Working Paper Series 825
(ISSN 2788-0443)

The Accountability Paradox: White-Collar Crime, State Capacity, and the Perception of Inequality

Zhasmen Gevorgyan

CERGE-EI
Prague, November 2026

The Accountability Paradox: White-Collar Crime, State Capacity, and the Perception of Inequality

CERGE-EI

Zhasmen Gevorgyan[*] [†]

November 11, 2026

Abstract

The perception and acceptance of income inequality depend fundamentally on beliefs in the fairness of the economic system. Consequently, salient events that undermine these beliefs, such as high-profile white-collar crime, can significantly alter social preferences. In this study, I use white-collar scandals to test how the disclosure of these crimes influences public perceptions of inequality. I find that the impact of these scandals is strictly conditional on baseline institutional trust. In low-trust environments, scandals serve as evidence of systemic corruption, increasing the perception that income distribution is unfair and excessive. Conversely, in high-trust contexts, the disclosure of white-collar crime is associated with a decrease in perceived inequality. I interpret this divergence as evidence that, where institutions are trusted, the prosecution of elite offenders is read as a signal of accountability and state capacity, whereas in low-trust settings scandals are read as confirmation of systemic corruption. These results suggest that white-collar scandals act as a "stress test" for state capacity, either reinforcing or undermining the perceived legitimacy of the economic order. These findings contribute to a deeper understanding of how institutional integrity shapes social beliefs and public support for redistributive policies.

Keywords: white-collar crime, perceived inequality, institutional trust.

JEL Codes: D31, D72, D83, H11, K42

[*]Affiliation: zhasmen.gevorgyan@cerge-ei.cz, CERGE-EI, a joint workplace of Charles University and the Economics Institute of the Czech Academy of Sciences, Politických vězňů 7, 111 21 Prague, Czech Republic.

[†]Acknowledgment: I am sincerely grateful to all the help provided by Sergey Slobodyan, Christian Ochsner, Ctirad Slavík, Byeongju Jeong and other faculty members of CERGE-EI.

1 Introduction

Income inequality has reached historic levels over the past four decades, yet public concern about income inequality does not always track with objective data. While the income share of the top 1% has consistently outpaced the bottom 50% since the 1970s, subjective perceptions of this gap vary wildly across regions (Mian et al., 2020, Appendix A1 and A2). According to the data from the International Social Survey Programme waves 2009 and 2019 (ISSP Research Group, 2017 and 2022), US respondents perceive less income inequality than the actual income distribution implies, whereas respondents in most EU countries perceive more inequality than the actual distribution contains. Overall, the key driving factor of this gap between actual and perceived inequality is belief about fairness: When people believe that income differences mainly reflect effort and merit, they tend to accept higher levels of inequality as legitimate (Alesina $\&$ Angeletos, 2002).

Building on the premise that fairness beliefs dictate the acceptance of inequality, I investigate how public perceptions respond to sudden shocks in the conduct of economic elites. Specifically, I examine high-profile white-collar crime scandals as salient events that directly challenge perceptions of income inequality. I propose that the public reaction to these crimes is not monolithic; instead, institutional trust serves as a critical interpretive filter. In environments characterized by low institutional trust, scandal revelations validate suspicions of systemic corruption, sharply elevating perceived inequality and grievances over fairness. Conversely, where baseline institutional trust is strong, the disclosure and prosecution of elite offenders serve as evidence of institutional integrity and effective state capacity, leading to a decrease in perceived inequality. Institutional trust thus operates as a decisive moderator, determining whether white-collar scandals undermine or reinforce the legitimacy of the economic order.

I document that perceptions of income inequality and corruption, which is one of the most severe forms of white-collar crime, are strongly correlated. Using individual-level OLS regressions from the ISSP survey, I find a robust positive association: individuals who perceive higher levels of corruption are significantly more likely to perceive higher income inequality, a relationship that is particularly pronounced among high-income and highly educated respondents. However, these correlations may be confounded by unobserved factors or endogeneity. To isolate a causal effect, I follow the "interrupted survey" framework (similar to the approach used by

Metcalfe et al. [2019] in their study on the impact of football championships on students' exam performance) and use salient white-collar crime scandals as exogenous shocks in a Regression Discontinuity Design (RDD). By exploiting the timing of interviews in the World Values Survey (Wave 7, 2017–2022) relative to the public disclosure of major, investigated crimes, I evaluate how fairness beliefs shift between the pre- and post-scandal periods.

The RDD estimates are systematically negative in regions with high baseline institutional trust and positive in regions with low baseline trust. I interpret this sign reversal through the lens of institutional trust. To capture regional variation in trust, I use the 2016 US presidential election outcomes and pre-event Eurobarometer (2016-2017) measures as proxies for institutional confidence. In the US context, this approach accounts for the sharp partisan reversal in trust following the 2016 presidential election and administration change, where political identity serves as a filter for institutional trust. In the United States and Europe, the public disclosure of a scandal leads to higher perceived income inequality specifically in regions where baseline trust in government is low. In these environments, the event reinforces the view that the economic system is unfair and driven by illegitimate gains. In contrast, in high-trust contexts, such as Finland or Republican-leaning US states (using the 2016 presidential election as a proxy for institutional confidence), the same events are associated with a decrease in perceived inequality. In these high-trust settings, scandals are interpreted as isolated incidents or as evidence of successful state capacity and government action against corruption. This context-dependent response highlights a novel perceptual channel through which white-collar crime influences attitudes toward redistribution.

The validity of these results is supported by a series of standard robustness checks. Balance tests on pre-determined covariates and density manipulation tests confirm that respondent characteristics are smooth across the scandal cutoffs. Furthermore, placebo tests using artificial announcement dates do not produce significant effects, ensuring that the findings are driven by the scandals themselves rather than seasonal trends. The results remain stable across various bandwidths (cut-offs) and model specifications, providing strong causal support for the identified perceptual shifts.

This paper contributes to the literature in three primary ways. First, I extend the research on the determinants of subjective inequality. Although objective measures like the Gini coefficient

are standard, McCall (2013) suggests that individuals' beliefs about inequality can be just as impactful on social stability. Existing studies have documented a significant "perception gap," where respondents in the US often underestimate inequality, while those in Europe overestimate it (ISSP Research Group, 2017; Norton & Ariely, 2011). I build on this by identifying white-collar crime as a specific catalyst for these perceptions. Unlike the drivers identified in previous work, such as technological change or tax policy (Mian et al., 2020), white-collar scandals provide a sudden and salient shock to fairness beliefs by exposing how elite success can be decoupled from merit.

Second, I contribute to the literature on fairness and redistributive preferences. Alesina and Angeletos (2005) established that the acceptance of inequality depends critically on whether the distribution is perceived as the result of "luck and connections" versus "hard work." Although most of the papers in this line focus on the two-way relationship between corruption and actual income levels (Khan, 2022; Dincer & Gunalp, 2012), I shift the focus to the perceptual channel. By demonstrating that high-profile fraud and corruption act as a "stress test" for the social contract, I show how these events shift the threshold of what citizens consider an "acceptable" level of inequality.

Finally, I provide causal evidence of institutional moderation. Most existing studies linking corruption and inequality are correlational or focused on developing contexts (Gyimah-Brempong, 2002; Nguyen & Nguyen, 2020). By using an "interrupted survey" RDD framework in the US and Europe, I move beyond simple associations to show that the public's reaction to a scandal is filtered through existing institutional trust. These findings offer a novel bridge between the literature on political trust and the formation of inequality perceptions, showing that the same criminal event can produce opposite social responses depending on the baseline strength of state institutions.

2 Background: White-Collar Crime and Perception of Income Inequality

2.1 White-Collar Crime

In the literature, financial crime covers at least one of the following crimes: corruption, tax evasion, fraud, forgery, bribery, property crime, electronic criminality, money laundering, cybercrime, terrorism funding, market abuse, and utilization of confidential information (Achim & Borlea, 2020). Corruption, tax evasion, embezzlement and mortgage fraud are classified as white-collar crime, which is a crime committed by a person of respectability and high social status who uses his status to gain financial benefits (Sutherland, 1940). The victims of those crimes are individuals and businesses; however, high white-collar crime rates can affect a country's macroeconomic indicators, including GDP, GDP per capita, and unemployment (Achim and Borlea, 2020). White-collar crime is rising significantly in both the US and EU countries. According to the US Department of Justice (2023), 62% of banks in the US reported an increase in white-collar crime. In the European Union, the European Union Agency for Criminal Justice Cooperation (Eurojust) handled 354 corruption cases in 2023, of which 243 were ongoing from previous years, with fraud and related economic crimes constituting the single largest category of cross-border casework and overall case volumes increasing. Further, according to the same agency, the most common type of white-collar crime in Europe is tax fraud. Based on relevant literature and statistics from the Uniform Crime Report (UCR) program and the European Union Agency for Criminal Justice Cooperation, I focus on two main types of white-collar crime, responsible for most financial losses: corruption and fraud.

2.2 Perception and Acceptance of Income Inequality

In the late 1970s, many factors, including technological change, the decline in minimum wage, and tax policies, significantly widened the income gap worldwide (Mian et al., 2020). Subsequently, income inequality, measured as the Gini coefficient, increased significantly in the United States from 1980 to 2023 (OECD, 2023). Although the initial reasons for the rise in inequality are clear, the persistence of the increase suggests that other factors affect income inequality.

McCall (2013) suggests that people's beliefs about income inequality can be as important as actual income inequality. Moreover, increasing concerns about income inequality have resulted

in growing support for spending on education and support for traditional redistributive policies (McCall & Kenworthy, 2009), meaning that the government will take action not only when income inequality is high but also when the perception of income inequality is high.

Nevertheless, public perceptions of income distribution in the United States and EU countries often diverge from reality, revealing a gap between perceived notions and actual inequality levels. The General Social Survey (GSS), International Social Survey Programme (ISSP), and World Values Survey (WVS) collect data on individuals' beliefs and perceptions about the level of income inequality in 22 countries, including the US and EU countries (Lauer & Yodanis, 2004), and are widely used in the literature. When discussing income inequality specifically, the literature employs several perception measures. For example, Pedersen and Mutz (2019) asked respondents to estimate the pay ratio between CEOs of large corporations and unskilled workers. In the United States, while the actual CEO-to-unskilled-worker pay ratio is approximately 354:1, the perceived ratio is only about 30:1 (Pedersen & Mutz, 2019), indicating substantial underestimation. These misperceptions extend to wealth inequality as well. Norton and Ariely (2011) found that survey respondents in the US believed the wealthiest 20% held approximately 59% of the wealth, significantly underestimating the actual figure of around 84%. In contrast, Faggian et al. (2023) argue that, in EU countries, actual wealth inequality is lower than the perception of wealth inequality.

Although people's perception of income inequality plays a crucial role in their preferences for redistribution, it is essential to understand whether individuals accept these differences or instead desire a more equal distribution. In the literature, there is no agreed indicator for acceptance of income inequality. However, there are measures that help to explain the acceptance of income inequality. For example, Faggian et al. (2023) consider desired income inequality in EU countries and compare it with the perception of income inequality. The results show that, on average, respondents prefer substantially lower inequality than what they perceive to exist. In this paper I address another way to measure the acceptance of income inequality that focuses on the perceived fairness of the income distribution itself.

3 Data and Measures

3.1 Data on White-Collar Crime

For the US white-collar crime data, I conducted a targeted search of press releases issued by US Attorneys' Offices to identify cases publicly announced during the 2017–2022 survey period (United States Department of Justice, 2017-2022). Similarly, for white-collar crime data in EU countries (Eurojust, 2024), I used publications from the European Union Agency for Criminal Justice Cooperation, in which all cases of crimes are reported by type, location, date of occurrence, victims, and losses. In both the US and Europe, I focused on cases that met the following criteria:

- Charges: only cases that resulted in charges or convictions were included to ensure the reliability of the data.
- High-profile cases: cases with total losses exceeding USD 1 million were selected, as these are more likely to be high-profile and to have a significant public impact.
- Sufficient observations: events were further filtered to include only those with enough pre- and post-event observations in the survey data (based on optimal bandwidth selection), ensuring a minimum number of interviews both before and after the cutoff date for credible RDD estimation.

