mapping in shaping political responses to new economic challenges.

The remainder of this paper proceeds as follows. First, I elaborate the theoretical framework outlining the conditions under which automation risk shapes policy preferences. Next, I describe the survey experimental design and empirical results. Finally, I conclude with a discussion of the study's implications and suggestions for future research directions.

HOW AUTOMATION RISK SHAPES POLICY PREFERENCES

In this section, I develop a framework to explain the conditions under which automation risk affects individuals' policy preferences. I argue that individuals perceive automation risk and subsequently update their policy preferences through two distinct stages. The first, information acquisition, occurs when individuals receive new information about the risk of job displacement due to automation and subsequently update their perceived risk. The second, policy mapping, refers to the process by which individuals connect their perceived automation risk to relevant policy options. These two sequential and interconnected stages jointly shape policy preferences, particularly given that automation risk has yet to gain significant political prominence (see Figure 1).

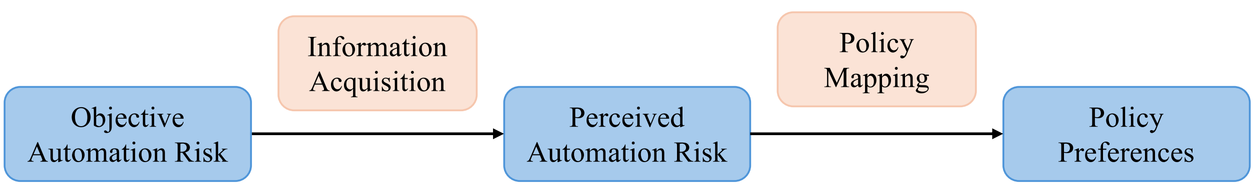

Figure 1: Relationships between Automation Risk and Policy Preferences

Information Acquisition: From Objective Threat to Perceived Risk

The first stage, information acquisition, requires that individuals perceive automation risk before it can influence their policy preferences. A large body of research shows that advances in automation technologies threaten job displacement across a wide range of occupations (e.g., Autor and Dorn 2013). For example, studies estimate that between 10% and 47% of jobs may be susceptible to automation in the coming decades (Frey and Osborne 2017; Nedelkoska and Quintini 2018), a risk further amplified by recent advances in artificial intelligence (Eloundou et al. 2024). However, these objective risks do not necessarily translate into subjective perceptions of automation risk, particularly when individuals lack sufficient information to fully assess their potential impact.

This disconnect between objective risk and subjective perception stems from two main sources. First, individuals often attribute economic hardship to more salient factors, even when automation may be the underlying cause (Gazmararian 2025; Wu 2022). Given the many potential drivers of personal economic outcomes, individuals may lack the information needed to link changes in their financial situation directly to automation. Even when automation is recognized as a contributing factor, uncertainty about its overall impact can remain. As a result, more visible factors are often blamed for economic hardship, with international trade and immigration frequently being used as scapegoats for automation's effects (Sandbu 2022).

Second, public discourse surrounding technological change is often overly optimistic. For example, media coverage of artificial intelligence in the United States has been predominantly positive (Mitts and Raviv 2025). This positive framing shapes how individuals interpret information about automation. Repeated exposure to favorable narratives can reduce their willingness to seek out or accept information about potential economic downsides, leading them to update their beliefs in a systematically optimistic direction. As a result, individuals may develop an optimism bias, whereby they perceive automation as less threatening overall and, in particular, believe their own jobs are less likely to be automated than those of others. For example, a representative survey of U.S. workers finds that 32% of those highly exposed to AI believe it will benefit them more than harm them (Kochhar 2023).

This tendency toward misattribution and optimism prevents individuals from developing a clear subjective perception of automation risk. As a result, additional information is often required to translate an objective automation threat into a perceived one. Existing survey experiments typically provide respondents with information about the probability or proportion of jobs likely to be replaced by automation in order to elicit this perception. Such treatments are generally effective at increasing concern: respondents exposed to this information report greater worry about future job loss (e.g., Gallego et al. 2022; Menon and Zhang 2025; Zhang 2022). However, increased concern does not consistently translate into changes in policy preferences. This suggests that information alone is insufficient for preference formation.

Why does providing individuals with information only about automation risk often fail to change their policy preferences? One explanation is that automation remains a weakly politicized issue. Politicization is defined as "the process of making an issue political, that is, debating it in the public sphere as an issue of public contestation" (De Vries, Hobolt, and Walter 2021, p. 308). When an issue is highly politicized, it tends to be salient to the public, and political parties take clear—often polarized—positions (Van Hootegem 2026). Automation risk does not meet these conditions. It has remained relatively low in public salience compared to other sources of economic disruption. Moreover, political parties and politicians have not consistently articulated clear positions on the issue, at least prior to the recent rise of generative AI since 2022. Although some politicians have attempted to mobilize around automation, these efforts have not been sustained. For example, Andrew Yang's 2020 U.S. presidential campaign prominently framed automation as a major threat to jobs and proposed a universal basic income as a policy response.[1] However, this framing was largely treated as maverick and failed to gain sustained traction within mainstream party competition. More broadly, political elites have either downplayed automation risk or resisted more pessimistic narratives. There has been little consistent effort to link automation risk to concrete policy responses.

In the absence of politicization, both information about automation risk and information about relevant policy responses remain underdeveloped in public discourse. Simply providing individuals with information about the risks of automation is therefore insufficient for individuals to reduce the informational burden required to form policy preferences, especially when the available policy options remain unclear. In the following section, I introduce a second stage of preference formation, policy mapping, to address this gap.

1. https://www.nytimes.com/2019/11/14/opinion/andrew-yang-jobs.html.

Policy Mapping: Translating Perceived Risk into Policy Preference

Since automation risk remains weakly politicized, information about automation as a source of job displacement is often ambiguous, while information about relevant policy responses is even less developed. Even when individuals come to perceive automation as a potential threat, they may still lack a clear understanding of how that threat relates to concrete policy choices. To explain how policy preferences form under such conditions, I introduce a second stage of preference formation: policy mapping. Policy mapping is the process by which individuals connect a perceived problem or risk to specific policy responses. It allows individuals to move from general concern to evaluable policy choices by identifying both the relevant policy domain and the direction of policy intervention. This stage is particularly important for emerging issues, where the link between risk and policy responses is not yet well established.

To situate this policy mapping mechanism, it is useful to consider how individuals form political preferences more generally. Forming opinions on specific issues is cognitively demanding, as it requires individuals to acquire substantial information and translate it into concrete preferences. In practice, however, individuals rarely obtain or process all available political information. Instead, they rely on heuristics, combining a small amount of selected information with simple rules of thumb that map these inputs onto more complex political cognitions (Fortunato and Stevenson 2019). Because most citizens have limited knowledge of policy issues (Delli Carpini and Keeter 1996), these shortcuts help reduce the cognitive burden of preference formation (Lau and Redlawsk 2001; Lupia 1994). Rather than processing the full stream of information, individuals discard much of it and rely on a small set of accessible cues to form their policy preferences.

Within this broader framework, policy mapping can be understood as a specific type of heuristic that helps individuals translate perceived risks into concrete policy choices with minimal cognitive effort. For highly politicized issues, such as climate change (McCright and Dunlap 2011), immigration (Hopkins 2010), international institutions (Rixen and Zangl 2013), and public health (Motta and Stecula 2023), this stage often operates implicitly. In these contexts, information about risks is widely available in public discourse, and political parties have articulated clear and distinct policy positions. Individuals can therefore more easily connect perceived risks to policy responses, often completing the policy mapping stage alongside information acquisition.

However, even in highly politicized issues, policy mapping does not occur automatically. Although elite cues and policy positions are more readily available, individuals may still lack a clear understanding of how policies affect their own interests. In such cases, explicitly linking policies to their consequences remains important for shaping preferences. For example, Rho and Tomz (2017) show that limited knowledge of the economic consequences of protectionism leads individuals' trade preferences to diverge from their material interests. When respondents are provided with information that clarifies the distributional effects of trade policies, the alignment between personal interests and policy preferences becomes stronger. This evidence suggests that even in well-politicized domains, individuals do not necessarily connect perceived economic conditions to relevant policy responses on their own. Preference formation depends on whether the relationship between risks and policy consequences is made explicit.

In the case of automation, such information is far more limited. The link between perceived risk and policy responses is therefore not readily available in the existing information environment. Making policy mapping explicit can help individuals use it as a heuristic to connect perceived risks to relevant policy options and form preferences. The most immediate policy responses to automation risk are redistribution and regulation. The prospect of job displacement primarily threatens individuals' expectations about their future economic security, making redistributive policies a form of insurance against potential losses and regulatory policies a way to slow or manage the pace of technological change. These responses provide clear and intuitive pathways through which individuals can map perceived risks onto concrete policy choices. They are also widely studied in the literature on automation and political preferences (e.g., Busemeyer and Tober 2023; Gallego et al. 2022; Haslberger, Gingrich, and Bhatia 2025; Lauterbach et al. 2023) and are frequently invoked by political elites.[2] An example comes from Senator Charles Schumer's August 1, 2024

2. These two policies are often emphasized by left-leaning politicians, but they are not the only policies through which automation risk can be mapped. Politicians, particularly on the right, may also link automation to cultural or identity-based policies. Such appeals often frame automation

announcement that the Defense Finance and Accounting Services (DFAS) office in Rome would not lay off its 600 employees due to automation. Instead, he proposed new training programs to transition affected workers into alternative roles.[3]

Taken together, these arguments suggest that policy mapping is a necessary step in translating automation risk into policy preferences. Existing experimental studies, however, typically focus on the first stage by providing information about the risk itself, without explicitly linking that risk to policy responses. As a result, such treatments may fail to activate the policy mapping stage, limiting their ability to generate changes in policy preferences.

To test this mechanism, this study employs two survey experiments that vary whether automation risk is presented alone or explicitly linked to policy responses. Based on the two-stage framework, I expect that:

H1: *Support for redistribution and regulation policies will not differ between those who receive only automation risk information and those who receive no information.*

H2a: *Support for redistribution and regulation policies will be higher for those who receive information that maps automation risk to those policies compared to those who receive no information.*

H2b: *Support for redistribution and regulation policies will be higher for those who receive information that maps automation risk compared to those who receive only automation risk information.*

Furthermore, partisanship plays a central role in how individuals process political information. Partisan cues provide an additional layer of guidance that shapes how individuals interpret and apply information. Individuals are more likely to be influenced when information is endorsed by their preferred political party (Hicks, Milner, and Tingley 2014; Slothuus and De Vreese 2010). When exposed to new information, people are motivated not only to form opinions but also to defend

in populist terms, contrasting "big companies" with "ordinary people" or attributing job loss to foreign countries (e.g., Borwein et al. 2024; Chaudoin and Mangini 2025; Menon and Osgood 2024). For example, Donald Trump publicly supported striking dockworkers in October 2024, framing automation as an imposition by foreign-owned shipping firms. See https://www.nytimes.com/2024/12/12/us/politics/trump-ila-dockworkers-automation.html.

3. https://www.schumer.senate.gov/newsroom/press-releases.

and reinforce their existing values, identities, and attitudes (Bolsen, Druckman, and Cook 2014). In the context of policy mapping, partisan labels can shape whether and how individuals connect perceived risks to policy responses. An explicit party endorsement signals how a given policy relates to one's political identity, thereby affecting the credibility and relevance of the mapping between risk and policy. When a policy mapping frame is endorsed by a co-partisan elite, individuals are more likely to accept the linkage between automation risk and the proposed policy response. By contrast, when the same frame is associated with an opposing party, individuals may discount or resist this connection, even if the informational content is identical (Druckman, Peterson, and Slothuus 2013).

This implies that the effectiveness of policy mapping depends not only on making the risk–policy connection explicit but also on the partisan source of that information. Accordingly, I expect policy mapping effects to be amplified when endorsed by a co-partisan elite and attenuated when endorsed by a counter-partisan elite:

H3a: *Support for redistribution and regulation policies will be higher for those who receive information that maps automation risk from co-partisan political elite compared to those who receive the same information without an elite cue.*

H3b: *Support for redistribution and regulation policies will be lower for those who receive information that maps automation risk from counter-partisan political elite compared to those who receive the same information without an elite cue.*

RESEARCH DESIGN

To test these hypotheses, I fielded two pre-registered survey experiments with targeted sample sizes of 1,500 and 2,000 U.S. adults. The surveys were administered in March and August 2025, respectively, via the online platform Prolific.[4] The sample was restricted to working-age adults in the U.S. who identified as either Democrats or Republicans. Respondents who identified as Independents, "None," or "Other" were excluded from the study.[5] Although Prolific is a non-

4. Participants from the first survey were automatically screened out of the second via Prolific's pre-screen option.

5. The exclusion of these respondents is a common practice in studies investigating the effects of partisan cues (e.g., Boudreau and MacKenzie 2018; Tappin, Berinsky, and Rand 2023).

probability sample, its data quality is considered high among the online survey panels frequently used by social scientists (Douglas, Ewell, and Brauer 2023; Peer et al. 2017; Peer et al. 2022).

To ensure comparability, both survey experiments were designed to be highly similar and follow the same procedures, with only minor adjustments to the treatment groups. In both experiments, respondents first provided informed consent and then answered two attention check questions. The initial section of the survey collected basic demographic information, including party affiliation, partisan strength, gender, age, race, education, income, employment status, and occupation.

Respondents were then randomly assigned with equal probability to one of the experimental conditions, with randomization blocked by partisanship. The first survey experiment included three conditions: a pure control group that received no information; a policy mapping treatment group that received an excerpt presenting automation risk alongside proposed redistribution and regulation policies; and a co-partisan policy mapping treatment group that received the same excerpt but with an endorsement from a co-partisan elite, based on respondents' self-reported partisanship. For instance, a Democrat in the co-partisan policy mapping group read a vignette about general automation risk in the U.S. that proposed redistribution and regulation policies, endorsed by a Democrats elite. A similar design was applied to Republicans.[6]

The second survey experiment included four conditions. In addition to the control and policy mapping groups, it introduced an automation-risk-only condition to isolate the effect of risk information from policy mapping, as well as a counter-partisan policy mapping condition.[7] Taken together, the two experiments allow me to assess the independent and combined effects of automation risk, policy mapping, and partisan cues on policy preferences (see Table 1 for an overview of

6. While Democratic elites like Senator Charles Schumer have linked automation risk to redistribution and regulation policies, to my knowledge, no Republican elite has done so as of the time of survey fielding. Hence, participants in the partisan group who received a Republican elite endorsement were given a disclosure form at the end of the survey to clarify the hypothetical nature of the content and avoid misleading them.