There are multiple reasons for these selection criteria; Benson and Simpson (2024) argue that high-profile cases involving multiple fraud schemes often receive higher media coverage and generate more negative perceptions. Moreover, according to the Association of Certified Fraud Examiners (ACFE) (2022), cases exceeding USD 1 million are typically complex frauds (e.g., Ponzi schemes, large-scale corruption, or financial statement fraud) that involve significant resources and coordination. Thus, these cases are more likely to attract media attention and public scrutiny.

The median loss is significantly lower than the average loss in both the US and Europe, indicating that most white-collar crimes involve relatively smaller amounts, but several high-value cases skew the average upward. Additionally, the USD 1 million cutoff is above the 75th percentile in both regions, meaning it captures the top 25% of cases in terms of financial losses.

This aligns with the goal of focusing on high-profile cases that are more likely to influence public perceptions. Finally, in Europe, many high-value white-collar crimes involve cross-border elements, such as VAT fraud or money laundering, which can complicate investigations and increase losses. Furthermore, this data specification offers an opportunity to identify spillover effects among involved countries. Appendix B, Table B3 contains the final set of US state-level and European country-level events retained after the application of the selection filters, as well as demonstrative examples.

3.2 Data on Perceptions: Measures from Surveys

To capture the subjective dimensions of inequality and corruption, I use individual-level data from the International Social Survey Programme (ISSP) for the OLS analysis and the World Values Survey (WVS) for the RDD framework. The analysis focuses on three primary conceptual areas:

1. Perception of Income Inequality: These measures capture how individuals view the magnitude of the income gap in their respective countries. This includes both Likert-scale responses regarding the significance of income differences and selections from diagrammatic representations of social stratification.

2. Acceptance and Fairness of Inequality: Following the theoretical framework of Alesina and Angeletos (2002), this variable measures whether individuals believe success is driven by merit and hard work or luck and connections, serving as a proxy for the perceived legitimacy of the economic order.

3. Perception of Corruption: These variables capture the extent to which individuals believe bribery and illicit advantages are necessary to get ahead in the income distribution.

For consistency across the different survey waves and to facilitate the interpretation of the RDD and OLS coefficients, all measures were standardized and converted into binary variables. Specifically, responses were scaled from 0 to 1, and individuals scoring above the median were assigned a value of 1 (indicating a high perception of inequality/corruption or low acceptance of inequality), while the remainder were assigned a value of 0. Appendix C contains detailed descriptions of the survey questions, the original scales, and the specific logic for each measure. Additionally, Appendix B, Table B2 reports summary statistics for the analytic samples from the WVS.

4 Corruption and Perceived Inequality

To understand the relationship between corruption and perception of income inequality, I run OLS using individual-level survey questions. The strategy is summarized by the following formula:

$$Y_{i,c} = \beta X_i + \gamma_1 Z_i + \gamma_2 K_c + \epsilon_{i,c}, \tag{1}$$

where $Y_{i,c}$ is the perception of income inequality for individual i in country c (see Appendix C, M1), X_i is the perception of corruption proxy for individual i in country c (see Appendix C, M4), Z_i is the vector of individual controls, including age, gender, education, employment status, and income level, K_c is the vector of country-level controls, including unemployment rate, GDP per capita, and average education level, and $\epsilon_{i,c}$ is the error term. The results of the OLS are summarized in Table 1. The associated p-values are lower than 0.01 even when all control variables are added, which means that the results are statistically significant. In particular, because both the dependent variable and the corruption measure are binary, the model is a linear probability model and the coefficient admits a direct probability interpretation: moving from the lower half to the upper half of the corruption perception distribution is associated with an 8.4 percentage point higher probability of perceiving high income inequality (Column 3).

Table 1: Individual-Level OLS Results

	Dependent variable: perception of income inequality				
	US & EU	US & EU	US & EU	US	EU
	(1)	(2)	(3)	(4)	(5)
Perception of corruption	0.162***	0.134***	0.084**	0.113*	0.084**
	(0.059)	(0.051)	(0.037)	(0.062)	(0.039)
Individual controls	No	Yes	Yes	Yes	Yes
Country controls	No	No	Yes	No	Yes
Mean dependent variable	0.417	0.417	0.417	0.437	0.415
Observations	18,458	18,458	18,458	1,557	16,901
R-squared	0.007	0.038	0.053	0.038	0.065

Notes: This table reports OLS estimates of the relationship between perceived corruption and perceived income inequality. Standard errors are clustered at the country level in columns (1)–(3) (15 clusters) and (5) (14 clusters); column (4) reports HC1 standard errors (US only). Individual controls include age, education, income, sex, and work-status controls. Country controls include Gini, corruption, GDP per capita, GDP, and education spending. Data are from ISSP 2019. * p<0.1, ** p<0.05, *** p<0.01.

This relationship holds even after controlling for individual-level characteristics (e.g., age, gender, education, income) and country-level factors (e.g., GDP, unemployment rate, education). Additionally, the results are consistent across both the US and EU countries (Columns 4 and 5), indicating that the link between corruption perceptions and income inequality perceptions is robust across different contexts.

Results for further subgroup analyses by education, employment, and income are provided in Appendix D1; however, while these findings remain statistically significant across groups, the variation in coefficient magnitude is marginal. This suggests that the relationship between corruption and inequality perceptions is a pervasive phenomenon that transcends most demographic boundaries. Additionally, several alternative measures of perceived income inequality and acceptance of inequality (constructed from different questions in the ISSP) are employed as dependent variables. The corresponding results are presented in Appendix D2 and confirm that the positive relationship between perceived corruption and inequality perceptions remains highly robust across these alternative measures.

5 RDD: Empirical Strategy and Results

This section evaluates the causal impact of salient white-collar crime scandals on public perceptions of income inequality.

5.1 Methodology

I use RDD to evaluate the causal impact of high-profile fraud and corruption scandals on public perceptions of income inequality. By leveraging the precise timing of these scandals' public disclosures as exogenous shocks, I compare changes in perceptions immediately before and after the events, thus isolating the effects of scandal exposure from other confounding influences. In the US analysis, standard errors are clustered at the state level; in the European analysis, at the country level, which are the levels at which the regional trust proxy and the country-level environment vary. Individual-level clustering is reported as a robustness exercise in Appendix E. The basic model is summarized by the following equation:

$$Y_{i,t} = \alpha + \beta D_{i,t} + \gamma Z_{i,t} + \delta T_t + \varepsilon_{i,t}, \tag{2}$$

where $D_{i,t}$ is a dummy variable that equals 1 when an individual was interviewed after a white-collar scandal:

$$D_{i,t} = \begin{cases} 1, & t > \tau, \\ 0, & t \leq \tau. \end{cases}$$

Outcome $Y_{i,t}$ is the perception of income inequality described in the previous section (see Appendix C, M2), i, τ, and t denote, respectively, the individual, the cutoff point (the announcement day of the white-collar crime), and the date of the interview. $Z_{i,t}$ is the vector of individual characteristics (age, gender, education, work, income level). T_t is a vector of day-of-week dummies, included to account for possible within-week patterns in survey responses.

5.2 Identification Strategy

The causal identification of the impact of white-collar scandals relies on the "interrupted survey" framework. The core identifying assumption is that the timing of survey interviews is as good as random with respect to the timing of the scandal announcements. Specifically, I assume that, in the absence of the white-collar scandal, there would be no discontinuous jump in the perception of income inequality at the cutoff date. This ensures that any observed shift in perception can be causally attributed to the salience of the white-collar crime revelation rather than unobserved time trends or respondent self-selection. To ensure robust identification of this local average treatment effect (LATE), I identified a set of survey windows in the WVS that were interrupted by major white-collar scandals. These events were selected based on the criteria described in the data section. Specifically, the initial number of events that satisfy criteria from the data section revealed 70 high-profile US and 25 European cases. Next, regional aggregation, testing for availability of enough observations around the cutoff and the exclusion of overlapping events produced 12 US state cut-offs and 10 European country–crime-date pairs for the main RDD analysis (see Appendix B3 for more details). Following Calonico et al. (2019), Cattaneo et al. (2020), and Cattaneo et al. (2024), I assess the validity of the RDD results using the following tests:

1. Smoothness of Confounding Variables

The smoothness of confounding variables around the cutoff is tested to ensure that there are no discontinuities in the distribution of covariates at the treatment threshold. This is conducted by examining the relationship between each covariate (e.g., age, income, education, work status) and the cutoff (Cattaneo et al., 2020; Cattaneo et al., 2024). All covariates were found to be

smooth across the cutoff, satisfying the smoothness assumption required for RDD (Appendix E6 and E7). This confirms that the jump in the perception of income inequality after the cutoff is not associated with differences in the characteristics of respondents, and that individuals interviewed immediately before and after the event are otherwise comparable.

2. Manipulation Test of the Running Variable

To ensure that there is no manipulation of the running variable around the cutoff, I conduct a density test. This test checks whether the density of the running variable is continuous at the cutoff. I follow Cattaneo et al. (2020) and test for manipulation of the running variable using a robust local polynomial density estimator. The results show no evidence of a discontinuity in the density of the running variable at the cutoff in any country (all p-values > 0.1), consistent with no systematic manipulation (see Appendix E8). The absence of density jumps indicates that the timing of survey interviews is effectively as good as random with respect to the scandal announcements. Consequently, individuals interviewed immediately before and after each cutoff can be treated as comparable except for their exposure to the scandal news. This finding strongly supports the validity of the RDD strategy and increases confidence that the estimated effects reflect the genuine causal impact of white-collar crime revelations on public perceptions of income inequality.

3. Bandwidth Strategy

Bandwidth choice differs across regions because the data structure differs. In the US, interview density is relatively high, but event timing is more complex: in several cases, multiple scandals occur in close succession within the same broad region. To handle this overlap while preserving enough observations around each cutoff, the main US specification uses the event-specific optimal bandwidth selected. This approach is designed to balance two constraints simultaneously: avoiding contamination from nearby events and maintaining a sufficient local sample size for credible inference.

For Europe, interview timing is more regular across countries, so the baseline specification uses a symmetric 7-day window to capture immediate responses. A 14-day window is then reported to assess the persistence of the effect. When the 7-day specification does not provide enough local observations, the estimate is reported using the optimal bandwidth instead (labeled Optm.). This rule keeps the Europe estimates comparable while still ensuring estimability in low-density country-event windows.

5.3 Results for the US

The US analysis is grouped by event. I begin with a pooled specification that stacks all twelve US event windows, aligning each scandal's public-disclosure date at day 0. Table 2.1 reports the event-stacked estimates: the average effect of scandal disclosure on perceived income inequality is small and statistically insignificant, both without (-0.073) and with (-0.082) event fixed effects. Consequently, there is a substantial heterogeneity across regions, which the remainder of this section documents.

Table 2.1: Pooled RDD Results for the United States (Event-Stacked)

Specification	RDD coefficient	R-squared	Events	Observations
No event fixed effects	-0.073	0.015	12	22,009
	(0.059)			
With event fixed effects	-0.082	0.015	12	22,009
	(0.065)			

Notes: This table reports pooled RDD estimates from the event-stacked US sample: each of the twelve event windows is aligned at day 0 (the public-disclosure date) and all windows are combined in a single regression. The outcome is the standardized binary measure of perceived income inequality. All models include individual controls and day-of-week fixed effects. Standard errors, clustered at the state level (48 states), are in parentheses. Data are from WVS wave 7 (2017–2022). * p<0.10, ** p<0.05, *** p<0.01.

For the regional analysis, the sample is grouped by scandal-disclosure event. For each event, I first estimate the RDD at the Census-region level (nine-region classification), merging adjacent regions when the same event date applies (for example, East and West South Central). I then estimate event-level RDDs at the state level to assess whether effects spill over within the corresponding region and to evaluate the geographic concentration of the response. Consistent with the bandwidth strategy described above, Table 2.2 reports the main US results using event-specific optimal bandwidth selection.

The estimates reveal substantial regional heterogeneity. Several regions show negative and statistically significant coefficients, including East North Central (-0.26), West North Central (-0.42), and the combined Missouri–Michigan sample (-0.45). By contrast, Middle Atlantic and Pacific display positive and statistically significant effects (0.63 and 0.53), and Pacific+Mountain shows the largest positive estimate (0.73). South Atlantic and several state-level estimates are imprecisely estimated, underscoring that both sign and precision vary across local contexts.

Table 2.2: RDD Results for the United States

Region/state	RDD coefficient	SE	R-squared	Observations
East North Central	-0.26*	0.140	0.02	299
West North Central	-0.42**	0.210	0.04	227
States (MI+MO)	-0.45**	0.199	0.09	155
Middle Atlantic	0.63***	0.155	0.02	164
States (NY+PA)	-0.51**	0.248	0.05	151
Pacific	0.53***	0.164	0.05	229
Pacific+Mountain	0.73***	0.241	0.09	62
State (CA)	0.53***	0.205	0.04	174
East & West South Central	-0.30*	0.161	0.03	300
States (TX+LA+AL)	-0.30	0.227	0.02	183
South Atlantic	0.44	0.284	0.03	332
State (FL)	-0.27	0.277	0.05	132

Notes: This table reports local treatment effects from the RDD specification, where treatment equals being interviewed after scandal disclosure. All models include individual controls and day-of-week fixed effects. Data are from WVS wave 7 (2017–2022). * p<0.10, ** p<0.05, *** p<0.01.

Robustness exercises that employ alternative bandwidths, continuous rather than binary outcome measures, and additional survey items measuring fairness beliefs yield qualitatively identical results (Appendixes E2–E4).

5.4 Results for Europe

For European countries, estimation begins with a pooled event-stacked specification that aligns all 25 country-event windows at the disclosure date (day 0). The average effect is small and statistically insignificant, confirming that the informative variation is cross-country rather than continental (See table 3.1).

Table 3.1: Pooled RDD Results for Europe (Event-Stacked)

Specification	RDD coefficient	R-squared	Events	Observations
No event fixed effects	-0.054	0.009	25	25,496
	(0.059)			
With event fixed effects	-0.050	0.011	25	25,496
	(0.052)			

Notes: This table reports pooled RDD estimates from the event-stacked European sample: each of the 25 country-event windows is aligned at day 0 (the public-disclosure date) and all windows are combined in a single regression. The outcome is the standardized binary measure of perceived income inequality. All models include individual controls and day-of-week fixed effects. Standard errors, clustered at the country level (16 countries), are in parentheses. Data are from WVS wave 7 (2017–2022). * p<0.10, ** p<0.05, *** p<0.01.

Attention therefore turns to the country-level estimates. The preferred local window is the symmetric 7-day, which captures the immediate response to scandal disclosure. The 14-day window is reported to evaluate short-run persistence. In country-event cases with insufficient

local observations under the 7-day window, the primary estimate is reported with the optimal bandwidth (Optm.), following the same selection procedure described above. Table 3.2 presents these estimates.

In the majority of countries the seven-day coefficients are positive and statistically significant, indicating an immediate rise in the probability of perceiving high income inequality. The magnitudes are economically large: Estonia (0.55), France (0.79–0.87), Germany (0.84), and the three Italian episodes (0.72–1.17). In most cases, the effect attenuates sharply within fourteen days and becomes statistically indistinguishable from zero. Finland constitutes a clear exception: both the optimal-bandwidth and fourteen-day estimates are negative and significant (-0.35 and -0.46). A small number of countries (Netherlands, Romania, Sweden) yield coefficients that are either small or statistically insignificant under the preferred bandwidths.

Table 3.2: RDD Results for Europe

Country	Cutoff	RDD coefficient	R-squared	Observations
Estonia	7 day	0.55^{***}	0.01	494
	14 day	0.04	0.01	951
Finland	Optm.	-0.34^{**}	0.11	126
	14 day	-0.45^{**}	0.16	105
France 1st	7 day	0.87^{***}	0.03	328
	14 day	0.02	0.01	478
France 2nd	7 day	0.80^{**}	0.08	240
	14 day	0.07	0.02	529
Germany (EVS)	7 day	0.84^{**}	0.05	149
	14 day	0.03	0.03	454
Italy 1st	7 day	0.94^{***}	0.03	305
	14 day	0.11	0.01	504
Italy 2nd	7 day	0.72^{**}	0.08	317
	14 day	0.08	0.02	630
Italy 3rd	7 day	1.16^{***}	0.07	347
	14 day	0.19^{**}	0.02	637
Netherlands (EVS)	Optm.	0.23	0.11	115
	14 day	0.49^{*}	0.13	83
Romania	Optm.	0.24^{*}	0.05	271
	14 day	0.13	0.07	203
Sweden	Optm.	0.17	0.09	125
	14 day	0.31	0.12	75

Notes: This table reports Europe RDD estimates under the bandwidth choices shown in the second column (7-day, 14-day, or optimal where indicated). Treatment equals interview timing after scandal disclosure. All models include individual controls and day-of-week fixed effects. Data are from WVS wave 7 (2017–2022). * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Alternative bandwidths, continuous outcome specifications, and fairness-related survey items produce results of the same qualitative pattern (Appendix E5).

5.5 Additional Robustness Checks

In addition to the main tests described in Section 5.2, I also address several robustness checks. All tests provide strong support for the identification assumptions.

1. Placebo Tests

To further validate the causal interpretation of the RDD estimates, I conduct a series of placebo (or falsification) tests. These tests artificially shift the cutoff date to non-event periods while keeping the same estimation window and model specification. Specifically, I re-estimate the model using fake cutoffs at ± 3, ± 5, ± 7, and ± 10 days relative to the actual scandal announcement dates. The results are highly reassuring: the estimated treatment effects at all placebo cutoffs are small, statistically insignificant, and often close to zero across countries and US regions. In sharp contrast, the effect remains large and highly significant when using the true announcement date as the cutoff (Appendix E9). This clear pattern indicates that the main findings are not driven by unobserved time trends, seasonal patterns, or other confounding events around the scandal dates.

2. Standardized Mean Difference Test

In addition to the standard RDD tests, I also test the standardized mean difference (SMD) to assess the balance of confounding variables before and after the treatment cutoff:

$$SMD = \frac{\hat{X}_t - \hat{X}_c}{\sqrt{\frac{s_t^2 + s_c^2}{2}}},$$

where $\hat{X}_t$ and $\hat{X}_c$ are the means and s_t^2 and s_c^2 are the variances of the variable in the treatment and control groups, respectively. The results show excellent balance: all SMD values are well below the conventional threshold of 0.25, and the vast majority fall below 0.10 across specifications and regions (Appendix E10 and E11). Following Austin (2011), such low values indicate that the treatment and control groups are highly comparable on observables. This strong balance further supports the validity of the RDD strategy and reinforces the conclusion that any observed discontinuity in perceptions of income inequality at the cutoff can be causally attributed to the public revelation of the white-collar crime scandal.

3. Bandwidth Sensitivity

A common concern in regression discontinuity designs is the sensitivity of the results to the choice of estimation window around the cutoff. In the main specification, I use a symmetric

$\pm$7-day window around each scandal announcement date. To ensure robustness, I also present results using the optimal bandwidth selected by the Calonico procedure (Calonico et al., 2014, 2020), alongside narrower (± 5 day) and wider (± 14 day) windows. The estimated treatment effects remain qualitatively and quantitatively stable across these different bandwidth choices. The coefficients keep the same sign, maintain statistical significance, and are of similar magnitude whether using the conventional $\pm$7-day window or the data-driven optimal bandwidth (see Appendix E4 and E5). This consistency confirms that the main findings are not driven by an arbitrary choice of bandwidth and strengthens confidence in the causal impact of white-collar crime scandals on perceptions of income inequality.

4. Additional Robustness Checks

I include day-of-week dummies in the regression models to control for potential day-specific patterns in survey responses. A potential concern with the sharp RDD is the presence of multiple white-collar crime revelations within the estimation window, which was particularly relevant in some US regions. To address this, states with nearly overlapping events were excluded. Additionally, I exploit the richer geographic variation in the US data by estimating state-level RDDs. Reassuringly, the coefficients at the state level are systematically larger in magnitude than those obtained at the regional level, consistent with attenuation bias from aggregation.

Two scope conditions are important for interpretation. First, the RDD estimates are local to respondents interviewed close to scandal-disclosure cutoffs, so they identify short-run belief updating rather than long-run preference formation. Second, the outcome is perceived inequality, not realized inequality; the mechanism therefore operates through information and institutional interpretation. These boundaries do not weaken the main contribution, but they define where external validity is strongest: high-salience white-collar disclosures observed in comparable survey-interview settings.

6 Mechanism

The results in Section 5 *establish* that the response to white-collar scandals differs sharply across regions. This section asks *why*, and provides my interpretation of the sign reversal documented above, namely that the same scandal decreases perceived inequality where institutional trust is high and increases it where trust is low.

6.1 The United States: 2016 Presidential Election as a Proxy for Institutional Trust

For the United States, states are classified according to the outcome of the 2016 presidential election. This classification is motivated by a well-established pattern in the political science literature: Americans' trust in government is systematically higher when their preferred party controls the White House; a phenomenon known as the "president-in-power" effect (Citrin, 1974; Keele, 2005; Hetherington & Rudolph, 2015). The 2016 election produced a particularly sharp and well-documented partisan reversal in institutional trust. In particular, between October 2015 and April 2017, the share of Republicans expressing trust in the federal government rose by 17 percentage points, while trust among Democrats fell to a near-historic low; for the first time since the George W. Bush administration, Republicans reported higher trust in government than Democrats (28% versus 15%; Pew Research Center, 2017). Analyses of American National Election Studies (ANES) and General Social Survey (GSS) data spanning five decades further document an asymmetrical president-in-power effect, with conservatives and Republicans exhibiting larger trust gains under a co-partisan administration (Morisi, Jost & Singh, 2019). Because the white-collar crime disclosures in the sample occurred in 2017–2018, the 2016 election outcome provides a pre-determined proxy for regional differences in institutional confidence precisely at the time of the scandals. I therefore interpret Republican-leaning areas as relatively high-trust and Democratic-leaning areas as relatively low-trust environments. This proxy is admittedly indirect: electoral outcomes are not a direct measure of institutional trust. The European analysis below, which relies on direct survey measures of trust in national government, therefore serves as an important cross-validation of the interpretation.

Figure 1 illustrates the resulting pattern: in Republican-leaning states the RDD coefficients on perceived income inequality are systematically negative, whereas in Democratic-leaning states the coefficients are positive. I interpret this conditional response as follows. Where institutional trust is higher (Republican-leaning areas after the 2016 election), the public disclosure and

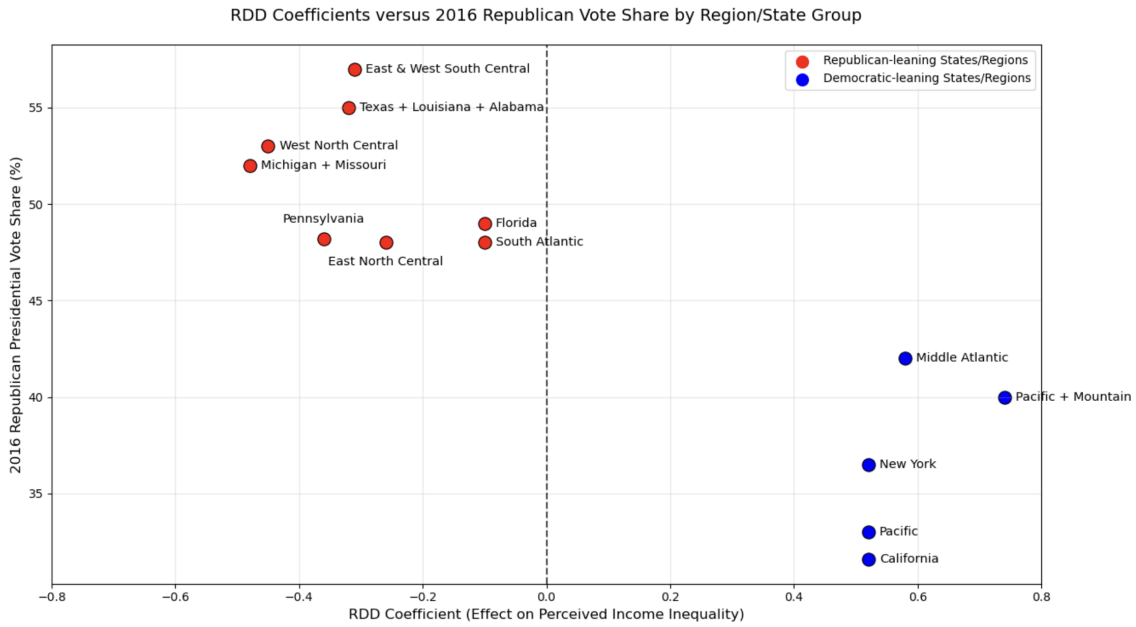


Figure 1: RDD Coefficients and Political Leaning by US Region/State

Notes: Political leaning is based on the 2016 US presidential election outcome. A region is classified as Republican-leaning when most states in that region voted Republican. Points report estimated RDD coefficients for perceived income inequality.

prosecution of elite offenders appear to signal successful enforcement of the rules, thereby reducing perceptions of systemic unfairness. Where trust is lower, the identical events are more readily interpreted as evidence of entrenched corruption, elevating perceptions of excessive and illegitimate inequality.