7. Both experiments do not include a condition that isolates policy mapping alone. This is intentional, as policy mapping is designed to connect automation risk to policy responses. Without the context of automation risk, policy mapping cannot operate as intended. The strategy used to identify the effect of policy mapping is detailed in the Discussion section.

all treatment conditions).

Table 1: Summary of Experimental Design

Treatment Conditions	First Experiment	Second Experiment
Control Group	✓	✓
Automation Risk Information		✓
Automation Risk Information + Policy Mapping	✓	✓
Automation Risk Information + Co-partisan Policy Mapping	✓	
Automation Risk Information + Counter-partisan Policy Mapping		✓

In both experiments, all treatment groups received the same instructions and information on automation risk.[8] Below is the vignette from the second survey experiment:

> "Please read the following excerpt carefully. You will be asked questions about it afterward.
>
> **Is Automation a Threat to American Jobs? A [News Report/Democratic Legislator/Republican Legislator] Warns of Economic Disruptions Amid Rapid Advances in Automation Technologies**
>
> In recent years, automation and artificial intelligence (AI) have enabled companies to replace human labor with machines and algorithms at an unprecedented pace. While these technologies promise greater efficiency and economic growth, a [news report/Democratic legislator/Republican legislator] has raised concerns about their impacts on American workers. According to recent research, nearly 46% of jobs could be at risk of automation within the next decade. This shift could lead to widespread job losses, downward pressure on wages, and heightened economic uncertainty for millions of workers and their families."[9]

The policy mapping groups received an extra paragraph that links the automation risk with redistribution and regulation policies:

> "The [news report/Democratic legislator/Republican legislator] warns that failing to address these risks could deepen economic inequality and undermine job security. To protect workers, it highlights proposals in the upcoming budget bill that would require companies to uphold fair employment practices and invest in worker support programs. These proposals include funding for job retraining, wage protections, and transitional assistance to help workers adapt to changes in the labor market."

8. The wording of the treatment vignettes was slightly changed for the second survey experiment. See the pre-analysis plan for details.

9. To ensure the comparability of automation risk information with previous studies, the excerpt was designed with reference to Jeffrey (2021), Mitts and Raviv (2025), and Zhang (2022).

After exposure to the treatment vignettes, all respondents answered a series of outcome questions.[10] I began by measuring their worry about job loss due to automation technologies on a 5-point scale. Specifically, respondents were asked: "How likely, or unlikely, do you think it is that all jobs in the U.S. will be mostly done by robots or computers within the next 10 years?" (1 = Very low likelihood (0-20% chance) and 5 = Very high likelihood (80-100% chance)).

Next, respondents provided their attitudes toward redistribution policies on a 7-point scale. They were asked whether they would agree or disagree with the federal government doing the following: a) Provide all Americans with a guaranteed income (UBI) that would allow them to meet their basic needs; b) Expand social spending to support laid-off workers and workers in similar positions; c) Fund retraining programs for those who lose their jobs because of new technology (1 = Strongly disagree and 7 = Strongly agree).

Finally, respondents were asked to express their support or opposition on a 7-point scale for two regulatory policies addressing automation's impact on the labor force: a) technology adoption, specifically whether the U.S. government should take measures to speed up or slow down the use of new technologies in the workplace (1 = Speed up a lot and 7 = Slow down a lot); and b) corporate taxation, regarding whether to support or oppose a special corporate tax aimed at discouraging U.S. companies from introducing new technology (1 = Strongly oppose and 7 = Strongly support).

To test the hypotheses, I conduct a series of pre-registered analyses. I first estimate unadjusted OLS regressions with robust standard errors for each outcome variable, using the control group as the reference category. I then conduct pairwise comparisons across treatment groups based on these models, using the `emmeans` package in `R`, with p-values adjusted for multiple comparisons using Tukey's method (see Tables A5–A7). Finally, I estimate OLS models that include a standard set of covariates: gender, age, race, education, income, and party affiliation. Table 2 summarizes how the experimental design maps onto each hypothesis and its expected effect.

10. The wording of the outcome variables either matches that of previous studies or includes a slight adjustment. This helps to ensure comparability across studies.

Table 2: Summary of Hypotheses

Hypothesis	Comparison	Expected Effect	Tested in Expt.
H1	Risk Information vs. Control	No difference in support	Expt. 2
H2a	Policy Mapping vs. Control	Higher support under Policy Mapping	Expt. 1, 2
H2b	Policy Mapping vs. Risk Information	Higher support under Policy Mapping	Expt. 2
H3a	Co-partisan Policy Mapping vs. Policy Mapping	Higher support under Co-partisan Mapping	Expt. 1
H3b	Counter-partisan Policy Mapping vs. Policy Mapping	Lower support under Counter-partisan Mapping	Expt. 2

RESULTS

Before describing the results of my survey experiments, I first examine baseline job loss concerns, policy preferences toward redistribution and regulation, and demographic characteristics. Table 3 presents descriptive statistics for respondents in the control group.[11]

Table 3: Descriptive Statistics for the Baseline Groups

	Experiment (1) (N = 496)		Experiment (2) (N = 483)		Difference in Mean
	Mean	SD	Mean	SD	p-value
Panel A: Demographic variables					
Male (1 = Male)	0.52	0.50	0.51	0.50	0.88
Age	40.43	11.70	41.80	11.85	0.07
White (1 = White)	0.73	0.44	0.71	0.45	0.54
Education Level (1–5)	3.66	0.92	3.71	0.94	0.34
Income Decile (1–12)	7.39	3.34	7.80	3.41	0.05
Employment (1 = Employed)	0.79	0.41	0.83	0.38	0.15
Republican (1 = Rep.)	0.50	0.50	0.50	0.50	0.97
Panel B: Outcome variables					
General Job Loss Concern	2.72	1.13	2.78	1.07	0.40
Universal Basic Income	4.85	2.09	5.01	2.04	0.25
Unemployment Benefits	5.02	1.84	5.28	1.69	0.02
Retraining Opportunities	5.37	1.74	5.71	1.43	$< .001$
Slow Down Technology Adoption	3.74	1.36	4.11	1.32	$< .001$
Increase Corporate Taxation	3.34	1.72	3.45	1.65	0.30

Notes: This table shows mean and standard deviation (SD) for the baseline groups in both experiments, including the p-value for the difference in means between two groups. A t-test was used to calculate p-values for continuous variables, and a chi-square test was used for binary variables.

11. The balance test for pre-treatment variables across conditions is provided in Table A1 and A2. The results indicate no systematic differences across conditions.

The baseline attitudes from the first experiment show a moderate level of average concern about job loss in the U.S. over the next decade (Mean = 2.72, SD = 1.13). A score of 3 on the scale indicates a “moderate likelihood (40-60% chance)” of widespread job replacement. The average support for redistribution policies hovers around the “Somewhat agree” option, with retraining opportunities receiving the highest support (Mean = 5.37, SD = 1.74). This is followed by unemployment benefits (Mean = 5.02, SD = 1.84) and universal basic income (Mean = 4.85, SD = 2.09). Support is lower for regulation policies, with the speed of technology adoption receiving a mean score of 3.74 (SD = 1.36), where 3 represents “Slightly speed up,” and corporate taxation to discourage new technologies scoring 3.34 (SD = 1.72), where 3 indicates “Somewhat oppose.”

Notably, the baseline attitudes in the second experiment for unemployment benefits, retraining opportunities, and the speed of technology adoption are statistically significantly higher than those in the first experiment, even though there were no systematic differences in demographic features.

Effects on General Job Loss Concern

For automation risk to translate into policy preferences, I argue that individuals need to undergo two stages: information acquisition and policy mapping. The first stage requires individuals to be aware of the risks of automation, transforming an objective threat into a subjective perception. Building on the forecasts by Frey and Osborne (2017), I provided all treatment groups with information about 47% U.S. jobs at risk of automation in the next 10 years, expecting this information to increase respondents’ concern about general job loss.

The average treatment effects on general job loss concern across both experiments are shown in Figure 2. The results consistently show that groups who received vignettes including general automation risk expressed greater concern about job loss compared to the control group. For instance, in the second survey experiment, respondents in the automation risk information group reported a 0.26 (SE = 0.06) increase in job loss concern relative to the control group. This effect is statistically significant ($p < .001$). Similarly, respondents assigned to the policy mapping groups, regardless of partisan cue, also showed a statistically significant increase in concern ($p < .001$).

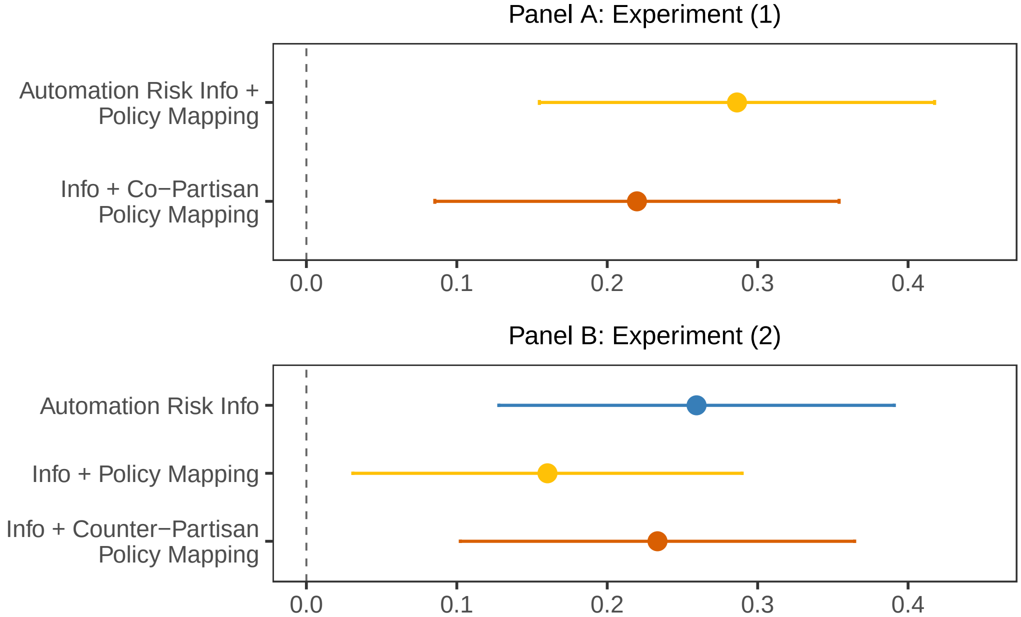

Figure 2: Estimated Effects of Treatments on General Job Loss Concern

Notes: Panel A shows the average treatment effects of the experimental conditions on general job loss concern for the first experiment (N = 1,490). Panel B shows the results for the second experiment (N = 1,996). The control group serves as the baseline for all comparisons. Results are based on unadjusted OLS models with robust standard errors (SE). Points represent coefficients, and error bars indicate 95% confidence intervals. Full results are reported in Table A3 and A4.

The results replicate previous survey experiments, providing evidence that informing individuals about automation risk is important for them to perceive the threat. This, in turn, demonstrates that the treatment activates the first stage of the two-stage framework. The next section will explore the second stage, a component not examined in past works, and report on the effect of policy mapping on individuals' policy preferences.

Effects on Policy Preferences

To test the effect of policy mapping on policy preferences, both experiments included a condition where respondents, after receiving information on automation risk, were shown policy mapping content that explicitly proposed redistribution and regulation policies. To further examine the role of partisan cues, an additional condition framed this policy mapping content as originating from a co-partisan or counter-partisan legislator. By activating this second stage of policy mapping, the experimental design allows individuals to connect their perceived automation risk to specific policy options. I therefore expect that respondents exposed to this policy mapping content will express greater support for redistribution and regulation policies.

Redistribution policies. Panel A in Figure 3 displays mixed results for the average effects of treatments on three redistribution policies for the first experiment. First, regarding preferences for UBI, policy mapping, whether endorsed by a co-partisan legislator or not, increased support compared to the control group, but effects are not statistically significant. Second, turning to the policy on unemployment benefits, which proposes expanding social spending for laid-off workers, respondents exposed to policy mapping—both from a news report and with a co-partisan elite endorsement—were statistically significantly more likely to support this policy ($p < .05$ for both groups). Substantively, receiving the policy mapping content, even without a co-partisan elite endorsement, increased support for unemployment benefits by 0.25 points (SE = 0.11), which represents a 5% increase compared to the control group. Finally, concerning retraining opportunities, support increased significantly when the policy mapping was presented by a co-partisan legislator ($p < .01$). However, when the same policy mapping was presented from a news report, the coefficient remained positive as expected, but it was not significant ($p = 0.056$).

Panel B of Figure 3 presents the results from the second experiment. For all three redistribution policies, none of the three treatment conditions had a significant effect on respondents' attitudes. Consistent with H1, the non-significant effects for the automation risk information-only group were expected, as the information acquisition stage alone is posited to be unable to shift policy preferences. A key finding is the failure of the second experiment to replicate the statistically significant effect observed in the policy mapping group of the first experiment. The non-significant effect of policy mapping from counter-partisan elites is also not surprising. The observed effects in the second experiment are also lower than those in the first. Taken together, the results from the second experiment indicate a lack of robustness in the findings of the first.