6.2 Europe: Pre-event Trust in National Government

For Europe, Figure 2 plots each country's RDD coefficient against pre-event trust in national government, measured by the average of the three Eurobarometer (2016–2017) waves immediately preceding the relevant scandal. Finland records one of the highest and most stable levels of governmental trust in Europe and is the only country that exhibits a clear negative response. Countries with lower pre-event trust display positive coefficients. This conditional relationship mirrors the US findings and supports the interpretation that high-profile white-collar scandals function as a stress test of state capacity: where institutions are already perceived as accountable, the prosecution of elite offenders reinforces legitimacy and reduces perceived unfairness; where trust is low, the same events are read as confirmation of systemic corruption and elevate perceptions of excessive inequality.

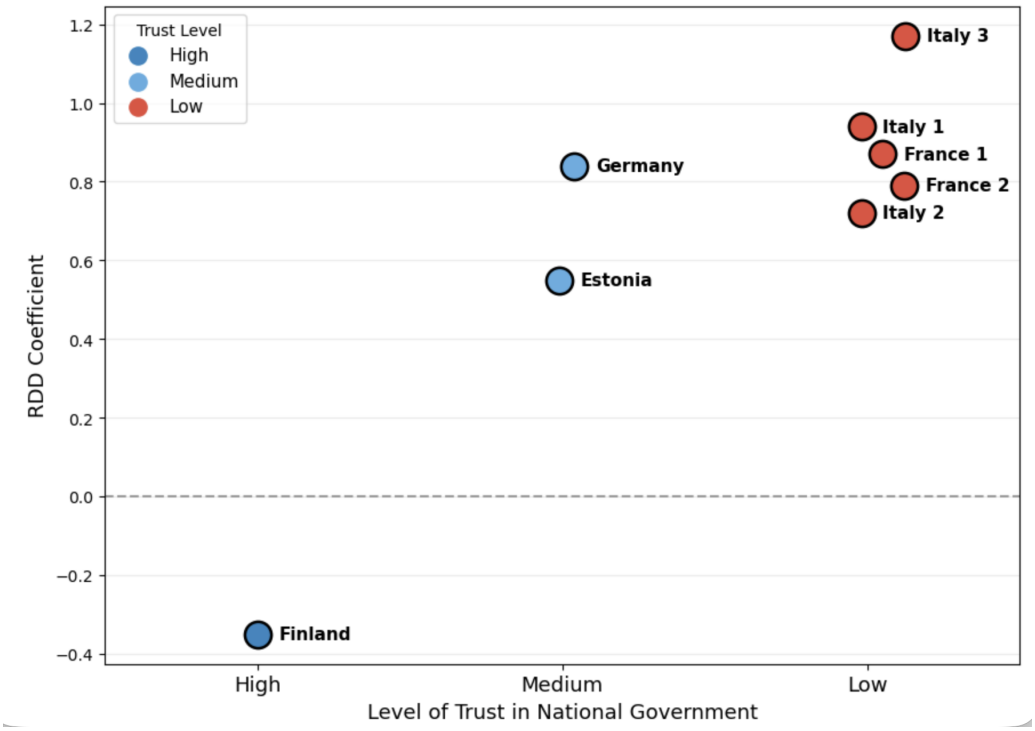


Figure 2: RDD Coefficients and Trust in Government Across Europe

Notes: Trust categories are based on Eurobarometer (2016–2017) responses to trust in national government. Countries are grouped as high trust (above 55%), medium trust (33% to 55%), and low trust (below 33%). Points report estimated RDD coefficients for perceived income inequality.

The interpretation advanced here is subject to two scope conditions. First, the trust coefficient patterns in Figures 1 and 2 are descriptive in nature: with a limited number of events, a cross-region regression of RDD coefficients on trust would be underpowered, and the evidence is therefore best read as a coherent pattern replicated across two independent settings rather than as a formal test. Second, the US proxy is indirect; the convergence with the European evidence, which rests on a direct survey measure of trust, is thus central to the credibility of the interpretation.

7 Conclusion

In this study, I use high-profile white-collar crime scandals as salient events to examine how the disclosure of those events influences public perceptions of income inequality. The results show that the impact of these scandals is strictly conditional on baseline institutional trust. In low-trust environments, scandals serve as evidence of systemic corruption, increasing the perception that the income distribution is unfair and excessive. Conversely, in high-trust contexts, the disclosure of white-collar crime is associated with a decrease in perceived inequality, as the prosecution of elite offenders signals the accountability and strength of state institutions.

Using an interrupted-survey regression discontinuity design that exploits the timing of interviews in the World Values Survey relative to scandal announcements, I document this pattern in both the United States and Europe. In the US, regional variation is captured by 2016 presidential election outcomes as a proxy for institutional confidence; in Europe, pre-event Eurobarometer (2016–2017) measures of trust in national government serve the same role. The validity of the findings is supported by balance tests, density manipulation tests, and placebo cutoffs.

The results suggest that white-collar scandals act as a "stress test" for state capacity, either reinforcing or undermining the perceived legitimacy of the economic order. By identifying this context-dependent perceptual channel, this study contributes to a deeper understanding of how institutional integrity shapes social beliefs and public support for redistributive policies.

References

[1] Achim, M. V., & Borlea, S. N. (2020). *Economic and financial crime*. Springer International Publishing.

[2] Alesina, A., & Angeletos, G. M. (2005). Fairness and redistribution. *American Economic Review*, 95(4), 960–980.

[3] Alesina, A., & Angeletos, M. (2002). Fairness and redistribution: US versus Europe. *Harvard Institute of Economic Research, Harvard University*.

[4] Association of Certified Fraud Examiners. (2022). *Report to the nations: 2022 global study on occupational fraud and abuse*. https://www.acfe.com

[5] Austin, P. C. (2011). An introduction to propensity score methods for reducing the effects of confounding in observational studies. *Multivariate Behavioral Research*, 46(3), 399–424.

[6] Benson, M. L., Simpson, S. S., Rorie, M., & Kennedy, J. P. (2024). *White-collar crime: An opportunity perspective*. Routledge.

[7] Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. Econometrica, 82(6), 2295–2326.

[8] Calonico, S., Cattaneo, M. D., Farrell, M. H., & Titiunik, R. (2019). Regression discontinuity designs using covariates. *Review of Economics and Statistics*, 101(3), 442–451.

[9] Cattaneo, M. D., Jansson, M., & Ma, X. (2020). Simple local polynomial density estimators. *Journal of the American Statistical Association*, 115(531), 1449–1455.

[10] Cattaneo, M. D., Idrobo, N., & Titiunik, R. (2024). A practical introduction to regression discontinuity designs: Extensions. *Cambridge University Press*.

[11] Citrin, J. (1974). Comment: The political relevance of trust in government. *American Political Science Review*, 68(3), 973–988.

[12] Dincer, O. C., & Gunalp, B. (2012). Corruption and income inequality in the United States. *Contemporary Economic Policy*, 30(2), 283–292.

[13] European Union Agency for Criminal Justice Cooperation. (2024). *Crime types and cases*. https://www.eurojust.europa.eu/crime-types-and-cases/key-documents

[14] Faggian, A., Michelangeli, A., & Tkach, K. (2023). Income inequality in Europe: reality, perceptions, and hopes. *Research in Globalization*, 6, 100118.

[15] Gyimah-Brempong, K. (2002). Corruption, economic growth, and income inequality in Africa. *Economics of Governance*, 3, 183–209.

[16] Hetherington, M. J., & Rudolph, T. J. (2015). *Why Washington won't work: Polarization, political trust, and the governing crisis*. University of Chicago Press.

[17] Inglehart, R., Haerpfer, C., Moreno, A., Welzel, C., Kizilova, K., Diez-Medrano, J., Lagos, M., Norris, P., Ponarin, E., & Puranen, B. (Eds.). (2022). *World Values Survey: All Rounds - Country-Pooled Datafile* (Version 3.0.0). JD Systems Institute & WVSA Secretariat.

[18] ISSP Research Group. (2022). *International Social Survey Programme: Social Inequality V - ISSP 2019* (ZA7600 Data file Version 3.0.0). GESIS. https://doi.org/10.4232/1.14009

[19] ISSP Research Group. (2017). *International Social Survey Programme: Social Inequality IV - ISSP 2009* (ZA5400 Data file Version 4.0.0). GESIS. https://doi.org/10.4232/1.12777

[20] Khan, S. (2022). Investigating the effect of income inequality on corruption: new evidence from 23 emerging countries. *Journal of the Knowledge Economy*, 13(3), 2100–2126.

[21] Keele, L. (2005). The authorities really do matter: Party control and trust in government. *The Journal of Politics*, 67(3), 873–886.

[22] Lauer, S. R., & Yodanis, C. L. (2004). The international social survey programme (ISSP): a tool for teaching with an international perspective. *Teaching Sociology*, 32(3), 304–313.

[23] McCall, L. (2013). *The undeserving rich: American beliefs about inequality, opportunity, and redistribution*. Cambridge University Press.

[24] McCall, L., & Kenworthy, L. (2009). Americans' social policy preferences in the era of rising inequality. *Perspectives on Politics*, 7(3), 459–484.

[25] Metcalfe, R., Burgess, S., & Proud, S. (2019). Students' effort and educational achievement: Using the timing of the World Cup to vary the value of leisure. Journal of Public Economics, 172, 111–126. https://doi.org/10.1016/j.jpubeco.2018.12.006

[26] Mian, A. R., Straub, L., & Sufi, A. (2020). The saving glut of the rich (No. w26941). *National Bureau of Economic Research*.

[27] Morisi, D., Jost, J. T., & Singh, V. (2019). An asymmetrical "president-in-power" effect. *American Political Science Review*, 113(2), 614–620.

[28] Norton, M. I., & Ariely, D. (2011). Building a better America—One wealth quintile at a time. *Perspectives on Psychological Science*, 6(1), 9–12.

[29] Nguyen, H. T., Vo, T. H. N., Le, D. D. M., & Nguyen, V. T. (2020). Fiscal decentralization, corruption, and income inequality: evidence from Vietnam. *Journal of Asian Finance, Economics and Business*, 7(11), 529–540.

[30] OECD. (2023). *Income distribution*. OECD Social and Welfare Statistics. https://doi.org/10.1787/data-00654-en

[31] Pedersen, R. T., & Mutz, D. C. (2019). Attitudes toward economic inequality: The role of perceived control and belief in a just world. Social Psychology Quarterly, 82(2), 154–174.

[32] Pew Research Center. (2017, May 3). Public trust in government remains near historic lows as partisan attitudes shift. https://www.pewresearch.org/politics/2017/05/03/public-trust-in-government-remains-near-historic-lows-as-partisan-attitudes-shift/

[33] Sutherland, E. H. (1940). White-collar criminality. *American Sociological Review*, 5(1), 1–12.

[34] United States Department of Justice. (2017-2022). *All USAO press releases*. https://www.justice.gov/index.php/usao/pressreleases

[35] United States Department of Justice. (2023). Annual statistical report: Fiscal year 2023. https://www.justice.gov/usao/media/1343726/dl

[36] Xu, P., & Garand, J. C. (2010). Economic context and Americans' perceptions of income inequality. Social Science Quarterly, 91(5), 1220–1241.