Regulation policies. Panel A of Figure 4 shows the results from the first experiment for regulation policies. Notably, respondents assigned to either of the policy mapping treatment groups were

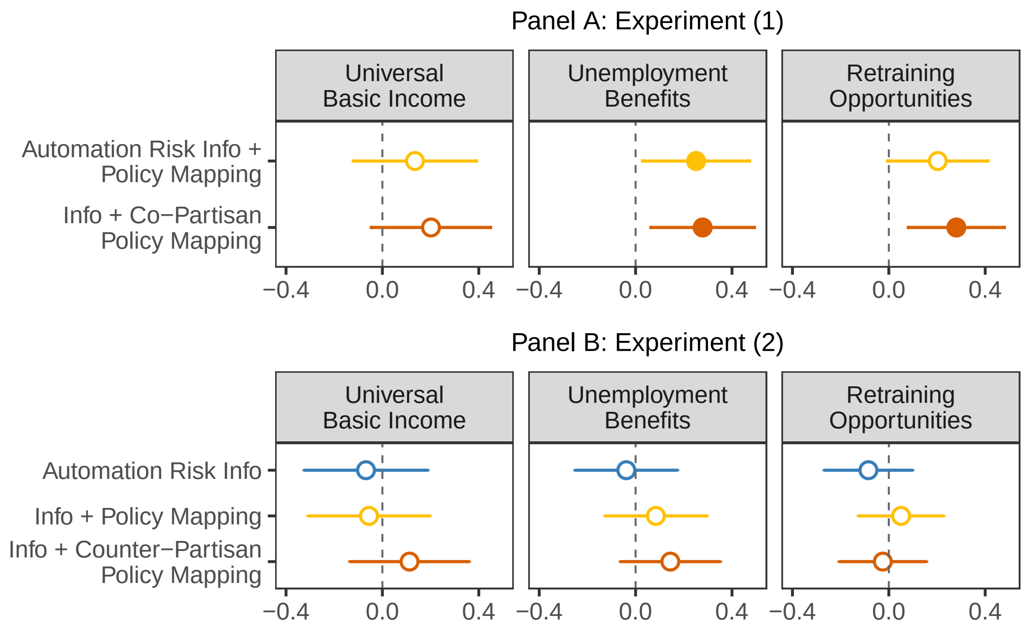

Figure 3: Estimated Effects of Treatments on Redistribution Policies

Notes: Panel A shows the average treatment effects of the experimental conditions on three redistribution policies for the first experiment(N = 1,490). Panel B shows the results for the second experiment (N = 1,996). The control group serves as the baseline for all comparisons. Results are based on unadjusted OLS models with robust standard errors. Points represent coefficients, and error bars indicate 95% confidence intervals. Full results are reported in Table A3 and A4.

more likely to support regulating automation technologies. For technology adoption, support for policies to slow down the use of new technology in the workplace significantly increased ($p < .001$ for both frames). A substantive interpretation of the results is that reading a vignette about automation risk along with policy responses increased respondents' support for slowing down the pace of technological change in the workplace by 0.36 points (SE = 0.09). The effect size is non-trivial, representing a 9.7% increase compared to the control group. Similarly, for corporate taxation, support for a special tax aimed at discouraging the introduction of new technology also significantly increased ($p < .001$ for both groups).

In contrast, the results from the second experiment in Panel B of Figure 4 are more mixed. While it replicates the findings on the effect of policy mapping on technology adoption, it fails to do so for corporate taxation. Contrary to H1, which hypothesized that the automation risk information-only group would not increase support for regulation policies, both the information-only and policy

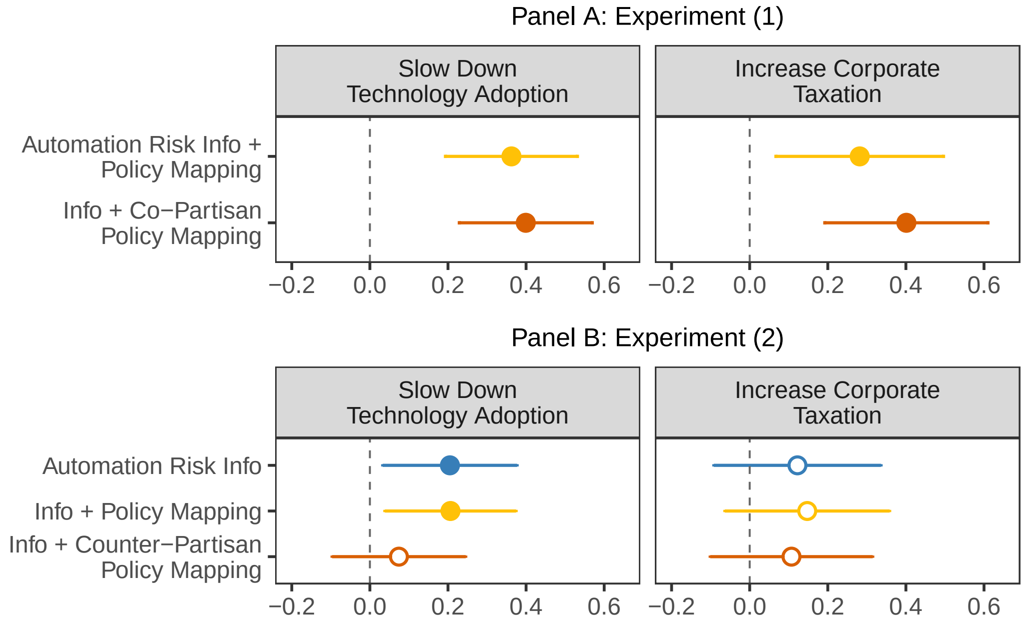

Figure 4: Estimated Effects of Treatments on Regulation Policies

Notes: Panel A shows the average treatment effects of the experimental conditions on two regulation policies for the first experiment (N = 1,490). Panel B shows the results for the second experiment (N = 1,996). The control group serves as the baseline for all comparisons. Results are based on unadjusted OLS models with robust standard errors. Points represent coefficients, and error bars indicate 95% confidence intervals. Full results are reported in Table A3 and A4.

mapping groups showed a statistically significant increase in support for slowing the pace of new technology adoption in the workplace ($p < .05$ for both groups). Consistent with expectations, respondents were less likely to support policies when the policy mapping information came from a counter-partisan elite. Finally, similar to the pattern seen with redistribution policies, the effect sizes for regulation policies are lower in the second experiment than those found in the first experiment.

Heterogeneous Effects of Policy Mapping

The aggregated results may mask heterogeneity across groups. I pre-registered analyses examining whether treatment effects vary by party affiliation, strength of partisanship, and objective occupational vulnerability. To do so, I estimate OLS models that include interactions between treatment indicators and these conditioning variables, with robust standard errors.

Results reported in Appendix A3 provide limited evidence of systematic heterogeneity. Although there are clear partisan differences in baseline support for redistribution policies, Democrats and Republicans generally respond to the treatments in similar directions and with comparable magnitudes. The strength of partisanship does not meaningfully moderate treatment effects, even when partisan cues are present, consistent with the argument that automation remains weakly politicized. Similarly, occupational vulnerability to automation, measured by the Routine Task Intensity (RTI) index, shows little moderating role. While RTI is associated with baseline preferences for some policies, interaction effects with treatments are small and not statistically significant. Overall, these results suggest that the effects of policy mapping are broadly consistent across groups.

Robustness Check

For additional robustness checks, I restricted the analysis to a specific sample by applying several exclusion criteria and then re-ran the main analysis on this subset. First, I removed observations from respondents who failed attention checks, as inattention can lead to answers that do not reflect their true preferences and may cause issues with treatment non-compliance.[12] Second, I removed respondents with outlier survey completion times, as this can be a sign of inattention. Screening out these inattentive respondents helps to reduce noise and improve data quality. Third, I excluded respondents who shared the same IP address to prevent potential treatment spillovers, which might happen if multiple individuals from the same household took the survey or if the same person took the survey multiple times.[13] The results from these checks are reported in Appendix A4 and confirm that the main findings remain robust.

DISCUSSION

Taken together, the results from the two survey experiments provide a nuanced assessment of the effects of policy mapping on individuals' policy preferences regarding automation risk. Overall, the evidence is insufficient to establish a clear policy mapping effect. Identifying such an effect

12. In the first experiment, 72 respondents failed to pass both attention check questions, while in the second experiment, 27 respondents failed to pass both.

13. In the first experiment, 38 respondents had duplicated IPs, while the second had 23.

requires support for both H1 and H2a, that is, a null effect for the "Automation Risk Information" condition alongside a positive effect for the "Automation Risk Information + Policy Mapping" condition, both relative to the control group.

The results support H1 which is automation risk information alone does not change respondents' attitudes toward redistribution and regulation policies, with the exception of preferences regarding the speed of technological adoption. Evidence for H2a, however, is mixed. In the first experiment, the combination of automation risk information and policy mapping increased support for unemployment benefits, retraining programs, and regulatory policies related to technology adoption and corporate taxation. However, this design did not include a standalone automation risk condition, preventing a clean identification of the policy mapping effect. The second experiment addressed this limitation, but its results are largely inconclusive. In most cases, neither treatment differs significantly from the control group. The only exception is support for slowing technological adoption, where both treatments show significant effects. Taken together, these findings do not provide consistent evidence in favor of H2a.

Consistent with this pattern, H2b is not supported, as there is no significant difference between the automation risk information-only group and the policy mapping group. For H3a and H3b, the results suggest directional patterns—greater support under co-partisan endorsements and lower support under counter-partisan endorsements—but these differences are not statistically distinguishable from the baseline policy mapping condition. Finally, there is no evidence of heterogeneous treatment effects across party affiliation, strength of partisanship, or occupational vulnerability. These findings are summarized in Table 4.

Nevertheless, the results from the first experiment are noteworthy, as they diverge from prior studies on automation risk, which typically report null effects on policy preferences. In this section, I evaluate the credibility of these findings by examining three potential concerns: experimenter demand effects, the intensity and tone of the treatment, and whether the policy mapping vignette captures a distinct mechanism relative to alternative explanations. I show that these concerns are unlikely to account for the observed effects. Assuming the results are credible, I then turn to the

Table 4: Summary of Experimental Findings

Hypotheses	Results	Summary of Findings
H1	✓	Automation risk information alone does not change support for redistribution or regulation policies.
H2a	Mixed	In Experiment 1, policy mapping increased support for unemployment benefits, retraining, slower technology adoption, and corporate taxation. In Experiment 2, only support for slowing technology adoption remains significant.
H2b	✗	Policy mapping does not increase support relative to the automation risk information-only condition.
H3a	✗	Co-partisan elite endorsement increases support relative to the control group, but not relative to the policy mapping condition.
H3b	✗	Counter-partisan elite endorsement decreases support relative to the control group, but not relative to the policy mapping condition.

discrepancy between the two experiments and consider how changes in the broader information environment—particularly shifts in baseline public opinion and potential ceiling effects—may explain the differing patterns of findings.

Demand effects. A potential concern regarding the statistically significant findings in the first experiment is the presence of experimenter demand effects. In such instances, respondents, aware they are being experimented on, might guess the study's intention and adjust their responses accordingly. The treatment vignettes mention specific redistribution policies, such as funding for job retraining, and also discuss regulation policies, which in conjunction with treatment language could potentially lead respondents to change their responses. However, there are two reasons that mitigate this concern. First, if demand effects exist, they should be observed in both survey experiments, as there were only minor changes to the vignettes between the two. The second experiment did not find a significant effect for unemployment benefits and retraining opportunities, or for corporate taxation. This failure to replicate the results of the first experiment gives the confidence that the initial findings were not driven by demand effects alone. Second, even if demand effects exist, their impact should be limited. De Quidt, Haushofer, and Roth (2018) and Mummolo and Peterson (2019) directly test for demand effects in survey experiments, finding that they are typically modest or even nonexistent even when respondents were informed about the experimenters' intent. The vignettes in this study, however, did not signal the specific hypotheses being tested, so I expect any such demand effects to be minimal.

Treatment intensity and tone. A second concern is that the treatment vignette in the first experiment may be more negative, overstating the threat of automation and thereby delivering a stronger treatment than those used in prior studies. To assess this possibility, I collected treatment vignettes from previous survey experiments that examine the effects of automation risk on policy preferences. For comparability, I focus only on the portions of these vignettes that describe automation risk. I then conduct a sentiment analysis of these texts. As shown in Figure 5, which reports the share of words associated with anger, fear, and sadness, the vignette used in this study is not more negative than those used in prior work. If anything, it is less emotionally charged. This suggests that the results are unlikely to be driven by an unusually strong or negative treatment.

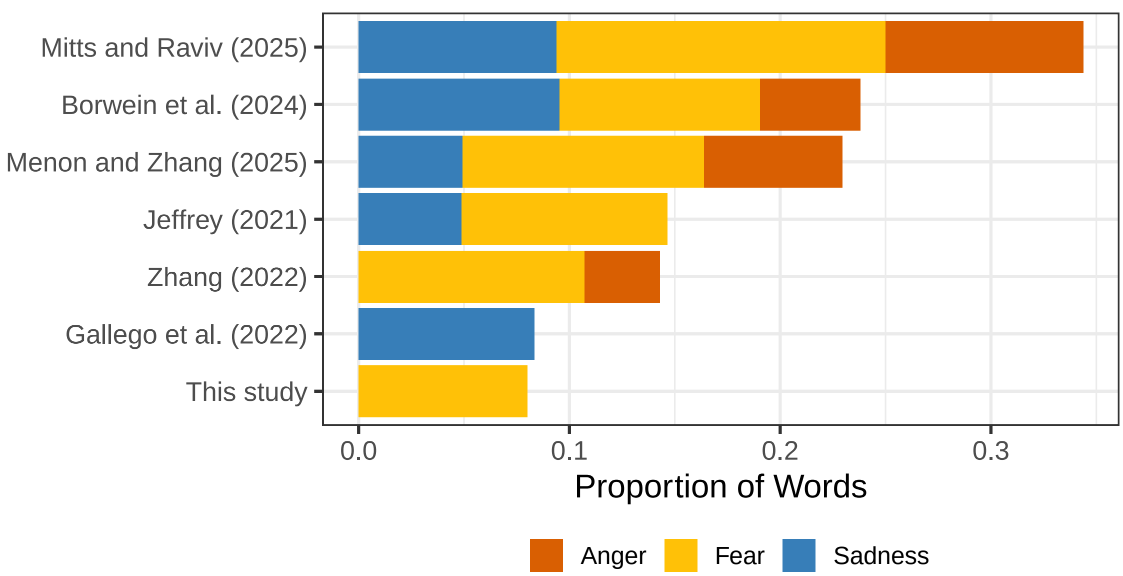

Figure 5: Sentiment Analysis of Treatment Vignettes

Notes: The figure shows the proportion of words categorized as anger, fear, or sadness in the survey experiment's treatment vignettes, based on the NRC Sentiment Lexicon.

Distinct mechanism of the treatment. A related concern is that the policy mapping vignette may simply provide additional exposure to automation risk, rather than introducing a distinct mechanism. There are at least two reasons why this may not be the case. First, the policy mapping vignette does not introduce new information about automation risk or repeat automation-related language; instead, it focuses on policy responses. Second, prior studies use longer (e.g., Zhang 2022) or more emotionally charged automation-risk treatments (see Figure 5) but still find null effects. This pattern suggests that increasing the intensity of risk information alone is unlikely to shift policy preferences.

Another concern is that the policy mapping treatment may operate through a mechanism similar to embedded liberalism, in which compensation through welfare policies reduces opposition among those adversely affected by economic change (Mansfield and Rudra 2021; Ruggie 1982). The vignette in this study does include references to policy proposals by legislators, which could be interpreted as compensatory signals. However, the mechanism tested here is conceptually distinct. Embedded liberalism operates through material compensation, increasing support among those who expect to benefit directly. By contrast, policy mapping links perceived automation risk to policy responses, enabling individuals to translate general concern into policy evaluation without requiring anticipated personal gains. Importantly, this mechanism is not limited to compensatory policies; in principle, policy mapping could also involve non-economic or cultural responses. Moreover, if compensation were driving the results, effects should be concentrated among individuals more exposed to automation risk.[14] I do not find such heterogeneous effects by occupational vulnerability (see Figure A3), suggesting that the results are less consistent with the embedded liberalism mechanism. Nevertheless, future research could more directly distinguish between compensatory and policy mapping mechanisms by varying the content of policy treatments or isolating compensation effects from policy mapping components.

The discussion of demand effects and treatment design helps to mitigate concerns about a "false positive" result in the first experiment. However, if the findings from both experiments are credible, one question remains: what accounts for the discrepancy between them? To investigate this, I first

14. This expectation is based on tests of embedded liberalism in the context of international trade. Ehrlich and Hearn (2014) shows that knowledge of compensation programs increases support for free trade among likely losers of globalization, but not among likely winners.

compared the demographic characteristics and policy preferences of the control groups in both experiments. As shown in Table 3, while there were no systematic demographic differences, the average support for unemployment benefits, retraining opportunities, and the speed of technology adoption was statistically significantly higher in the second experiment. This finding points to a shift in baseline public opinion over time. While formally testing this change in policy preferences is outside the scope of this study, evidence from other surveys indicates that the issue of automation risk is rapidly evolving. For instance, two YouGov surveys from March and June 2025 found a steady increase in the share of Americans who hold a negative view of artificial intelligence's societal effects, rising from 40% to 47% (Ballard 2025b, 2025a). This growing public anxiety could plausibly drive increased support for related regulation and redistribution policies.

This baseline shift may also explain the disappearance of significant results in the second experiment. As baseline support rises, the treatment effect may be subject to a "ceiling effect," making it more challenging to detect a measurable impact. Figure 6 illustrates the outcomes for the three variables where the control groups showed a statistically significant change across experiments. I also plot the results from the comparable policy mapping groups across both experiments. A comparison of the control groups reveals a statistically significant increase in the level of support. In contrast, when comparing these to the policy mapping group, the difference in means is not statistically significant for any variable except support for slowing down technology adoption. This suggests that a ceiling effect may be at play for the treatment vignette: if public support is already high, it is difficult to move preferences further. The results for technology adoption, however, imply that when public support is moderate, a treatment effect may still be detectable.

CONCLUSION

As automation technologies continue to advance, concerns about technological unemployment have made their political implications increasingly salient. This study examines the conditions under which automation risk shapes individuals' policy preferences in advanced democracies, with a focus on how risk perceptions translate into policy attitudes. I argue that this process operates through

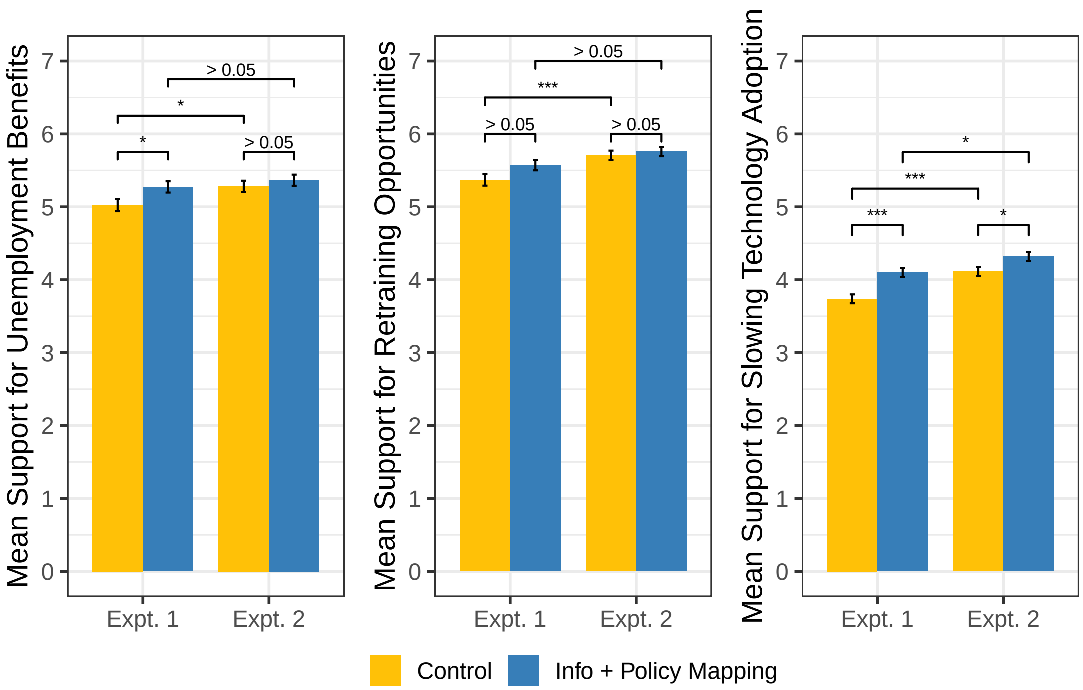

Figure 6: Mean Support for Redistribution and Regulation Policies Across Experiments

Notes: The figure compares mean support for unemployment benefits, retraining opportunities, and slowing technology adoption between the control and policy mapping groups of both experiments. Error bars show the standard error of the mean. Mean differences within and across groups were evaluated using a t-test. $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$.

two stages: individuals must first perceive automation risk and then connect that risk to concrete policy responses. To test this argument, I conducted two pre-registered survey experiments that vary whether respondents receive information about automation risk alone or in combination with policy mapping cues. The results provide mixed but informative evidence. Providing information about automation risk alone does not consistently change policy preferences, though it does increase general concern about job loss. Linking automation risk to specific policy responses yields more consistent effects, particularly in increasing support for slowing the adoption of workplace automation. However, effects on other policies, such as corporate taxation and redistribution, are not consistently replicated across the two experiments.

These findings help reconcile the gap between observational and experimental research on the political consequences of automation. While observational studies often find that automation risk is associated with policy preferences and political behavior, experimental studies typically report null effects. The results here suggest that this discrepancy may stem from differences in design: treatments that provide risk information alone may fail to activate the process through which individuals connect risk to policy. When this linkage is made explicit, individuals are more likely to adjust their preferences. At the same time, the mixed results indicate that this mechanism is conditional and sensitive to the broader information environment. The findings also point to the role of political elites in shaping how automation risk enters political debate. As automation becomes more salient, elites may attempt to connect these risks to policy agendas. The evidence here suggests that such efforts can influence public preferences when they clarify the relationship between risk and policy.

Nevertheless, several limitations of this study should be noted. First, the policy mapping treatment may capture multiple mechanisms simultaneously. In addition to linking automation risk to policy responses, the vignette introduces information about policy proposals, which may also convey compensatory signals rather than isolating the mapping process. Although the analysis provides some evidence against a purely compensatory interpretation, the design does not fully disentangle these mechanisms. Second, the treatment bundles automation risk with specific policy domains, limiting the ability to assess how individuals would respond to alternative mappings or competing policy interpretations. Third, the experiments are conducted in a rapidly evolving information environment, where baseline attitudes toward automation and related policies may shift over time. As suggested by differences across the two waves, such shifts can attenuate treatment effects and complicate causal interpretation. Finally, the focus on the U.S. context and a limited set of policy responses may constrain the generalizability of the findings to other institutional settings or to non-economic framings of automation risk.

Future research can build on these limitations in several ways. It would be valuable to more clearly disentangle the mechanisms underlying policy mapping, for example by separating the effect of linking risks to policy responses from compensatory signals associated with government action. Expanding the range of policy mappings to include cultural or market-oriented responses would help assess how flexible and generalizable policy mapping is in shaping individuals' preferences. Given the evidence of shifting baseline attitudes, longitudinal or repeated designs could assess how changes in the broader information environment condition the effectiveness of policy mapping over time. Finally, extending this framework beyond the U.S. context would improve external validity and allow for testing whether policy mapping operates differently in settings where policy responses to technological change are more developed or more salient in political debate.

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APPENDIX

A1 Balance between Treatment and Control Groups

Table A1 and A2 presents descriptive statistics on key covariate balance.

Table A1: Balance Tests (Experiment 1)

Variable	Overall	Control	Policy Mapping	Co-Partisan Policy Mapping
Male (%)	737 (49%)	257 (52%)	256 (51%)	224 (45%)
Age (Mean, SD)	41 (12)	40 (12)	41 (12)	41 (12)
White (%)	1,087 (73%)	363 (73%)	362 (73%)	362 (73%)
Education Level				
Some high school or less (%)	2 (0.1%)	1 (0.2%)	0 (0%)	1 (0.2%)
High school diploma or GED (%)	188 (13%)	70 (14%)	53 (11%)	65 (13%)
Some college, but no degree (%)	320 (21%)	107 (22%)	106 (21%)	107 (22%)
Bachelor's degree (%)	676 (45%)	238 (48%)	219 (44%)	219 (44%)
Graduate or professional degree (%)	304 (20%)	80 (16%)	120 (24%)	104 (21%)
Income (Mean, SD)	7.6 (3.4)	7.4 (3.3)	7.6 (3.5)	7.7 (3.3)
Republican (%)	751 (50%)	249 (50%)	251 (50%)	251 (51%)
Strong Partisan (%)	859 (58%)	291 (59%)	294 (59%)	274 (55%)
Employed (%)	1,180 (79%)	393 (79%)	399 (80%)	388 (78%)
Sample Size	1,490	496	498	496

Notes: This table reports covariate balance for Experiment 1. For binary variables, counts and percentages are reported. For continuous variables, means and standard deviations are reported.

Table A2: Balance Tests (Experiment 2)

Variable	Overall	Control	Automation Risk Info	Policy Mapping	Counter-Partisan Policy Mapping
Male (%)	995 (50%)	248 (51%)	249 (50%)	235 (46%)	263 (52%)
Age (Mean, SD)	42 (12)	42 (12)	42 (12)	42 (12)	42 (12)
White (%)	1,408 (71%)	345 (71%)	349 (70%)	364 (72%)	350 (69%)
Education Level					
Some high school or less (%)	14 (0.7%)	3 (0.6%)	3 (0.6%)	7 (1.4%)	1 (0.2%)
High school diploma or GED (%)	226 (11%)	52 (11%)	64 (13%)	61 (12%)	49 (9.7%)
Some college, but no degree (%)	512 (26%)	129 (27%)	116 (23%)	141 (28%)	126 (25%)
Bachelor's degree (%)	805 (40%)	195 (40%)	217 (43%)	181 (36%)	212 (42%)
Graduate or professional degree (%)	439 (22%)	104 (22%)	102 (20%)	116 (23%)	117 (23%)
Income (Mean, SD)	7.8 (3.3)	7.8 (3.4)	8.0 (3.3)	8.0 (3.4)	7.6 (3.3)
Republican (%)	992 (50%)	243 (50%)	245 (49%)	256 (51%)	248 (49%)
Strong Partisan (%)	1,063 (53%)	241 (50%)	266 (53%)	272 (54%)	284 (56%)
Employed (%)	1,661 (83%)	400 (83%)	419 (83%)	416 (82%)	426 (84%)
Sample Size	1,996	483	502	506	505

Notes: This table reports covariate balance for Experiment 2. For binary variables, counts and percentages are reported. For continuous variables, means and standard deviations are reported.