Appendix

A. Income Inequality

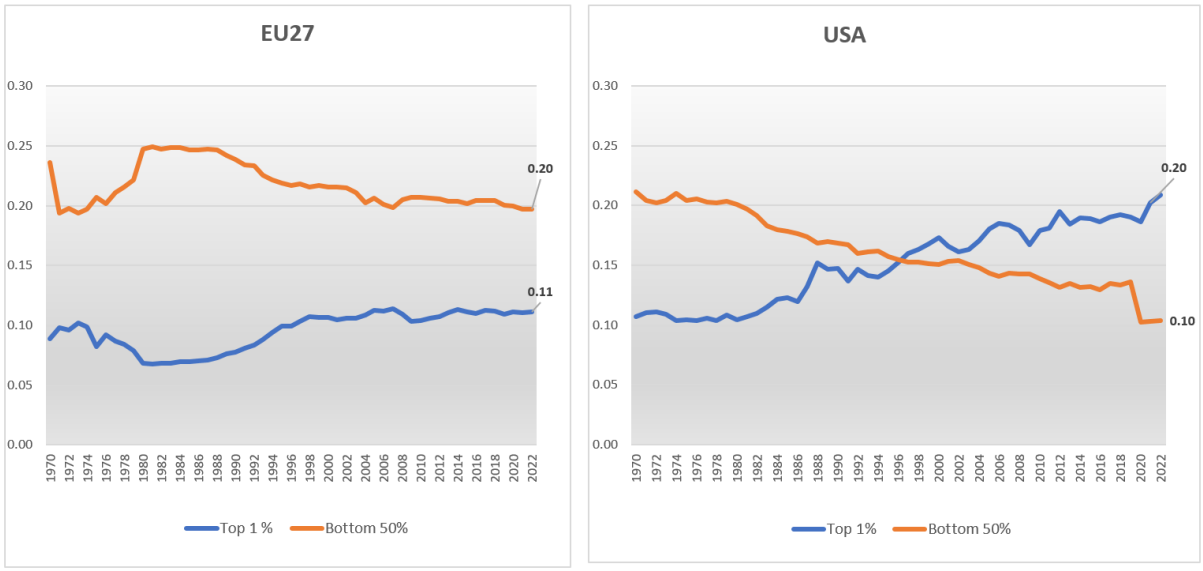


Figure A.1. Top 1% share and Bottom 50% share in the income distribution in the US and EU countries during the 1970-2022 period.

Source: World Inequality database.

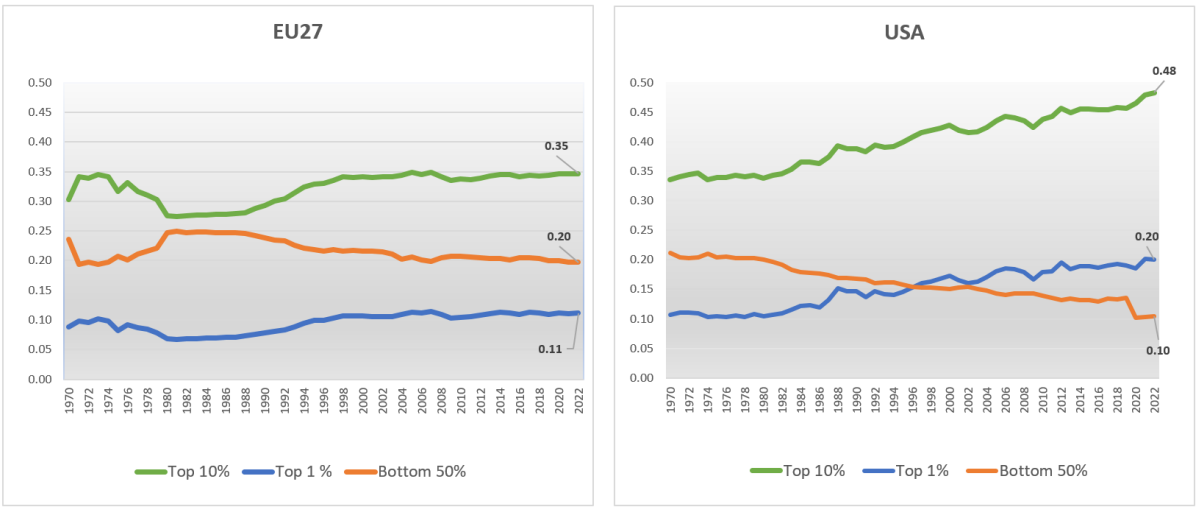


Figure A.2. Top 1% share, Top 10% share, Bottom 50% share in the income distribution in the US and EU countries during the 1970-2022 period.

Source: World Inequality database.

B.1: ISSP Survey Statistics

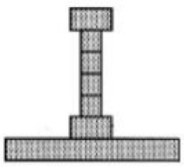
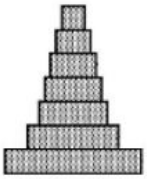
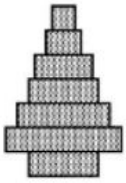
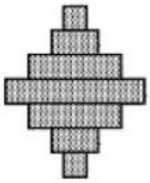
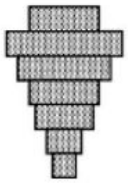
				
Type A	**Type B**	**Type C**	**Type D**	**Type E**
A small elite at the top, very few people in the middle and the great mass of people at the bottom.	A society like a pyramid with a small elite at the top, more people in the middle, and most at the bottom.	A pyramid except that just a few people are at the bottom.	A society with most people in the middle.	Many people near the top, and only a few near the bottom.

Figure B.1.1 Options for the question of different types of society.

Source: ISSP survey questionnaire, p. 25, 2009, 2019.

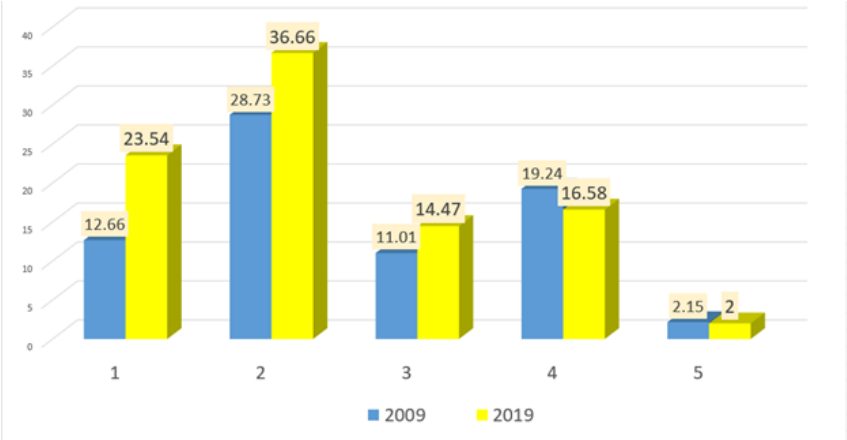


Figure B.1.2. Percentages of different types of society diagrams selected in the US, 2009 and 2019 among the types in Figure 2.1.

Source: ISSP survey data, 2009, 2019.

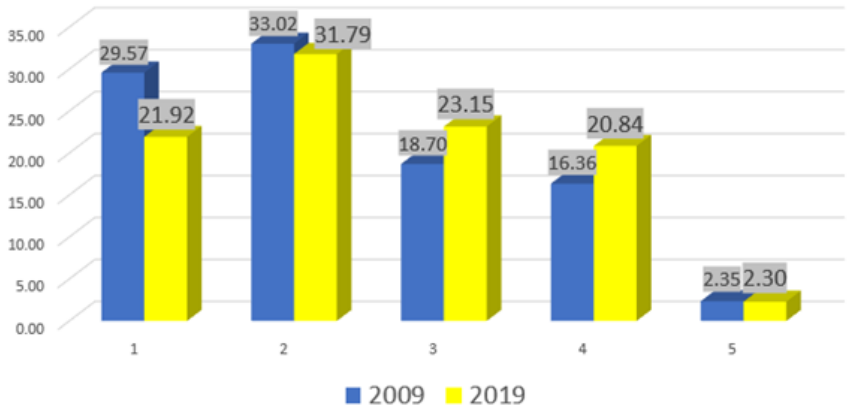


Figure B.1.3 Percentages of different types of society diagrams selected in EU countries averaged, 2009 and 2019 among the types in Figure 2.1.

Source: ISSP survey data, 2009, 2019.

B2: WV Survey, Descriptive Statistics

Table B2 reports summary statistics for the pooled analytic samples from the WV survey used in the RDD analysis. The US sample comprises 2,596 respondents interviewed during the relevant fieldwork windows, while the European sample contains 39,277 respondents. The average respondent in the United States is 43.4 years old (SD = 16.3) and 53.5 percent female. The corresponding figures for Europe are 50.9 years (SD = 17.7) and 45.2 percent female. Mean educational attainment and income categories are reported on the original survey scales. The main outcome variable (Perceived income inequality (II)) exhibits substantial variation in both samples. The number of complete cases (observations with non-missing values on all covariates used in the main specifications) remains high (2,216 in the United States and 37,969 in Europe), indicating limited item non-response on the key variables.

Table B2: Demographic Composition by Region

	United States	Europe
Number of respondents	2,596	39,277
Mean age (years)	43.42	50.92
SD age	16.31	17.65
Female share (%)	53.5	45.2
Mean education	0.51	3.82
Mean income category	1.88	1.65
Mean II	0.697	5.281
SD II	0.459	2.734
Complete-case sample	2,216	37,969

Notes: II denotes the survey measure of perceived income inequality. Education and income are measured on the original survey scales and are not directly comparable across regions. Complete-case samples exclude observations with missing values on the outcome or any covariate entering the main RDD specifications (Sex, Age, Work status, Education, Income).

B3: White-Collar Crime Events

To illustrate the nature of the events retained after the selection filters, three representative cases are described below.

In the United States, on 2 May 2017 a New York stock promoter was convicted in a $131 million market-manipulation scheme. In the same week a California CPA was sentenced for embezzling $7.2 million from high-profile clients, including the singer Alanis Morissette.

In Europe, the 11 April 2018 "Carton Rouge" operation dismantled a multi-country fraud ring that impersonated football agents and sold fake diamond and Bitcoin investments, defrauding approximately 50 victims of €2 million. A second European example is the 11 October 2018 Romanian-led VAT carousel involving second-hand vehicle sales that generated more than €20 million in unpaid tax; coordinated searches across seven Member States led to the freezing of €18 million in assets.

These cases exemplify the high-value, publicly salient fraud and related financial crimes that constitute the empirical sample. The list of final events used in the paper is illustrated below in tables B3.1 and B3.2.

Table B3.1: Retained White-Collar Crime Events: United States

Region / State	Date	Crime description (total losses in $)
East North Central	2017-05-02	Securities / investment fraud ($1.77M)
West North Central	2017-05-04	Embezzlement ($1.5M)
States (MI+MO)	2017-05-02	Fraud / embezzlement ($1.5–1.77M)
Middle Atlantic	2017-05-02/04	Securities fraud ($2.5–131M)
States (NY+PA)	2017-05-03/04	Securities fraud ($2.5–131M)
Pacific	2017-05-02/03	Healthcare fraud / embezzlement ($6–7.2M)
Pacific+Mountain	2017-05-09/11	Investment fraud ($1–2M)
State (CA)	2017-05-02/03	Healthcare fraud / embezzlement ($6–7.2M)
East & West South Central	2017-05-02/04	Fraud / bribery ($1.1–5M)
States (TX+LA+AL)	2017-05-02/04	Fraud / bribery ($1.1–5M)
South Atlantic	2017-05-02/05	Healthcare / securities fraud ($1.1–90M)
State (FL)	2017-05-02/05	Healthcare / securities fraud ($1.1–90M)

Notes: The table lists the 12 US region- and state-level windows retained after the application of the charge, loss-size, observation-window and non-overlap filters. Dates refer to the public disclosure used as the RDD cutoff. Loss figures are taken from official DOJ announcements.