A2 Main Experimental Results

Table A3: Effects of Policy Mapping on Job Loss Concern, Redistribution Policies, and Regulation Policies (Experiment 1)

	General Job Loss Concern		Universal Basic Income		Unemployment Benefits		Retraining Opportunities		Technology Adoption		Corporate Taxation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Constant (Baseline)	2.720***	2.552***	4.853***	6.973***	5.022***	6.484***	5.369***	5.861***	3.738***	5.072***	3.343***	5.018***
	(0.051)	(0.164)	(0.094)	(0.273)	(0.083)	(0.230)	(0.078)	(0.229)	(0.061)	(0.200)	(0.077)	(0.257)
Policy Mapping	0.286***	0.274***	0.135	0.163	0.251*	0.277**	0.203†	0.221*	0.362***	0.402***	0.282*	0.300**
	(0.067)	(0.067)	(0.130)	(0.121)	(0.113)	(0.106)	(0.106)	(0.102)	(0.086)	(0.083)	(0.109)	(0.108)
Co-Partisan Policy Mapping	0.220**	0.211**	0.202	0.226†	0.278*	0.297**	0.280**	0.280**	0.399***	0.393***	0.401***	0.402***
	(0.069)	(0.068)	(0.126)	(0.117)	(0.110)	(0.102)	(0.102)	(0.097)	(0.087)	(0.085)	(0.106)	(0.103)
Male		−0.123*		−0.099		−0.044		−0.139†		−0.403***		−0.371***
		(0.054)		(0.096)		(0.083)		(0.080)		(0.069)		(0.087)
Age		0.001		−0.015***		−0.004		0.013***		−0.002		−0.022***
		(0.002)		(0.004)		(0.003)		(0.003)		(0.003)		(0.004)
White		−0.105†		−0.155		−0.100		−0.097		0.073		−0.042
		(0.064)		(0.110)		(0.096)		(0.093)		(0.081)		(0.100)
Education		0.107***		−0.059		−0.107*		−0.123**		−0.242***		−0.007
		(0.031)		(0.055)		(0.046)		(0.045)		(0.038)		(0.049)
Income		−0.026**		−0.061***		−0.030*		−0.001		−0.009		−0.061***
		(0.009)		(0.015)		(0.013)		(0.013)		(0.011)		(0.014)
Republican		0.128*		−1.373***		−1.221***		−0.823***		−0.274***		−0.165†
		(0.055)		(0.096)		(0.084)		(0.080)		(0.069)		(0.087)
Observations	1490	1490	1490	1490	1490	1490	1490	1490	1490	1490	1490	1490
R^2	0.014	0.032	0.002	0.152	0.005	0.148	0.005	0.093	0.017	0.084	0.010	0.062
Adjusted R^2	0.012	0.027	0.000	0.148	0.004	0.143	0.004	0.088	0.016	0.079	0.008	0.057

Notes: Dependent variables include: concern about job loss in the U.S. in the next 10 years; support for universal basic income; unemployment benefits; retraining opportunities; support for slowing down technology adoption; and taxation on corporations to discourage introducing new technologies. Odd-numbered models present results from bivariate OLS models. Even-numbered models control for gender, age, race, education, income, and party affiliation, which were pre-registered in the PAP. Robust standard errors are reported in parentheses. $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$.

Table A4: Effects of Policy Mapping on Job Loss Concern, Redistribution Policies, and Regulation Policies (Experiment 2)

	General Job Loss Concern		Universal Basic Income		Unemployment Benefits		Retraining Opportunities		Technology Adoption		Corporate Taxation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Constant (Baseline)	2.778***	3.049***	5.006***	7.524***	5.282***	6.826***	5.706***	6.458***	4.112***	4.948***	3.453***	5.442***
	(0.049)	(0.136)	(0.093)	(0.227)	(0.077)	(0.196)	(0.065)	(0.179)	(0.060)	(0.179)	(0.075)	(0.214)
Automation Risk Info	0.259***	0.258***	−0.068	−0.092	−0.039	−0.059	−0.084	−0.100	0.205*	0.195*	0.122	0.110
	(0.067)	(0.067)	(0.131)	(0.114)	(0.109)	(0.097)	(0.094)	(0.089)	(0.088)	(0.086)	(0.109)	(0.106)
Policy Mapping	0.160*	0.164*	−0.056	−0.040	0.084	0.092	0.051	0.044	0.206*	0.181*	0.147	0.141
	(0.066)	(0.066)	(0.129)	(0.113)	(0.108)	(0.096)	(0.091)	(0.085)	(0.086)	(0.085)	(0.108)	(0.106)
Counter-Partisan Policy Mapping	0.233***	0.227***	0.113	0.074	0.144	0.117	−0.025	−0.039	0.074	0.084	0.107	0.106
	(0.067)	(0.067)	(0.127)	(0.108)	(0.106)	(0.094)	(0.093)	(0.087)	(0.087)	(0.086)	(0.106)	(0.105)
Male		−0.035		−0.181*		−0.107		−0.184**		−0.418***		−0.400***
		(0.047)		(0.081)		(0.069)		(0.062)		(0.062)		(0.076)
Age		−0.001		−0.011**		−0.003		0.006*		0.002		−0.020***
		(0.002)		(0.004)		(0.003)		(0.003)		(0.003)		(0.003)
White		−0.009		−0.277**		−0.095		−0.117†		0.136†		−0.087
		(0.051)		(0.087)		(0.076)		(0.070)		(0.070)		(0.085)
Education		0.002		−0.065		−0.044		−0.053		−0.152***		−0.124**
		(0.026)		(0.045)		(0.039)		(0.036)		(0.036)		(0.043)
Income		−0.026***		−0.091***		−0.057***		−0.028**		−0.021*		−0.042***
		(0.008)		(0.013)		(0.011)		(0.010)		(0.010)		(0.012)
Republican		−0.034		−1.660***		−1.364***		−0.846***		−0.144*		−0.224**
		(0.047)		(0.082)		(0.070)		(0.064)		(0.061)		(0.076)
Observations	1996	1995	1996	1995	1996	1995	1996	1995	1996	1995	1996	1995
R^2	0.009	0.017	0.001	0.226	0.002	0.191	0.001	0.104	0.004	0.052	0.001	0.054
Adjusted R^2	0.008	0.013	0.000	0.223	0.000	0.188	0.000	0.100	0.002	0.047	0.000	0.050

Notes: Dependent variables include: concern about job loss in the U.S. in the next 10 years; support for universal basic income; unemployment benefits; retraining opportunities; support for slowing down technology adoption; and taxation on corporations to discourage introducing new technologies. Odd-numbered models present results from bivariate OLS models. Even-numbered models control for gender, age, race, education, income, and party affiliation, which were pre-registered in the PAP. Robust standard errors are reported in parentheses. $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$.

Table A5: Pairwise Comparisons of Estimated Marginal Means Across All Models (Experiment 1)

Model	Contrast	Estimate	Std. Error	p-value
General Job Loss Concern	Policy Mapping vs. Control	0.286	0.067	$< .001$
	Co-Partisan Policy Mapping vs. Control	0.220	0.069	0.004
	Co-Partisan Policy Mapping vs. Policy Mapping	-0.067	0.063	0.546
General Job Loss Concern (Covariates)	Policy Mapping vs. Control	0.274	0.067	$< .001$
	Co-Partisan Policy Mapping vs. Control	0.211	0.068	0.005
	Co-Partisan Policy Mapping vs. Policy Mapping	-0.063	0.063	0.584
Universal Basic Income	Policy Mapping vs. Control	0.135	0.130	0.554
	Co-Partisan Policy Mapping vs. Control	0.202	0.126	0.247
	Co-Partisan Policy Mapping vs. Policy Mapping	0.066	0.124	0.852
Universal Basic Income (Covariates)	Policy Mapping vs. Control	0.163	0.121	0.370
	Co-Partisan Policy Mapping vs. Control	0.226	0.117	0.131
	Co-Partisan Policy Mapping vs. Policy Mapping	0.063	0.113	0.842
Unemployment Benefits	Policy Mapping vs. Control	0.251	0.113	0.069
	Co-Partisan Policy Mapping vs. Control	0.278	0.110	0.030
	Co-Partisan Policy Mapping vs. Policy Mapping	0.027	0.105	0.964
Unemployment Benefits (Covariates)	Policy Mapping vs. Control	0.277	0.106	0.024
	Co-Partisan Policy Mapping vs. Control	0.297	0.102	0.010
	Co-Partisan Policy Mapping vs. Policy Mapping	0.020	0.097	0.977
Retraining Opportunities	Policy Mapping vs. Control	0.203	0.106	0.135
	Co-Partisan Policy Mapping vs. Control	0.280	0.102	0.016
	Co-Partisan Policy Mapping vs. Policy Mapping	0.077	0.097	0.706
Retraining Opportunities (Covariates)	Policy Mapping vs. Control	0.221	0.102	0.078
	Co-Partisan Policy Mapping vs. Control	0.280	0.097	0.011
	Co-Partisan Policy Mapping vs. Policy Mapping	0.059	0.092	0.800
Technology Adoption	Policy Mapping vs. Control	0.362	0.086	$< .001$
	Co-Partisan Policy Mapping vs. Control	0.399	0.087	$< .001$
	Co-Partisan Policy Mapping vs. Policy Mapping	0.037	0.087	0.906
Technology Adoption (Covariates)	Policy Mapping vs. Control	0.402	0.083	$< .001$
	Co-Partisan Policy Mapping vs. Control	0.393	0.085	$< .001$
	Co-Partisan Policy Mapping vs. Policy Mapping	-0.009	0.084	0.993
Corporate Taxation	Policy Mapping vs. Control	0.282	0.109	0.027
	Co-Partisan Policy Mapping vs. Control	0.401	0.106	0.001
	Co-Partisan Policy Mapping vs. Policy Mapping	0.119	0.107	0.503
Corporate Taxation (Covariates)	Policy Mapping vs. Control	0.300	0.108	0.015
	Co-Partisan Policy Mapping vs. Control	0.402	0.103	$< .001$
	Co-Partisan Policy Mapping vs. Policy Mapping	0.101	0.104	0.594

Notes: This table shows the results from pairwise comparisons from the models in Table A3, calculated using the R package `emmeans`. For models with covariates, estimated marginal means are calculated by averaging over all covariates. p-values are adjusted for multiple comparisons using Tukey's method.

Table A6: Pairwise Comparisons of Estimated Marginal Means Across Unadjusted Models (Experiment 2)

Model	Contrast	Estimate	Std. Error	p-value
Job Loss Concern	Automation Risk Info vs. Control	0.259	0.067	0.001
	Policy Mapping vs. Control	0.160	0.066	0.072
	Policy Mapping vs. Automation Risk Info	-0.099	0.064	0.408
	Counter-Partisan Policy Mapping vs. Control	0.233	0.067	0.003
	Counter-Partisan Policy Mapping vs. Policy Mapping	0.073	0.064	0.660
Universal Basic Income	Automation Risk Info vs. Control	-0.068	0.131	0.955
	Policy Mapping vs. Control	-0.056	0.129	0.973
	Policy Mapping vs. Automation Risk Info	0.012	0.129	1.000
	Counter-Partisan Policy Mapping vs. Control	0.113	0.127	0.810
	Counter-Partisan Policy Mapping vs. Policy Mapping	0.168	0.124	0.529
Unemployment Benefits	Automation Risk Info vs. Control	-0.039	0.109	0.985
	Policy Mapping vs. Control	0.084	0.108	0.866
	Policy Mapping vs. Automation Risk Info	0.123	0.108	0.667
	Counter-Partisan Policy Mapping vs. Control	0.144	0.106	0.524
	Counter-Partisan Policy Mapping vs. Policy Mapping	0.060	0.105	0.941
Retraining Opportunities	Automation Risk Info vs. Control	-0.084	0.094	0.804
	Policy Mapping vs. Control	0.051	0.091	0.943
	Policy Mapping vs. Automation Risk Info	0.135	0.092	0.457
	Counter-Partisan Policy Mapping vs. Control	-0.025	0.093	0.993
	Counter-Partisan Policy Mapping vs. Policy Mapping	-0.076	0.091	0.840
Technology Adoption	Automation Risk Info vs. Control	0.205	0.088	0.091
	Policy Mapping vs. Control	0.206	0.086	0.076
	Policy Mapping vs. Automation Risk Info	0.001	0.088	1.000
	Counter-Partisan Policy Mapping vs. Control	0.074	0.087	0.830
	Counter-Partisan Policy Mapping vs. Policy Mapping	-0.132	0.088	0.437
Corporate Taxation	Automation Risk Info vs. Control	0.122	0.109	0.677
	Policy Mapping vs. Control	0.147	0.108	0.519
	Policy Mapping vs. Automation Risk Info	0.025	0.111	0.996
	Counter-Partisan Policy Mapping vs. Control	0.107	0.106	0.745
	Counter-Partisan Policy Mapping vs. Policy Mapping	-0.040	0.108	0.982

Notes: This table shows the results from pairwise comparisons from the models in Table A4, calculated using the R package `emmeans`. For models with covariates, estimated marginal means are calculated by averaging over all covariates. p-values are adjusted for multiple comparisons using Tukey's method.

Table A7: Pairwise Comparisons of Estimated Marginal Means Across Models with Covariates (Experiment 2)

Model	Contrast	Estimate	Std. Error	p-value
Job Loss Concern (Covariates)	Automation Risk Info vs. Control	0.258	0.067	0.001
	Policy Mapping vs. Control	0.164	0.066	0.064
	Policy Mapping vs. Automation Risk Info	-0.095	0.064	0.445
	Counter-Partisan Policy Mapping vs. Control	0.227	0.067	0.004
	Counter-Partisan Policy Mapping vs. Policy Mapping	0.064	0.064	0.754
Universal Basic Income (Covariates)	Automation Risk Info vs. Control	-0.092	0.114	0.852
	Policy Mapping vs. Control	-0.040	0.113	0.984
	Policy Mapping vs. Automation Risk Info	0.051	0.117	0.972
	Counter-Partisan Policy Mapping vs. Control	0.074	0.108	0.905
	Counter-Partisan Policy Mapping vs. Policy Mapping	0.114	0.112	0.737
Unemployment Benefits (Covariates)	Automation Risk Info vs. Control	-0.059	0.097	0.930
	Policy Mapping vs. Control	0.092	0.096	0.771
	Policy Mapping vs. Automation Risk Info	0.151	0.099	0.418
	Counter-Partisan Policy Mapping vs. Control	0.117	0.094	0.595
	Counter-Partisan Policy Mapping vs. Policy Mapping	0.025	0.096	0.994
Retraining Opportunities (Covariates)	Automation Risk Info vs. Control	-0.100	0.089	0.673
	Policy Mapping vs. Control	0.044	0.085	0.957
	Policy Mapping vs. Automation Risk Info	0.144	0.088	0.359
	Counter-Partisan Policy Mapping vs. Control	-0.039	0.087	0.970
	Counter-Partisan Policy Mapping vs. Policy Mapping	-0.083	0.087	0.777
Technology Adoption (Covariates)	Automation Risk Info vs. Control	0.195	0.086	0.105
	Policy Mapping vs. Control	0.181	0.085	0.142
	Policy Mapping vs. Automation Risk Info	-0.014	0.086	0.999
	Counter-Partisan Policy Mapping vs. Control	0.084	0.086	0.764
	Counter-Partisan Policy Mapping vs. Policy Mapping	-0.097	0.087	0.678
Corporate Taxation (Covariates)	Automation Risk Info vs. Control	0.110	0.106	0.727
	Policy Mapping vs. Control	0.141	0.106	0.543
	Policy Mapping vs. Automation Risk Info	0.030	0.107	0.992
	Counter-Partisan Policy Mapping vs. Control	0.106	0.105	0.740
	Counter-Partisan Policy Mapping vs. Policy Mapping	-0.034	0.106	0.988

Notes: This table shows the results from pairwise comparisons from the models in Table A4, calculated using the R package `emmeans`. For models with covariates, estimated marginal means are calculated by averaging over all covariates. p-values are adjusted for multiple comparisons using Tukey's method.

A3 Heterogeneous Analysis

Party Affiliation. Existing studies on partisan motivated reasoning offer mixed findings on ideological asymmetry. Some research suggests conservatives are less willing to update their views in response to uncongenial information than liberals are (e.g., Morisi, Jost, and Singh 2019). However, other studies find little evidence for this asymmetry, arguing that both conservatives and liberals exhibit similar levels of political bias (Guay and Johnston 2022). In the context of AI's negative impact on the labor market, Mitts and Raviv (2025) found that Republicans in the U.S. are more likely to be persuaded by information from Republican leaders, while Democrats show no such tendency. I explore whether partisan affiliation impacts the treatment effect in my studies. The results are display in Figure A1.

Figure A1 displays the estimated marginal means by Party ID for each outcome across all conditions. The left column presents the results from Experiment 1, while the right column shows those from Experiment 2. The figure highlights three main findings. First, Panels A–F show substantial partisan divides on redistribution policies, but not on general job loss concern or regulation policies. Second, Democrats and Republicans generally respond in the same direction and with similar magnitudes for most outcomes, suggesting no meaningful party differences in the effects. Nevertheless, two exceptions stand out. In Experiment 1, support for slowing technology adoption increased by approximately 0.54 points among Democrats and 0.19 points among Republicans, a difference that is statistically significant ($p < .05$). In Experiment 2, the counter-partisan policy-mapping condition increased support for corporate taxation by about 0.39 points among Republicans but decreased it by about 0.19 points among Democrats, a significant difference ($p < .01$). These patterns suggest limited heterogeneity by Party ID, with substantive differences only evident for technology adoption in Experiment 1 and corporate taxation under the counter-partisan cue in Experiment 2.

Strength of Partisanship. In addition to partisan affiliation, the strength of one's partisan identity may moderate the effects of policy mapping, as strong partisans are more likely to follow their party leaders' cues (Bakker, Lelkes, and Malka 2020). I therefore examine the heterogeneous treatment effect across strong and weak partisan groups. Figure A2 indicates that none of the treatments produced a heterogeneous effect between weak and strong partisan respondents, even in conditions with a partisan cue. This finding suggests that the automation issue has not yet been politicized and thus the partisan cue does not serve as a cue to reshape respondents' preferences.

One might be concerned that strong Democrats and Republicans are fundamentally different. I also tested a model with the interaction between treatment, strength of partisanship, and party affiliation. The results indicated no heterogeneous effect. See Table A10 and A11.

Occupational Vulnerability. Occupational vulnerability to automation is expected to moderate the effects of policy mapping. Individuals facing different levels of risk of being replaced by automation may respond differently when informed about such risks. Those in more vulnerable occupations are more likely to be exposed to the threat of automation and thus more likely to react actively to policy mapping, whereas those in less vulnerable positions may be less sensitive. To measure occupational vulnerability, I use the Routine Task Intensity (RTI) index. The RTI index is constructed as the difference between the log of routine tasks and the sum of the logs of abstract and manual tasks (see Autor and Dorn 2013; Goos, Manning, and Salomons 2014). This measure captures objective automation risk, representing an expert prediction. However, it does not necessarily capture subjective automation risk. To avoid priming automation risk, I did not include a question to measure respondents' subjective automation risks in the pre-treatment section. Objective automation risk is a good proxy for measuring different levels of vulnerability.

Figure A3 presents the estimated marginal means for groups with low, medium, and high RTI. In the first experiment, RTI is positively associated with baseline support for retraining programs but does not predict baseline support for the other policies. The second experiment reveals a different pattern: RTI is positively associated only with support for UBI. Across both experiments, however, evidence of moderation is limited: the interaction terms are small and generally not statistically significant, suggesting that occupational vulnerability does not moderate responses to policy mapping.

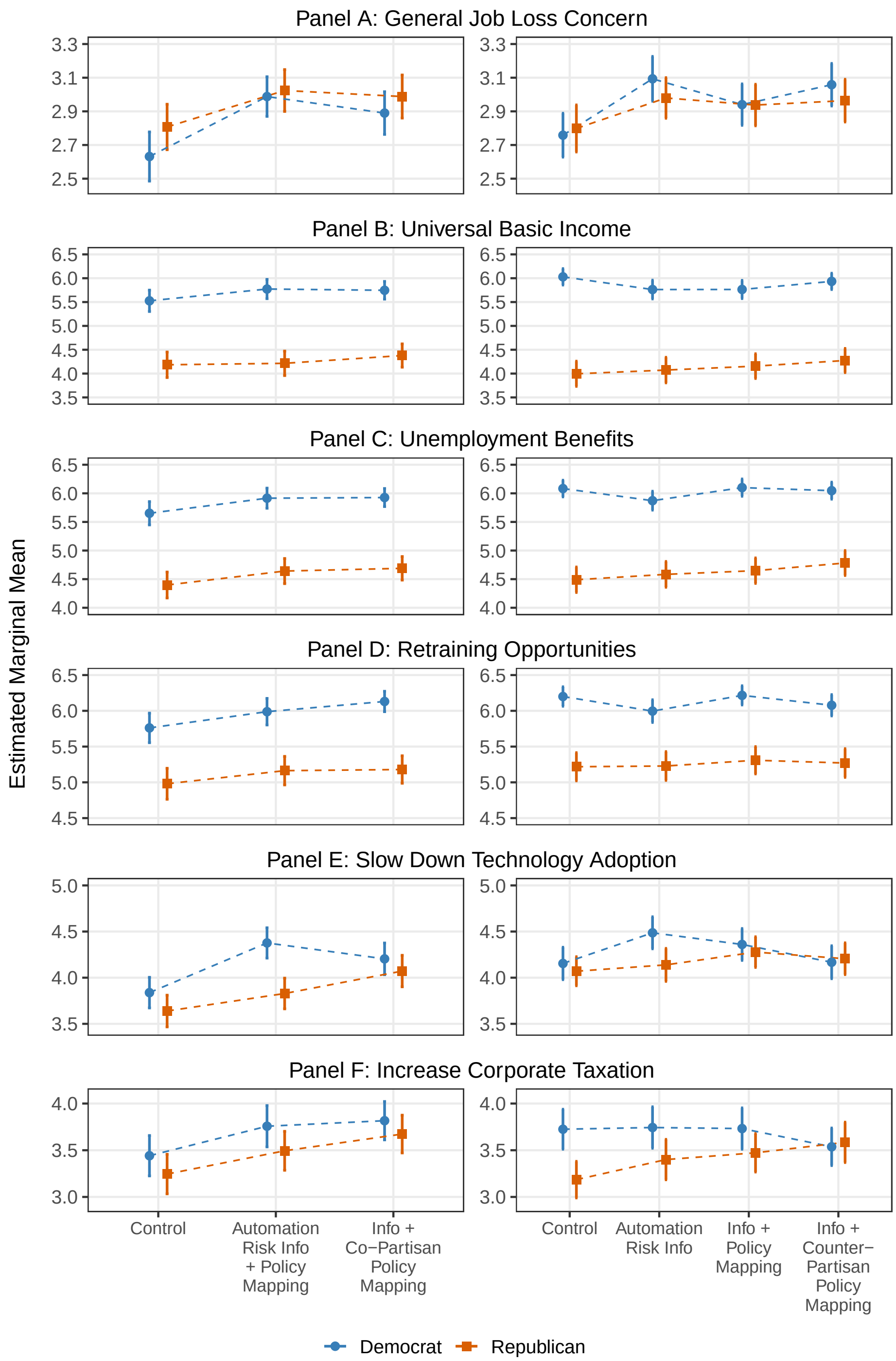

Figure A1: Estimated Marginal Means for General Job Loss Concern, Redistribution Policies, and Regulation Policies, by Treatment Conditions and Party Affiliation

Notes: This figure shows estimated marginal means for all outcome variables. The left column shows results for the first experiment (N = 1,490), and the right column shows results for the second experiment (N = 1,996). These means are derived from OLS models that include the main effects of treatment conditions, respondent party affiliation, and their interaction. Error bars represent 95% CIs around the estimates. Full regression results are reported in Table A8.

Table A8: Effects of Policy Mapping and Party Affiliation on General Job Loss Concern, Redistribution Policies, and Regulation Policies

	Risk	UBI	Unemp.	Train	Adopt	Tax
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment 1						
Policy Mapping	0.356***	0.247	0.263†	0.227	0.538***	0.316*
	(0.096)	(0.154)	(0.137)	(0.142)	(0.119)	(0.158)
Co-Partisan Policy Mapping	0.258**	0.221	0.275*	0.369**	0.366**	0.375*
	(0.099)	(0.148)	(0.131)	(0.128)	(0.122)	(0.152)
Republican	0.176†	−1.342***	−1.254***	−0.781***	−0.200	−0.196
	(0.101)	(0.178)	(0.156)	(0.153)	(0.122)	(0.154)
Policy Mapping × Republican	−0.140	−0.217	−0.019	−0.043	−0.348*	−0.067
	(0.134)	(0.244)	(0.212)	(0.206)	(0.170)	(0.219)
Co-Partisan Policy Mapping × Republican	−0.077	−0.027	0.017	−0.170	0.067	0.053
	(0.137)	(0.237)	(0.204)	(0.195)	(0.173)	(0.213)
Constant	2.632***	5.526***	5.652***	5.761***	3.838***	3.441***
	(0.075)	(0.114)	(0.104)	(0.105)	(0.085)	(0.110)
Observations	1490	1490	1490	1490	1490	1490
R^2	0.017	0.129	0.137	0.077	0.033	0.013
Adjusted R^2	0.013	0.126	0.134	0.073	0.030	0.010
Panel B: Experiment 2						
Automation Risk Info	0.335***	−0.267*	−0.212†	−0.204†	0.332**	0.018
	(0.095)	(0.134)	(0.113)	(0.107)	(0.126)	(0.157)
Policy Mapping	0.182*	−0.265*	0.017	0.016	0.206	0.007
	(0.092)	(0.132)	(0.107)	(0.098)	(0.126)	(0.157)
Counter-Partisan Policy Mapping	0.300**	−0.095	−0.037	−0.122	0.013	−0.188
	(0.093)	(0.124)	(0.106)	(0.103)	(0.128)	(0.150)
Republican	0.040	−2.033***	−1.594***	−0.982***	−0.084	−0.540***
	(0.098)	(0.161)	(0.136)	(0.122)	(0.120)	(0.148)
Automation Risk Info × Republican	−0.154	0.344	0.306	0.214	−0.263	0.197
	(0.134)	(0.234)	(0.197)	(0.179)	(0.175)	(0.217)
Policy Mapping × Republican	−0.043	0.426†	0.142	0.074	0.002	0.280
	(0.132)	(0.230)	(0.193)	(0.171)	(0.172)	(0.214)
Counter-Partisan Policy Mapping × Republican	−0.135	0.374†	0.329†	0.174	0.123	0.588**
	(0.134)	(0.224)	(0.191)	(0.177)	(0.175)	(0.211)
Constant	2.758***	6.029***	6.083***	6.200***	4.154***	3.725***
	(0.067)	(0.088)	(0.074)	(0.069)	(0.090)	(0.109)
Observations	1996	1996	1996	1996	1996	1996
R^2	0.011	0.190	0.175	0.090	0.008	0.011
Adjusted R^2	0.007	0.187	0.172	0.086	0.005	0.008

Notes: Robust standard errors are reported in parentheses. $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$.