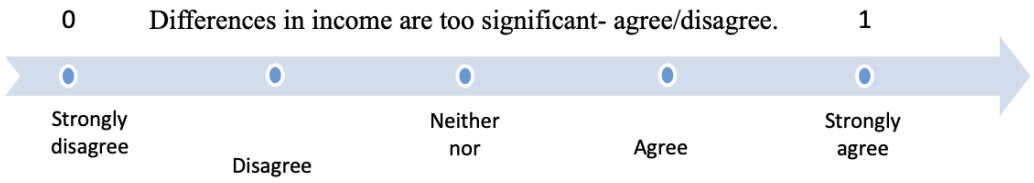
Table B3.2: Retained White-Collar Crime Events: Europe

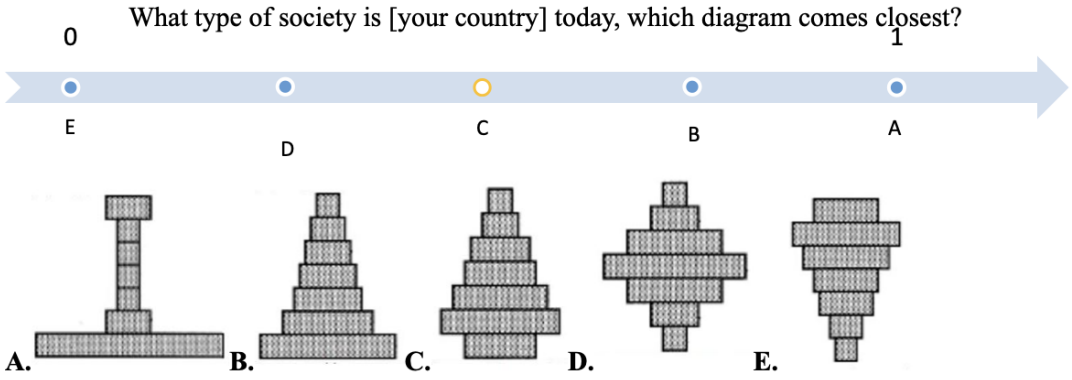
Country	Date	Crime description (total losses in €)
Estonia	2018-06-01	Online fraud & money laundering (18M+)
Finland	2018-03-08	Money laundering (2.8M)
France (1st)	2018-04-11	Multiple fraud (2M)
France (2nd)	2018-04-17	Organised property crime / fraud (millions)
Germany (EVS)	2018-01-09	Organised crime / money laundering (large-scale)
Italy (1st)	2018-10-16	Counterfeiting (large-scale)
Italy (2nd)	2018-11-14	Illegal betting / money laundering (80M seized)
Italy (3rd)	2018-12-05	Money laundering (2M+ seized)
Netherlands (EVS)	2018-01-30	Money laundering
Romania	2018-10-11	Large-scale VAT fraud (20M+)
Sweden	2018-03-08	Money laundering (2.8M)

Notes: The table lists the European country-events retained after the application of the charge, loss-size, observation-window and non-overlap filters. Dates refer to the public disclosure used as the RDD cutoff. Loss figures are taken from official Eurojust announcements.

C. Measures of Perceptions

C.1: Measure 1, Perception of Income inequality M1.





For data about the perception of income inequality in OLS analyses, I use the ISSP survey for the US and EU on Social Inequality, conducted in 1987, 1992, 1999, 2009, and 2019 (ISSP, 2019, questionnaire, p. 12, p.25). Specifically, I use answers to the following questions from the questionnaire:

1. "To what extent do you agree or disagree with the following statements? Differences in income in [your country] are too significant: strongly agree, agree, neither agree nor disagree, disagree, strongly disagree, cannot choose, and do not know."

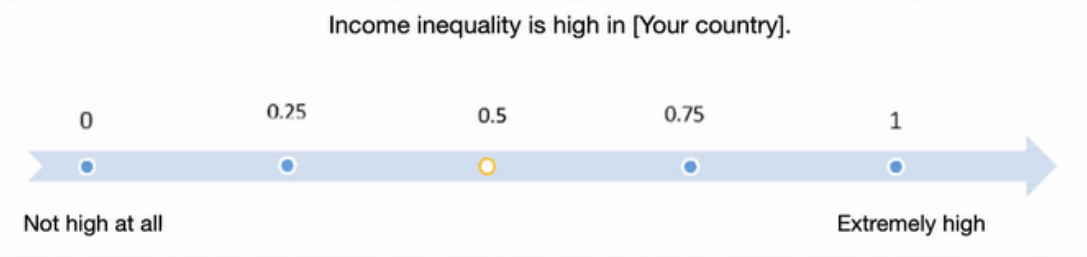
2. "These five diagrams show different types of society. Please read the descriptions, look at the diagrams, and decide which you think best describes [your country] (See appendix B, Figure B.1): What type of society is [your country] today, which diagram comes closest?"

In Appendix B, Figure B.1.1, options for the answer to this question go from the most unequal scenario (small elite and many people at the bottom) to the most equal (many people near the top). Figure B.1.2 and B.1.3 in Appendix B show how answers differ in 2019 from 2009 for US and EU. Specifically, in 2019, most responders (60.2%) chose Type A and Type B (the most unequal types); which is almost 20% point higher than in 2009 (41.39%), indicating that responders are more pessimistic in 2019.

Following Xu and Garand (2010), I created measures from the two questions above like the Gini coefficient, which ranges from 0-1, where 0 means that they "Strongly disagree" that income inequality is significant, while 1 means that they "Strongly agree" that income inequality is high. Similarly, for the second question, 0 means they chose the most equal distribution, and one means they chose the most unequal distribution. Next, I create a binary variable, where individuals who were above median in at least one of these two measures have value 1, and the rest 0. This means that respondents who select Type A or Type B, or respondents who strongly agree or agree that

differences are high, are above median and have 1 indicator, while the rest have 0.

C.2: Measure 2, Perception of Income inequality M2.



For RDD analyses, I use data from the World Value Survey, which includes data for the US and EU countries from 1981 to 2022. Specifically, I create measure of perception of income inequality from the following question (WV wave 7, 2017-2022, questionnaire page 8., question 106):

"How would you place your views on this scale? 1 means you agree completely with the statement on the left (Incomes should be made more equal); 10 means you agree completely with the statement on the right (There should be greater incentives for individual effort); and if your views fall somewhere in between, you can choose any number in between." Like the first measure of income inequality, responses are scaled from 0 to 1, where 0 indicates strong disagreement that incomes should be more equal (low perception of income inequality) and 1 indicates strong agreement that incomes should be made more equal (high perception of income inequality). In the RDD, a binary measure was used, meaning that all responders above median are 1 and the rest are 0. All respondents that were not sure (Responders who chose option "I am not sure") about their response were excluded from the sample for more accurate results.

Measure 3. Fairness of income inequality (M3)

Next, the measure of acceptance of income inequality (fairness of income inequality) is derived from the following WVS question: (WV wave 7, 2017-2022, questionnaire page 8., question 110): "How would you place your views on this scale? 1 means you agree completely with the statement on the left (In the long run, hard work usually brings a better life); 10 means you agree completely with the statement on the right (Hard work doesn't generally bring success—it's more a matter of luck and connections); and if your views fall somewhere in between, you can choose any number in between." The measure is scaling from 0 to 1, where 0 means the reason for success is hard work, while 1 means the reason is luck and connections.

C.3: Measure 4, Perception of Corruption M4. In OLS analyses, I created a measure of the perception of corruption based on the following question (ISSP, 2019, questionnaire, p. 15): "Do you think giving bribes will help you to go ahead in the income distribution?" Variable ranges from 0 to 1 and is 1 if individuals strongly agree with the question, and 0 if they strongly disagree.

Measure 5. Perception of corruption (M5)

For perception of corruption in RDD analyses, I use a question from the World Value Survey (WV wave 7 2017-22, questionnaire page 9, question 112):

“Now I’d like you to tell me about your views on corruption – when people pay a bribe, give a gift or do a favor to other people in order to get the things they need done or the services they need. How would you place your views on corruption in your country on a 10-point scale where “1” means “there is no corruption in my country” and “10” means “there is abundant corruption in my country”. If your views are somewhat mixed, choose the appropriate number in between." Like other measures, responses are scaled from 0 to 1, where 0 indicates strong disagreement that there is corruption in the country and 1 indicates strong agreement.

Additionally, several related survey questions were employed as robustness checks. These questions are explicitly defined and discussed in detail at each point of use (for example, in Appendix D, Appendix E4, E5).

D. OLS Results.

Table D1: *Individual Level OLS Results for Each Subgroup*

	Dependent Variable: Perception of Income Inequality		
	(1)	(2)	(3)
By Employment Status:			
Employed	0.165***	0.127***	0.083***
	(0.019)	(0.018)	(0.019)
Unemployed	0.154***	0.143***	0.086***
	(0.022)	(0.022)	(0.023)
By Income Group:			
Low Income	0.111***	0.117***	0.079***
	(0.022)	(0.022)	(0.023)
High Income	0.147***	0.144***	0.090***
	(0.018)	(0.019)	(0.019)
By Education:			
No Higher Education	0.133***	0.123***	0.085***
	(0.016)	(0.016)	(0.017)
Higher Education	0.201***	0.164***	0.094***
	(0.030)	(0.029)	(0.030)
Individual Controls	No	Yes	Yes
Country Controls	No	No	Yes

Notes: Individual data is from ISSP 2019 survey, 15 countries (14 EU countries and US). Country controls are from World Bank database. Country fixed effects are included in country controls. Standard errors in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%. From column (1) to column (3) controls are included.

Table D2: *Effect of Perceived Corruption on Alternative Measures of Perception and Acceptance of Income Inequality*

Measure	Coefficient	Observations	R-squared
M1.1 (Main - Perception of Income Inequality)	0.084***	18,458	0.053
II1 (Differences in income are too large)	0.080***	18,458	0.052
II2 (Feelings about wealth)	0.151***	18,458	0.034
II3 (Society now: Diagrams)	0.061***	18,458	0.219
II4 (Society should be: Diagrams)	0.007	18,458	0.041
II5 (Fairness of distribution)	0.147***	18,458	0.127
M1.2. (Acceptance of inequality)	0.074***	18,458	0.102

Notes: Full sample. All specifications include individual and country controls. * Significant at 10%; ** significant at 5%; *** significant at 1%.

The following measures were used to construct dependent variable:

- M1.1, Measure 1. Perception of income inequality, see section 3.2 Data on perceptions: Measures from survey.
- II1 (Part of M1): "To what extent do you agree or disagree with the following statements? Differences in income in [your country] are too significant: strongly agree, agree, neither agree nor disagree, disagree, strongly disagree, cannot choose, and do not know" (ISSP, 2019, questionnaire, p. 6).
- II2: "Some people feel angry about differences in wealth between the rich and the poor, while others do not. How do you feel when you think about differences in wealth between the rich and the poor in [COUNTRY]? Please place yourself on a scale of 0 to 10, where 0 means not angry at all and 10 means extremely angry" (ISSP, 2019, questionnaire, p. 8).
- II3 (Part of M1): "These five diagrams show different types of society. Please read the descriptions, look at the diagrams, and decide which you think best describes [your country] (See appendix B, Figure B.1): What type of society is [your country] today, which diagram comes closest?" (ISSP, 2019, questionnaire, p. 11).
- II4 (Part of M1.2): "These five diagrams show different types of society. Please read the descriptions, look at the diagrams, and decide which you think best describes [your country] (See appendix B, Figure B.1): What type of society would you prefer for [your country]?" (ISSP, 2019, questionnaire, p. 11).
- II5 (Part of M1.2): "How fair or unfair do you think the income distribution is in [COUNTRY]? Choose between 'Very fair', 'Fair', 'Unfair', 'Very unfair'" (ISSP, 2019, questionnaire, p. 11).
- M1.2: Similar measure to M1 but constructed from questions II4 and II5, showing the acceptance of income inequality.

E. RDD Results and Tests.

Table E1: *RDD Results for the US for Multiple Models*

Regions	Dependent Variable (Perceptions)	RDD Coefficient	R-squared	Observations
East North Central	Income Inequality	-0.26*	0.02	299
	Corruption and Income Inequality	-0.23*	0.03	
	Luck vs Hard Work	-0.17	0.02	
	Income Inequality state only (MI+MO)	-0.47**	0.09	155
	Corruption and Income Inequality state only (MI+MO)	-0.46**	0.07	
West North Central	Income Inequality	-0.45**	0.05	227
	Corruption and Income Inequality	-0.45**	0.03	
	Luck vs Hard Work	-0.33	0.05	
Middle Atlantic	Income Inequality	0.58***	0.03	164
	Corruption and Income Inequality	0.75***	0.06	
	Luck vs Hard Work	0.16	0.05	
	Income Inequality state only (NY+PA)	-0.51**	0.05	151
	Corruption and Income Inequality state only (NY+PA)	-0.34	0.05	
Pacific	Income Inequality	0.52***	0.15	229
	Corruption and Income Inequality	0.43*	0.07	
	Luck vs Hard Work	-0.09	0.11	
	Income Inequality state only (CA)	0.52**	0.04	174
	Corruption and Income Inequality state only (CA)	0.38	0.05	
Pacific+Mountain	Income Inequality	0.73***	0.09	62
	Corruption and Income Inequality	0.55**	0.09	
	Luck vs Hard Work	-0.29	0.11	
South Atlantic	Income Inequality	-0.00*	0.03	306
	Corruption and Income Inequality	-0.00	0.01	
	Luck vs Hard Work	-0.00	0.02	
	Income Inequality state only (FL)	-0.00	0.04	106
	Corruption and Income Inequality state only (FL)	-0.00	0.02	
East+West South Central	Income Inequality	-0.31*	0.03	300
	Corruption and Income Inequality	-0.26*	0.03	
	Luck vs Hard Work	0.34**	0.04	
	Income Inequality state only (TX+LA+AL)	-0.30	0.02	183
	Corruption and Income Inequality state only (TX+LA+AL)	-0.10	0.01	

Notes: WV survey wave 7 (2017-2022) data is used. Income inequality corresponds to M2, Luck vs hard work to M3, and Corruption and Income Inequality to M2 and M5 combined. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table E2: *RDD Results for the US (With the 2016 Presidential Election)*

Region/State	**2016 Election**	RDD Coefficient	R-squared	Observations
East North Central	Mostly Republican	-0.26*	0.02	299
West North Central	Mostly Republican	-0.45**	0.05	227
States (MI+MO)	Republican	-0.47**	0.09	155
Middle Atlantic	Mostly Democratic	0.58***	0.03	164
States (NY+PA)	Mixed (NY Democrats, PA Republican)	-0.51**	0.05	151
States (NY)	Democratic	0.52**	0.05	91
States (PA)	Republican	-0.36	0.05	75
Pacific	Democratic	0.52***	0.15	229
Pacific+Mountain	Mostly Democratic	0.73***	0.09	62
State (CA)	Democratic	0.52**	0.04	174
East & West South Central	Republican	-0.31*	0.03	300
States (TX+LA+AL)	Republican	-0.30	0.02	183
South Atlantic	Republican	-0.00*	0.03	306
State (FL)	Republican	-0.00	0.02	106

2016 Election (Republican, Democratic).
"Republican" = majority of states in the group won by Republicans; "Mixed" = split results.

Table E3: *RDD Results for Europe (with Pre-event Trust in National Government)*

Country	Cutoff	RDD Coefficient	R-squared	Observations	Trust in Nat. Gov. (2016–2017)
Netherlands	Optm.	0.02	0.03	124	**High (63%)**
Sweden	Optm.	0.25	0.04	124	**High (60%)**
Finland	Optm.	-0.35***	0.10	125	**High (58%)**
	14 day	-0.46***	0.15	105	
Germany	7 day	0.84**	0.04	149	**Medium (54%)**
	14 day	0.03	0.02	451	
Estonia	7 day	0.55***	0.02	460	**Medium (48%)**
	14 day	0.04	0.01	889	
France 1	7 day	0.87***	0.03	319	
	14 day	0.02	0.02	467	**Low (29%)**
France 2	7 day	0.79**	0.07	233	
	14 day	0.06	0.05	518	
Romania	Optm.	0.24*	0.04	267	**Low (28%)**
	14 day	0.12	0.06	201	
Italy 1	7 day	0.94***	0.03	297	
	14 day	0.10	0.01	495	
Italy 2	7 day	0.72**	0.05	314	**Low (16%)**
	14 day	0.07	0.01	623	
Italy 3	7 day	1.17***	0.08	344	
	14 day	0.19***	0.03	627	

WV survey wave 7 (2017–2022) data. For cases with fewer observations in the ±7 window, Optimal bandwidth results are reported. * p<0.10; ** p<0.05; *** p<0.01. All models include individual controls and day dummies.

Table E4: *RDD Results for the Pacific Region (7-day Cutoff and Optimal Bandwidth)*

	7-day Cutoff			Optimal Bandwidth (3 day cutoff)		
Variable	Coefficient	R-squared	Obs.	Coefficient	R-squared	Obs.
II1	0.22^{***}	0.07	268	0.52^{***}	0.05	229
II2	0.11^{**}	0.06	268	0.43^{**}	0.05	229
II3	0.12^{**}	0.06	268	0.43^{**}	0.05	229

Notes: WV survey wave 7 (2017-2022) data is used. * Significant at 10%; ** significant at 5%; *** significant at 1%.
II1 is equivalent to M2 as defined in the data section, a binary measure. II2 is the same question but an index ranging from 0 to 1, II3 is an index ranging from 1 to 10.

Table E5: *RDD Results for Italy (7-day Cutoff and Optimal Bandwidth)*

	7-day Cutoff			Optimal Bandwidth (20 days)		
Variable	Coefficient	R-squared	Obs.	Coefficient	R-squared	Obs.
II1	1.177^{***}	0.076	344	0.205^{***}	0.026	784
II2	0.117^{***}	0.074	347	0.199^{***}	0.024	796
II3	0.146^{**}	0.057	344	0.141^{*}	0.024	784

Notes: WV survey wave 7 (2017-2022) data is used. * Significant at 10%; ** significant at 5%; *** significant at 1%.
II1 is equivalent to M2 as defined in the data section, a binary measure. II2 is the same question but an index ranging from 0 to 1, II3 is an index ranging from 1 to 10.

Table E6: *Smoothness of Confounding Variables for the US*

Region	Gender	Age	Work	Education	Income	Balanced
East North Central	0.04	-0.06	-0.01	-0.32	0.00	✓
	(0.432)	(0.965)	(0.830)	(0.148)	(0.961)	
West North Central	-0.07	-0.60	0.06	-0.32	-0.05	✓
	(0.432)	(0.771)	(0.159)	(0.167)	(0.532)	
States (MI+MO)	-0.03	3.20	0.03	-0.47	0.09	✓
	(0.756)	(0.124)	(0.520)	(0.173)	(0.298)	
Middle Atlantic	-0.02	0.25	0.03	-0.05	-0.06	✓
	(0.818)	(0.910)	(0.496)	(0.829)	(0.453)	
NY + PA	-0.06	2.96	0.03	-0.11	0.03	✓
	(0.473)	(0.259)	(0.565)	(0.718)	(0.733)	
South Atlantic	0.02	-2.35	0.02	-0.09	-0.01	✓
	(0.785)	(0.121)	(0.464)	(0.639)	(0.934)	
East+West South Central	0.15	-2.63	0.02	-0.27	-0.01	✓
	(0.008)	(0.102)	(0.620)	(0.149)	(0.844)	
Pacific	-0.03	-0.79	0.02	0.14	-0.00	✓
	(0.610)	(0.606)	(0.481)	(0.404)	(0.939)	
Pacific+Mountain	-0.07	0.12	-0.01	0.21	-0.06	✓
	(0.220)	(0.942)	(0.810)	(0.251)	(0.299)	
California	-0.04	-2.15	0.02	0.20	-0.04	✓
	(0.550)	(0.246)	(0.610)	(0.325)	(0.508)	

Notes: RDD balance test results showing discontinuity coefficients with p-values in parentheses. Coefficients represent the jump in each variable at the cutoff. P-values > 0.10 indicate smoothness (balanced variables). All variables are smooth at the cutoff ($p > 0.10$). ✓ indicates balanced results. Regions with statistically insignificant RDD coefficients are excluded from the test.

Table E7: *Smoothness of Confounding Variables for Europe*

Country	Gender	Age	Work	Education	Income	Balanced
Estonia	0.011	-0.182	-0.129	-0.222	-0.067	✓
	(0.802)	(0.907)	(0.486)	(0.152)	(0.373)	
Finland	-0.015	0.803	-0.433	0.661	0.018	✓
	(0.897)	(0.850)	(0.241)	(0.124)	(0.933)	
Italy	-0.016	0.048	-0.221	0.011	0.051	✓
	(0.769)	(0.981)	(0.315)	(0.950)	(0.631)	
Germany	-0.041	-1.151	0.258	0.194	-0.049	✓
	(0.624)	(0.695)	(0.412)	(0.539)	(0.692)	
France	-0.013	-1.404	0.151	-0.008	0.018	✓
	(0.826)	(0.515)	(0.555)	(0.972)	(0.866)	

Notes: RDD balance test results showing discontinuity coefficients with p-values in parentheses. Coefficients represent the jump in each variable at the cutoff (±7 days). P-values > 0.10 indicate smoothness (balanced variables). All variables are smooth at the cutoff ($p > 0.10$). ✓ indicates balanced results. Countries with statistically insignificant RDD coefficients are excluded from the test.

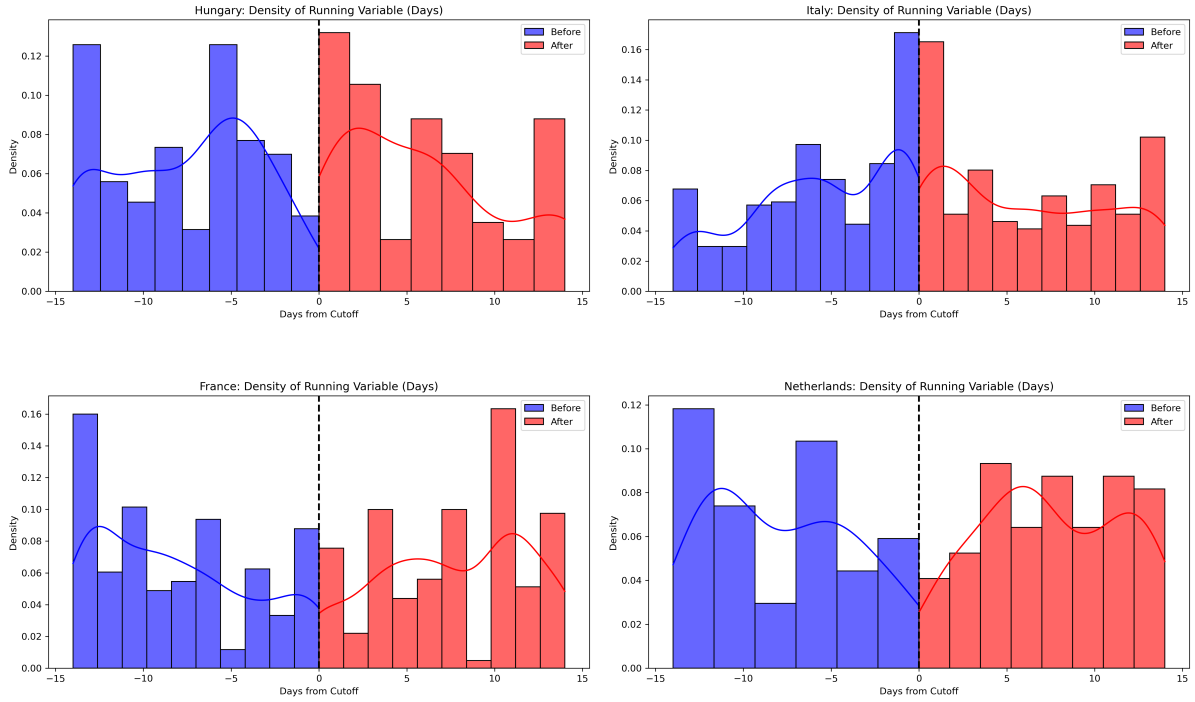


Figure E8. Density of the Running Variable

Density estimates of the running variable for selected countries. Blue: observations before the cutoff. Red: observations after the cutoff. The vertical line marks cutoff(day 0). There is no visual evidence of bunching or a discontinuity in density at the cutoff in any country, consistent with no systematic manipulation of survey timing.