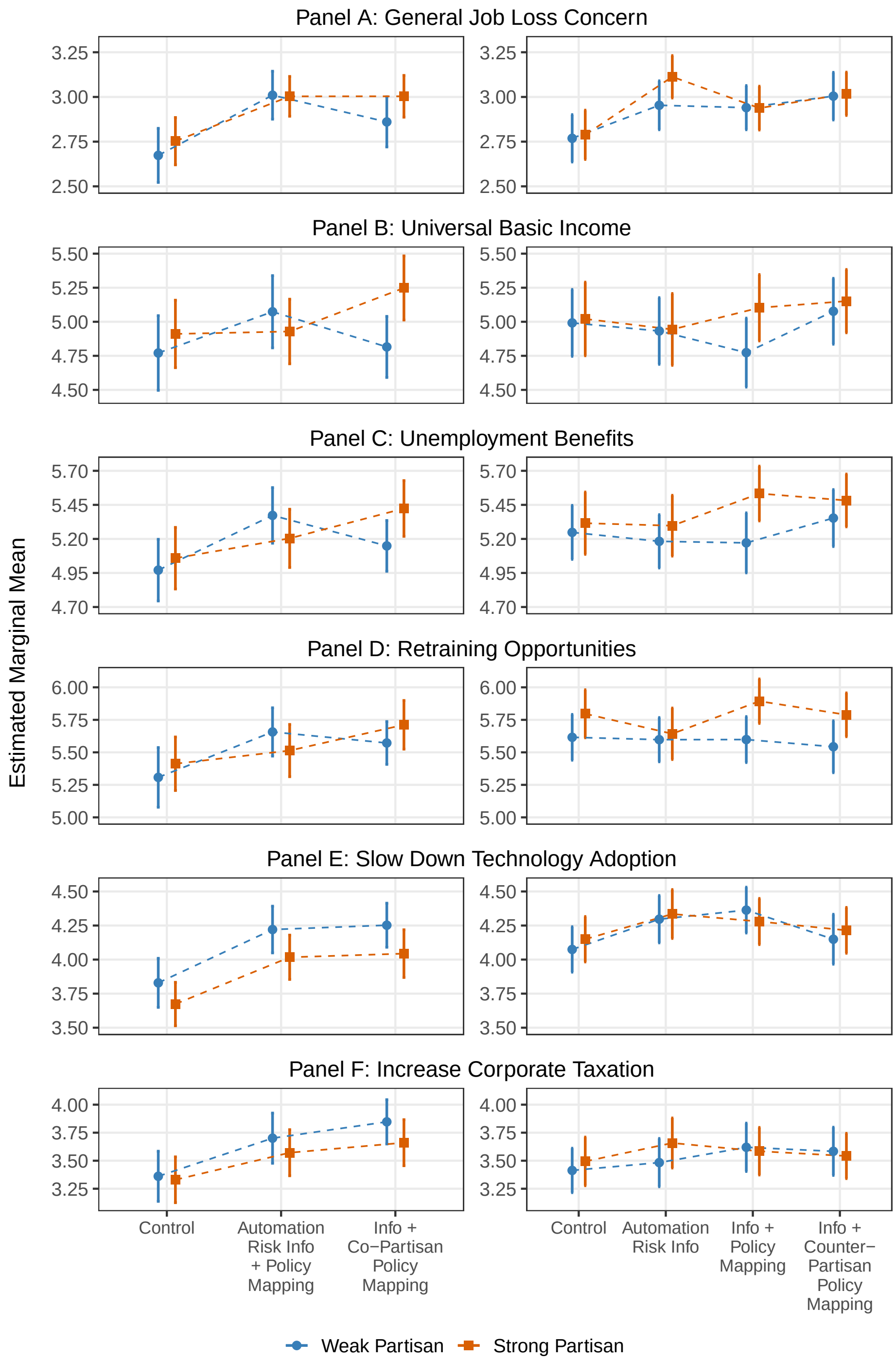

Figure A2: Estimated Marginal Means for General Job Loss Concern, Redistribution Policies, and Regulation Policies by Treatment Conditions and Strength of Partisanship

Notes: This figure shows estimated marginal means for all outcome variables. The left column shows results for the first experiment (N = 1,490), and the right column shows results for the second experiment (N = 1,996). These means are derived from OLS models that include the main effects of treatment conditions, respondent strength of partisanship, and their interaction. Strong Democrats/Republicans were categorized as strong partisans, while not very strong and lean Democrats/Republicans were categorized as weak partisans. Error bars represent 95% CIs around the estimates. Full regression results are reported in Table A9.

Table A9: Effects of Policy Mapping and Strength of Partisanship on Job Loss Concern, Redistribution, and Regulation Policies

	Risk	UBI	Unemp.	Train	Adopt	Tax
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment 1						
Policy Mapping	0.337**	0.303	0.402**	0.350*	0.391**	0.340*
	(0.103)	(0.195)	(0.155)	(0.151)	(0.127)	(0.161)
Co-Partisan Policy Mapping	0.187†	0.045	0.178	0.265†	0.423***	0.486**
	(0.105)	(0.181)	(0.150)	(0.144)	(0.123)	(0.152)
Strong Partisan	0.079	0.140	0.088	0.105	−0.156	−0.031
	(0.103)	(0.188)	(0.163)	(0.157)	(0.123)	(0.155)
Policy Mapping × Strong Partisan	−0.086	−0.285	−0.256	−0.248	−0.048	−0.098
	(0.136)	(0.262)	(0.222)	(0.210)	(0.172)	(0.219)
Co-Partisan Policy Mapping × Strong Partisan	0.064	0.293	0.187	0.035	−0.053	−0.155
	(0.138)	(0.251)	(0.216)	(0.202)	(0.173)	(0.212)
Constant	2.673***	4.771***	4.971***	5.307***	3.829***	3.361***
	(0.077)	(0.140)	(0.115)	(0.117)	(0.092)	(0.114)
Observations	1490	1490	1490	1490	1490	1490
R^2	0.016	0.006	0.008	0.007	0.022	0.011
Adjusted R^2	0.012	0.003	0.005	0.004	0.019	0.008
Panel B: Experiment 2						
Automation Risk Info	0.185†	−0.060	−0.066	−0.018	0.222†	0.070
	(0.097)	(0.177)	(0.142)	(0.125)	(0.123)	(0.149)
Policy Mapping	0.172†	−0.218	−0.077	−0.017	0.289*	0.206
	(0.093)	(0.180)	(0.151)	(0.127)	(0.121)	(0.150)
Counter-Partisan Policy Mapping	0.236*	0.085	0.105	−0.073	0.075	0.170
	(0.096)	(0.176)	(0.147)	(0.136)	(0.127)	(0.149)
Strong Partisan	0.020	0.029	0.067	0.181	0.075	0.081
	(0.098)	(0.186)	(0.154)	(0.130)	(0.120)	(0.150)
Automation Risk Info × Strong Partisan	0.140	−0.018	0.047	−0.136	−0.037	0.094
	(0.134)	(0.262)	(0.216)	(0.186)	(0.175)	(0.218)
Policy Mapping × Strong Partisan	−0.022	0.300	0.295	0.114	−0.159	−0.116
	(0.132)	(0.259)	(0.217)	(0.181)	(0.171)	(0.215)
Counter-Partisan Policy Mapping × Strong Partisan	−0.007	0.045	0.062	0.065	−0.010	−0.122
	(0.134)	(0.253)	(0.212)	(0.186)	(0.175)	(0.212)
Constant	2.769***	4.992***	5.248***	5.616***	4.074***	3.413***
	(0.068)	(0.125)	(0.101)	(0.090)	(0.085)	(0.101)
Observations	1996	1996	1996	1996	1996	1996
R^2	0.011	0.003	0.005	0.006	0.005	0.002
Adjusted R^2	0.007	0.000	0.002	0.003	0.001	−0.002

Notes: Strong Democrats/Republicans were categorized as strong partisans, while not very strong and lean Democrats/Republicans were categorized as weak partisans. Robust standard errors are reported in parentheses. $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$.

Table A10: Effects of Policy Mapping, Strong Partisanship, and Party Affiliation on Job Loss Concern, Redistribution, and Regulation Policies (Experiment 1)

	Risk	UBI	Unemp.	Train	Adopt	Tax
	(1)	(2)	(3)	(4)	(5)	(6)
Policy Mapping	0.413**	0.573*	0.596**	0.599**	0.565**	0.524*
	(0.158)	(0.241)	(0.202)	(0.210)	(0.189)	(0.243)
Co-Partisan Policy Mapping	0.239	−0.028	0.232	0.403*	0.325†	0.533*
	(0.156)	(0.237)	(0.205)	(0.205)	(0.182)	(0.220)
Strong Partisan	0.021	0.476*	0.530*	0.489*	−0.029	0.099
	(0.152)	(0.238)	(0.211)	(0.217)	(0.172)	(0.221)
Republican	0.104	−0.885**	−0.682**	−0.297	−0.050	−0.039
	(0.155)	(0.273)	(0.227)	(0.235)	(0.184)	(0.229)
Policy Mapping × Strong Partisan	−0.092	−0.537†	−0.549*	−0.612*	−0.043	−0.339
	(0.199)	(0.312)	(0.272)	(0.282)	(0.244)	(0.319)
Co-Partisan Policy Mapping × Strong Partisan	0.036	0.464	0.108	−0.028	0.069	−0.270
	(0.202)	(0.299)	(0.264)	(0.261)	(0.245)	(0.303)
Policy Mapping × Republican	−0.144	−0.494	−0.355	−0.463	−0.325	−0.343
	(0.208)	(0.368)	(0.297)	(0.296)	(0.254)	(0.324)
Co-Partisan Policy Mapping × Republican	−0.098	0.137	−0.102	−0.262	0.185	−0.089
	(0.211)	(0.351)	(0.291)	(0.286)	(0.248)	(0.305)
Policy Mapping × Strong Partisan × Republican	0.001	0.450	0.552	0.695†	−0.046	0.461
	(0.273)	(0.490)	(0.417)	(0.409)	(0.342)	(0.439)
Co-Partisan Policy Mapping × Strong Partisan × Republican	0.048	−0.325	0.169	0.117	−0.233	0.231
	(0.277)	(0.474)	(0.405)	(0.391)	(0.345)	(0.425)
Constant	2.619***	5.237***	5.330***	5.464***	3.856***	3.381***
	(0.117)	(0.192)	(0.164)	(0.173)	(0.133)	(0.165)
Observations	1490	1490	1490	1490	1490	1490
R^2	0.020	0.143	0.152	0.087	0.043	0.016
Adjusted R^2	0.013	0.137	0.146	0.081	0.036	0.009

Notes: Robust standard errors are reported in parentheses. $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$.

Table A11: Effects of Policy Mapping, Strong Partisanship, and Party Affiliation on Job Loss Concern, Redistribution, and Regulation Policies (Experiment 2)

	Risk	UBI	Unemp.	Train	Adopt	Tax
	(1)	(2)	(3)	(4)	(5)	(6)
Automation Risk Info	0.303*	−0.369†	−0.219	−0.175	0.349†	−0.064
	(0.152)	(0.206)	(0.176)	(0.161)	(0.190)	(0.230)
Policy Mapping	0.276†	−0.486*	−0.124	−0.114	0.314†	0.200
	(0.141)	(0.227)	(0.195)	(0.171)	(0.186)	(0.231)
Counter-Partisan Policy Mapping	0.275†	0.085	−0.075	−0.257	−0.048	−0.135
	(0.145)	(0.211)	(0.191)	(0.182)	(0.204)	(0.227)
Strong Partisan	0.078	0.238	0.402**	0.305*	0.189	0.256
	(0.135)	(0.180)	(0.153)	(0.142)	(0.180)	(0.218)
Republican	0.096	−1.596***	−1.076***	−0.729***	0.047	−0.296
	(0.136)	(0.223)	(0.187)	(0.171)	(0.174)	(0.205)
Automation Risk Info × Strong Partisan	0.060	0.193	0.024	−0.045	−0.026	0.156
	(0.194)	(0.270)	(0.227)	(0.215)	(0.254)	(0.314)
Policy Mapping × Strong Partisan	−0.165	0.373	0.230	0.215	−0.193	−0.340
	(0.186)	(0.273)	(0.225)	(0.203)	(0.252)	(0.315)
Counter-Partisan Policy Mapping × Strong Partisan	0.030	0.116	0.009	0.172	0.071	−0.114
	(0.189)	(0.260)	(0.226)	(0.218)	(0.262)	(0.302)
Automation Risk Info × Republican	−0.222	0.428	0.180	0.226	−0.244	0.229
	(0.196)	(0.326)	(0.267)	(0.241)	(0.249)	(0.302)
Policy Mapping × Republican	−0.187	0.442	0.056	0.156	−0.045	0.004
	(0.187)	(0.332)	(0.284)	(0.245)	(0.245)	(0.302)
Counter-Partisan Policy Mapping × Republican	−0.070	0.517	0.340	0.337	0.209	0.533†
	(0.193)	(0.320)	(0.277)	(0.263)	(0.260)	(0.301)
Automation Risk Info × Strong Partisan × Republican	0.139	−0.056	0.334	−0.007	−0.037	−0.026
	(0.272)	(0.470)	(0.396)	(0.361)	(0.351)	(0.434)
Policy Mapping × Strong Partisan × Republican	0.270	0.122	0.307	−0.095	0.074	0.537
	(0.268)	(0.468)	(0.395)	(0.349)	(0.344)	(0.430)
Counter-Partisan Policy Mapping × Strong Partisan × Republican	−0.110	−0.158	0.108	−0.215	−0.119	0.139
	(0.271)	(0.455)	(0.391)	(0.364)	(0.355)	(0.426)
Constant	2.714***	5.895***	5.857***	6.029***	4.048***	3.581***
	(0.102)	(0.144)	(0.128)	(0.115)	(0.135)	(0.159)
Observations	1996	1996	1996	1996	1996	1996
R^2	0.013	0.206	0.194	0.101	0.011	0.015
Adjusted R^2	0.006	0.200	0.187	0.094	0.004	0.007

Notes: Robust standard errors are reported in parentheses. $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$.

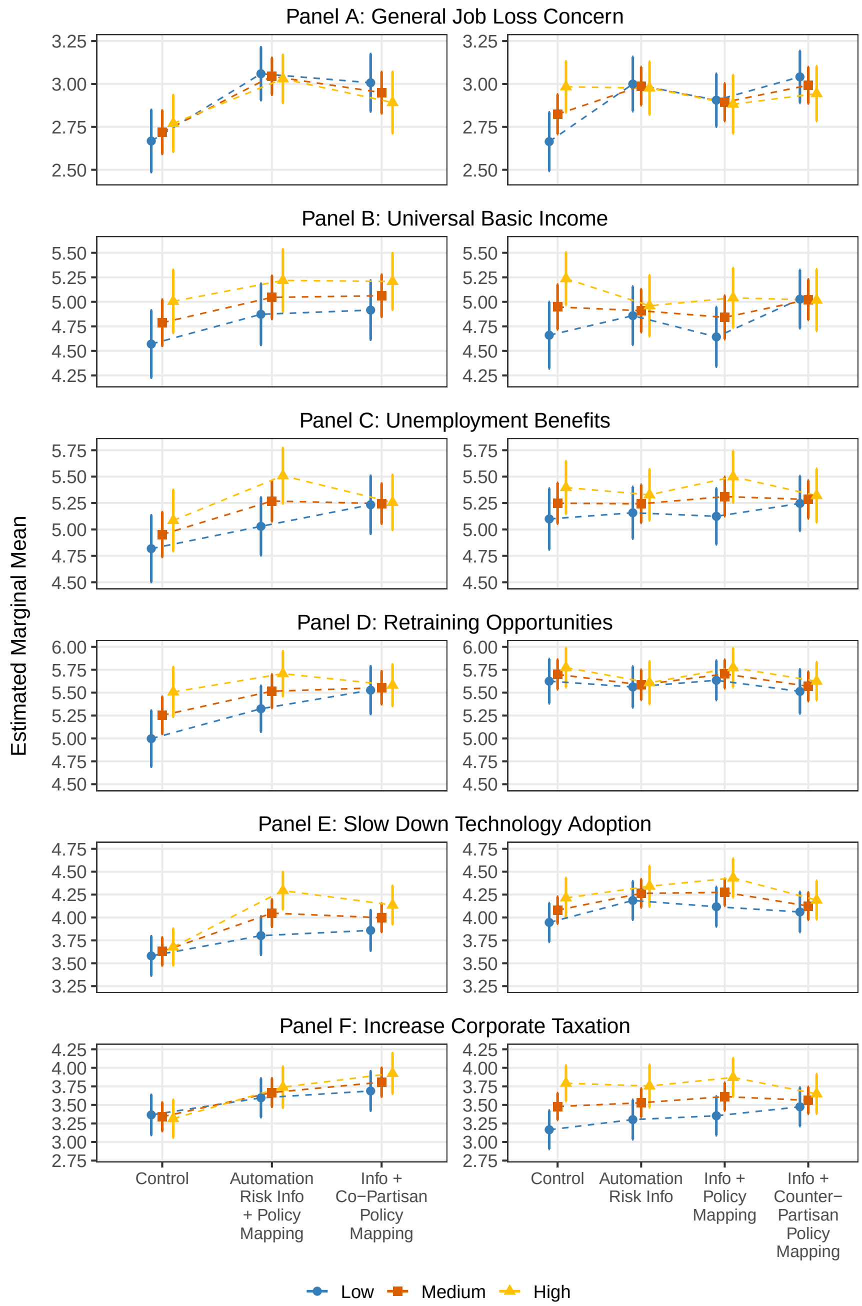

Figure A3: Estimated Marginal Means for General Job Loss Concern, Redistribution Policies, and Regulation Policies by Treatment Conditions and Occupational Vulnerability

Notes: This figure shows estimated marginal means for all outcome variables. The left column shows results for the first experiment (N = 9,16), and the right column shows results for the second experiment (N = 1,337). These means are derived from OLS models that include the main effects of treatment conditions, respondent RTI, and their interaction. RTI data are sourced from Goos, Manning, and Salomons (2014). As RTI is a continuous variable, the estimated marginal means are presented at its mean value (Medium) and at one standard deviation below (Low) and above (High) the mean, for visualization purposes. Error bars represent 95% CIs around the estimates. Full results are reported in Table A12.

Table A12: Effects of Policy Mapping and Occupational Vulnerability on Job Loss Concern, Redistribution, and Regulation Policies

	Risk	UBI	Unemp.	Train	Adopt	Tax
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment 1						
Policy Mapping	0.311***	0.248	0.342*	0.249†	0.464***	0.349*
	(0.084)	(0.167)	(0.147)	(0.139)	(0.110)	(0.141)
Co-Partisan Policy Mapping	0.206*	0.258	0.265†	0.249†	0.389***	0.499***
	(0.091)	(0.164)	(0.146)	(0.137)	(0.112)	(0.141)
RTI	0.051	0.216†	0.133	0.254*	0.048	−0.025
	(0.060)	(0.119)	(0.110)	(0.105)	(0.071)	(0.091)
Policy Mapping × RTI	−0.066	−0.045	0.106	−0.063	0.196†	0.096
	(0.079)	(0.165)	(0.145)	(0.135)	(0.101)	(0.132)
Co-Partisan Policy Mapping × RTI	−0.108	−0.071	−0.121	−0.227†	0.090	0.143
	(0.087)	(0.157)	(0.145)	(0.134)	(0.104)	(0.131)
Constant	2.731***	4.837***	4.982***	5.311***	3.640***	3.335***
	(0.065)	(0.121)	(0.109)	(0.103)	(0.078)	(0.098)
Observations	916	916	916	916	916	916
R^2	0.019	0.012	0.014	0.018	0.032	0.015
Adjusted R^2	0.013	0.006	0.009	0.013	0.026	0.010
Panel B: Experiment 2						
Automation Risk Info	0.133	−0.082	−0.017	−0.123	0.173	0.034
	(0.082)	(0.161)	(0.135)	(0.119)	(0.112)	(0.138)
Policy Mapping	0.039	−0.123	0.070	0.004	0.199†	0.123
	(0.082)	(0.161)	(0.136)	(0.115)	(0.109)	(0.134)
Counter-Partisan Policy Mapping	0.131	0.021	0.015	−0.133	0.032	0.042
	(0.081)	(0.157)	(0.135)	(0.116)	(0.110)	(0.132)
RTI	0.154**	0.279**	0.144	0.072	0.130†	0.303***
	(0.055)	(0.100)	(0.093)	(0.080)	(0.077)	(0.085)
Automation Risk Info × RTI	−0.166*	−0.230	−0.062	−0.049	−0.055	−0.085
	(0.077)	(0.144)	(0.123)	(0.112)	(0.108)	(0.130)
Policy Mapping × RTI	−0.166*	−0.086	0.037	−0.005	0.022	−0.053
	(0.081)	(0.144)	(0.127)	(0.108)	(0.109)	(0.124)
Counter-Partisan Policy Mapping × RTI	−0.202*	−0.283†	−0.108	−0.018	−0.067	−0.219†
	(0.079)	(0.150)	(0.130)	(0.111)	(0.109)	(0.127)
Constant	2.853***	5.001***	5.275***	5.713***	4.104***	3.536***
	(0.058)	(0.114)	(0.097)	(0.082)	(0.077)	(0.092)
Observations	1337	1337	1337	1337	1337	1337
R^2	0.011	0.009	0.006	0.003	0.010	0.019
Adjusted R^2	0.006	0.004	0.001	−0.002	0.005	0.014

Notes: The analysis excluded observations from respondents who were not employed in a full- or part-time job at the time of the survey, as well as those who selected “other” for their occupation. RTI data are sourced from Goos, Manning, and Salomons (2014). Robust standard errors are reported in parentheses. $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$.

A4 Robustness Check

A4.1 Attention Check

Table A13 shows that the sample includes only respondents who passed both attention checks. The first attention check instructed respondents to select "orange juice" as their favorite drink. The second asked, "during your visits to the zoo, how many times have you seen a real, living unicorn?"; the correct response was "Never". In the first experiment, a total of 70 respondents failed at least one attention check, with only two failing both. In the second experiment, 27 respondents failed at least one attention check, and no one failed both.

Table A13: Effects of Policy Mapping on General Job Loss Concern, Redistribution Policies, and Regulation Policies (Attention Check)

	Risk	UBI	Unemp.	Train	Adopt	Tax
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment 1						
Policy Mapping	0.311***	0.145	0.249*	0.208†	0.345***	0.321**
	(0.068)	(0.134)	(0.116)	(0.108)	(0.087)	(0.111)
Co-Partisan Policy Mapping	0.241***	0.223†	0.289**	0.284**	0.423***	0.454***
	(0.070)	(0.130)	(0.112)	(0.103)	(0.087)	(0.109)
Constant	2.674***	4.853***	5.043***	5.404***	3.791***	3.285***
	(0.052)	(0.097)	(0.085)	(0.080)	(0.062)	(0.078)
Observations	1418	1418	1418	1418	1418	1418
R^2	0.016	0.002	0.006	0.006	0.019	0.013
Adjusted R^2	0.015	0.001	0.004	0.004	0.017	0.011
Panel B: Experiment 2						
Automation Risk Info	0.261***	−0.080	−0.066	−0.097	0.200*	0.117
	(0.068)	(0.132)	(0.109)	(0.094)	(0.088)	(0.110)
Policy Mapping	0.159*	−0.053	0.074	0.043	0.192*	0.136
	(0.067)	(0.130)	(0.108)	(0.091)	(0.086)	(0.108)
Counter-Partisan Policy Mapping	0.235***	0.109	0.131	−0.031	0.050	0.111
	(0.067)	(0.127)	(0.106)	(0.092)	(0.088)	(0.107)
Constant	2.771***	5.017***	5.305***	5.721***	4.132***	3.443***
	(0.049)	(0.094)	(0.077)	(0.065)	(0.060)	(0.076)
Observations	1969	1969	1969	1969	1969	1969
R^2	0.009	0.001	0.002	0.001	0.004	0.001
Adjusted R^2	0.008	0.000	0.000	0.000	0.002	−0.001

Notes: The sample includes only respondents who passed both attention checks. Robust standard errors are reported in parentheses. $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$.

A4.2 Duplicated IP Address

In the first experiment, 13 IP addresses were duplicated and 4 were triplicated in the final sample. The second experiment had a similar issue, with 10 IP addresses appearing twice and one appearing three times. One reason is these respondents have different accounts on Prolific and completed the survey multiple times. Another possible reason is they are in the same household and thus share the same IP address. Regardless of the specific conditions, their responses might be unreliable. First, if they completed the survey multiple times, they might have conjectured the study's purpose and intentionally biased their responses. Second, if they were in the same household, the treatment might spill over to other participants. Table A14 reports the results of analyses excluding these respondents, confirming that the main findings remain robust.

Table A14: Effects of Policy Mapping on General Job Loss Concern, Redistribution Policies, and Regulation Policies (IP Address Check)

	Risk	UBI	Unemp.	Train	Adopt	Tax
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment 1						
Policy Mapping	0.288***	0.128	0.249*	0.190†	0.378***	0.273*
	(0.068)	(0.132)	(0.114)	(0.107)	(0.087)	(0.111)
Co-Partisan Policy Mapping	0.220**	0.188	0.266*	0.264*	0.410***	0.379***
	(0.069)	(0.128)	(0.111)	(0.102)	(0.088)	(0.108)
Constant	2.710***	4.857***	5.031***	5.391***	3.749***	3.348***
	(0.051)	(0.095)	(0.084)	(0.079)	(0.062)	(0.078)
Observations	1452	1452	1452	1452	1452	1452
R^2	0.014	0.002	0.005	0.005	0.018	0.009
Adjusted R^2	0.012	0.000	0.004	0.004	0.017	0.007
Panel B: Experiment 2						
Automation Risk Info	0.260***	−0.056	−0.048	−0.092	0.212*	0.152
	(0.067)	(0.132)	(0.109)	(0.094)	(0.088)	(0.109)
Policy Mapping	0.157*	−0.038	0.087	0.055	0.210*	0.170
	(0.067)	(0.131)	(0.110)	(0.092)	(0.086)	(0.109)
Counter-Partisan Policy Mapping	0.226***	0.117	0.138	−0.024	0.074	0.112
	(0.067)	(0.127)	(0.107)	(0.093)	(0.087)	(0.106)
Constant	2.782***	5.000***	5.285***	5.704***	4.107***	3.442***
	(0.049)	(0.094)	(0.078)	(0.066)	(0.060)	(0.075)
Observations	1973	1973	1973	1973	1973	1973
R^2	0.009	0.001	0.002	0.001	0.004	0.001
Adjusted R^2	0.008	0.000	0.000	0.000	0.003	0.000

Notes: The sample includes only respondents with unique IP address. Robust standard errors are reported in parentheses. $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$.

A4.3 Duration

Table A15 reports the descriptive statistics for survey completion duration in seconds. Although Prolific already screens out respondents who complete the survey too quickly or too slowly, there are still concerns that some respondents may not have paid sufficient attention, especially those in the treatment groups who had to read a vignette. Therefore, I excluded outliers based on the the 1.5 Interquartile Range (IQR) rule, which defines outliers as values that fall below $Q1 - 1.5 * IQR$ or above $Q3 + 1.5 * IQR$ for each group. Table A16 shows that the main results are robust after excluding outliers.

Table A15: Respondents' Survey Completion Duration in Seconds

Treatment Conditions	N	Mean	SD	Median	Min	Max
Panel A: Experiment 1						
Control	496	261.72	272.26	177.0	61	3378
Policy Mapping	498	321.76	268.25	248.0	74	3505
Co-Partisan Policy Mapping	496	333.72	225.04	254.5	71	1416
Panel B: Experiment 2						
Control	483	236.62	229.60	164.0	59	2652
Automation Risk Info	502	263.62	195.99	202.5	66	1684
Policy Mapping	506	289.22	225.06	226.0	63	1884
Counter-Partisan Policy Mapping	505	302.90	225.89	231.0	60	1697

Table A16: Effects of Policy Mapping on General Job Loss Concern, Redistribution Policies, and Regulation Policies (Duration Check)

	Risk	UBI	Unemp.	Train	Adopt	Tax
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment 1						
Policy Mapping	0.320***	0.145	0.264*	0.173	0.349***	0.304**
	(0.070)	(0.136)	(0.118)	(0.110)	(0.090)	(0.114)
Co-Partisan Policy Mapping	0.218**	0.214	0.287*	0.279**	0.404***	0.422***
	(0.072)	(0.132)	(0.115)	(0.104)	(0.091)	(0.112)
Constant	2.686***	4.855***	5.036***	5.414***	3.771***	3.336***
	(0.053)	(0.099)	(0.087)	(0.081)	(0.065)	(0.082)
Observations	1378	1378	1378	1378	1378	1378
R^2	0.016	0.002	0.006	0.005	0.017	0.011
Adjusted R^2	0.015	0.001	0.004	0.004	0.015	0.009
Panel B: Experiment 2						
Automation Risk Info	0.271***	−0.095	−0.042	−0.066	0.190*	0.132
	(0.071)	(0.139)	(0.115)	(0.098)	(0.091)	(0.115)
Policy Mapping	0.187**	−0.044	0.071	0.056	0.183*	0.143
	(0.070)	(0.137)	(0.114)	(0.095)	(0.089)	(0.113)
Counter-Partisan Policy Mapping	0.247***	0.128	0.135	−0.036	0.011	0.116
	(0.070)	(0.133)	(0.111)	(0.097)	(0.091)	(0.111)
Constant	2.759***	4.975***	5.284***	5.709***	4.176***	3.455***
	(0.051)	(0.098)	(0.081)	(0.068)	(0.062)	(0.079)
Observations	1835	1835	1835	1835	1835	1835
R^2	0.010	0.002	0.002	0.001	0.004	0.001
Adjusted R^2	0.009	0.000	0.000	−0.001	0.003	−0.001

Notes: The sample excludes respondents whose durations are outliers. Robust standard errors are reported in parentheses. $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$.

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