Table E9: Placebo Tests: Estimated Treatment Effects at Artificial Cutoffs for the US

	Placebo Cutoff (days)				
Region	-5	-4	4	5	7
East North Central	—	—	-0.034	-0.034	-0.037
			(0.598)	(0.621)	(0.602)
West North Central	0.042	0.059	-0.106	-0.071	-0.042
	(0.726)	(0.583)	(0.266)	(0.471)	(0.726)
States (MI+MO)	—	—	-0.074	-0.084	0.014
			(0.512)	(0.530)	(0.919)
Middle Atlantic	—	—	-0.096	-0.097	-0.006
			(0.191)	(0.200)	(0.943)
NY + PA	—	-0.067	-0.113	-0.031	0.012
		(0.673)	(0.205)	(0.734)	(0.894)
South Atlantic	—	—	0.093	0.078	0.063
			(0.051)	(0.120)	(0.225)
Florida	—	—	0.099	0.099	0.124
			(0.194)	(0.207)	(0.110)
West + South	—	—	-0.085	-0.058	-0.023
			(0.153)	(0.338)	(0.707)
TX + LA + AL	—	—	-0.123	-0.094	-0.071
			(0.104)	(0.221)	(0.358)
Pacific	—	—	0.023	0.026	0.005
			(0.670)	(0.629)	(0.934)
California	—	—	0.020	0.014	0.005
			(0.764)	(0.834)	(0.938)
Pacific+Mountain	0.026	0.019	-0.051	-0.036	-0.066
	(0.524)	(0.664)	(0.405)	(0.568)	(0.387)

Notes: Each cell reports the estimated treatment effect (standardized coefficient) from a placebo RDD using the indicated artificial cutoff, with the corresponding p-value in parentheses. All specifications mirror the main model. Dashes (—) indicate insufficient observations on one side of the fake cutoff.

No stars appear because none of the placebo tests are significant, confirming that the main results are not driven by pre-existing trends.

Table E10: *SMD Test Results for the US*

Region	Gender	Age	Work	Education	Income
East North Central	0.081	0.005	0.022	0.202	0.005
West North Central	0.132	0.049	0.255	0.219	0.099
States (MI+MO)	0.049	0.252	0.100	0.284	0.161
Middle Atlantic	0.034	0.018	0.098	0.032	0.117
NY + PA	0.126	0.222	0.098	0.064	0.063
South Atlantic	0.030	0.179	0.078	0.052	0.009
Florida	0.139	0.255	0.064	0.288	0.005
East+West South Central	0.311	0.199	0.057	0.170	0.023
TX + LA + AL	0.299	0.076	0.057	0.311	0.107
Pacific	0.058	0.061	0.079	0.093	0.008
California	0.080	0.166	0.068	0.130	0.086
Pacific+Mountain	0.149	0.009	0.030	0.138	0.121

Notes: Standardized Mean Differences (SMD) for covariate balance between treatment and control groups. Thresholds: $|SMD| < 0.1$ = excellent balance, $0.1 \leq |SMD| < 0.2$ = good balance, $0.2 \leq |SMD| < 0.5$ = moderate imbalance. The majority fall below 0.25, with isolated values under 0.35, indicating good to excellent balance.

Table E11: *SMD Test Results for Europe*

Country	Gender	Age	Work	Education	Income
Estonia	0.161	0.204	0.136	0.051	0.071
Finland	0.036	0.045	0.053	0.098	0.049
France	0.016	0.059	0.026	0.097	0.020
Germany	0.002	0.002	0.098	0.035	0.002
Italy	0.094	0.014	0.169	0.004	0.022
Netherlands	0.009	0.015	0.184	0.278	0.098
Romania	0.059	0.130	0.117	0.121	0.120
Sweden	0.072	0.063	0.057	0.009	0.049

Notes: Standardized Mean Differences (SMD) for covariate balance between treatment and control groups around the cutoff. Thresholds: $|SMD| < 0.1$ = excellent balance, $0.1 \leq |SMD| < 0.2$ = good balance, $0.2 \leq |SMD| < 0.5$ = moderate imbalance. The vast majority of SMD values are below 0.25 across countries (with a single isolated value of 0.28 for education in the Netherlands), indicating good to excellent balance.

F. RDD Plot Examples

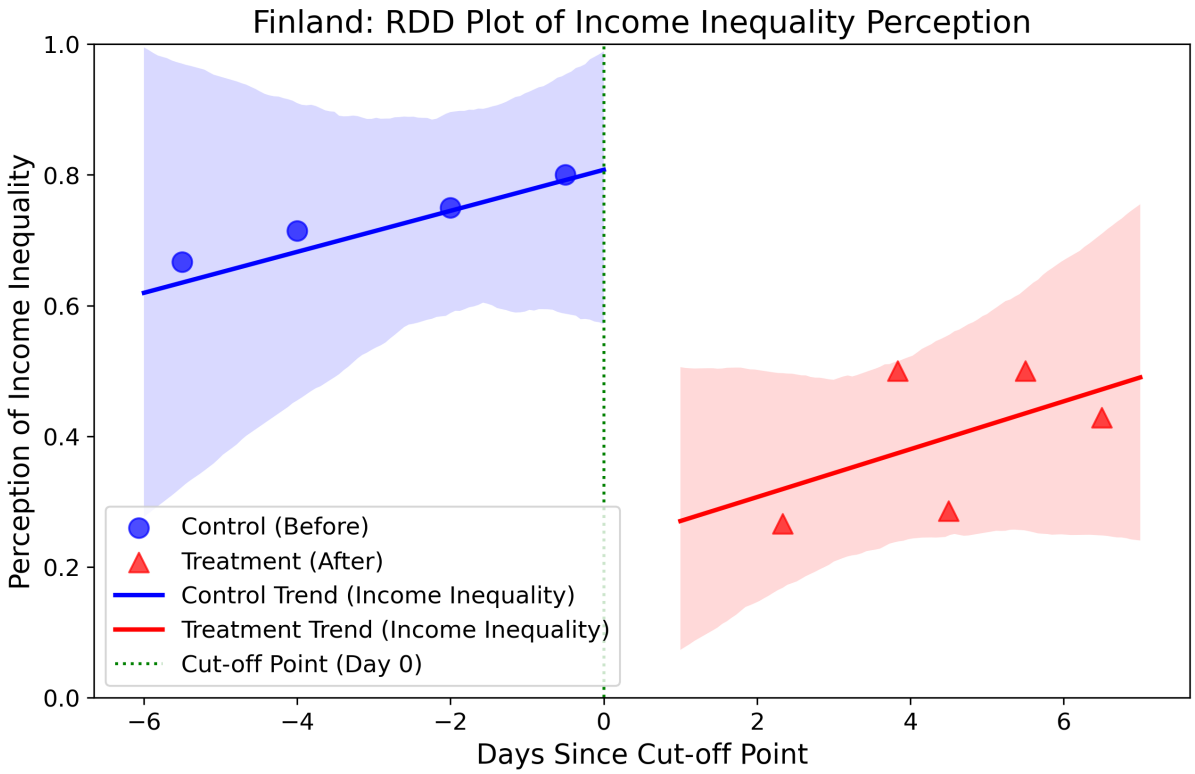


Figure F1. Discontinuity of Perception of Income Inequality (Decrease) Around Cutoff in Finland.

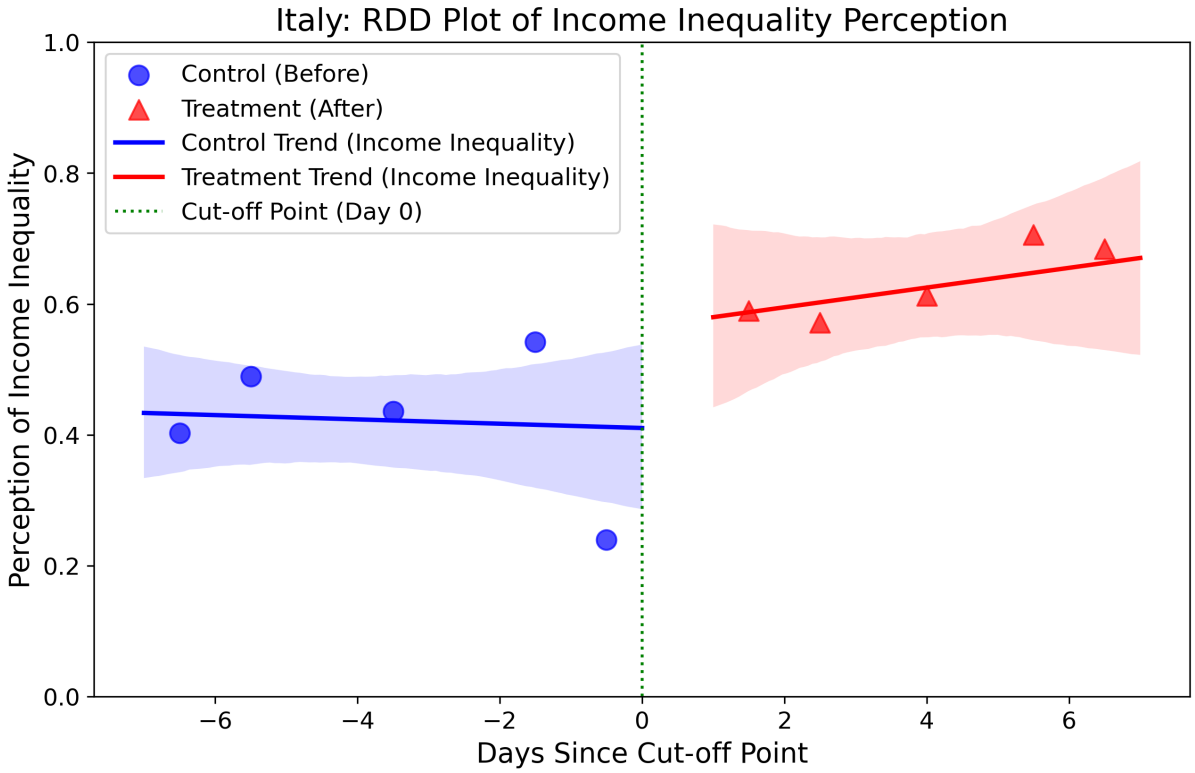


Figure F2. Discontinuity of Perception of Income Inequality (Increase) Around Cutoff in Italy.

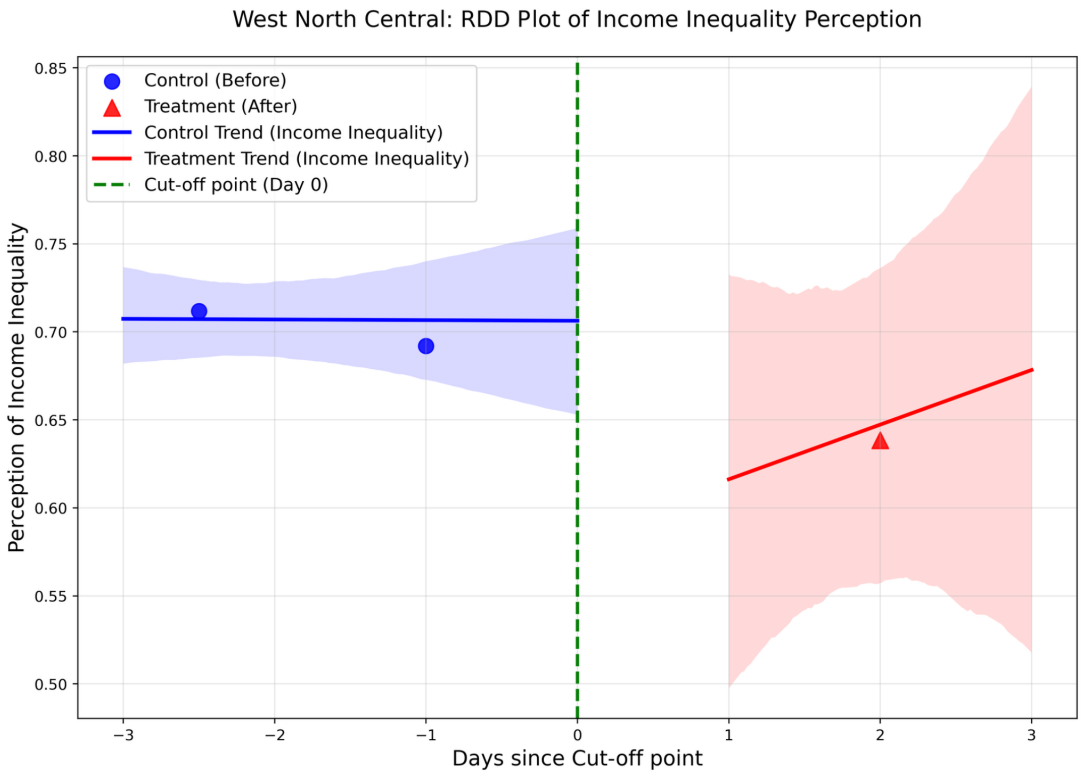


Figure F3. Discontinuity of Perception of Income Inequality (Decrease) Around Cutoff in the US.

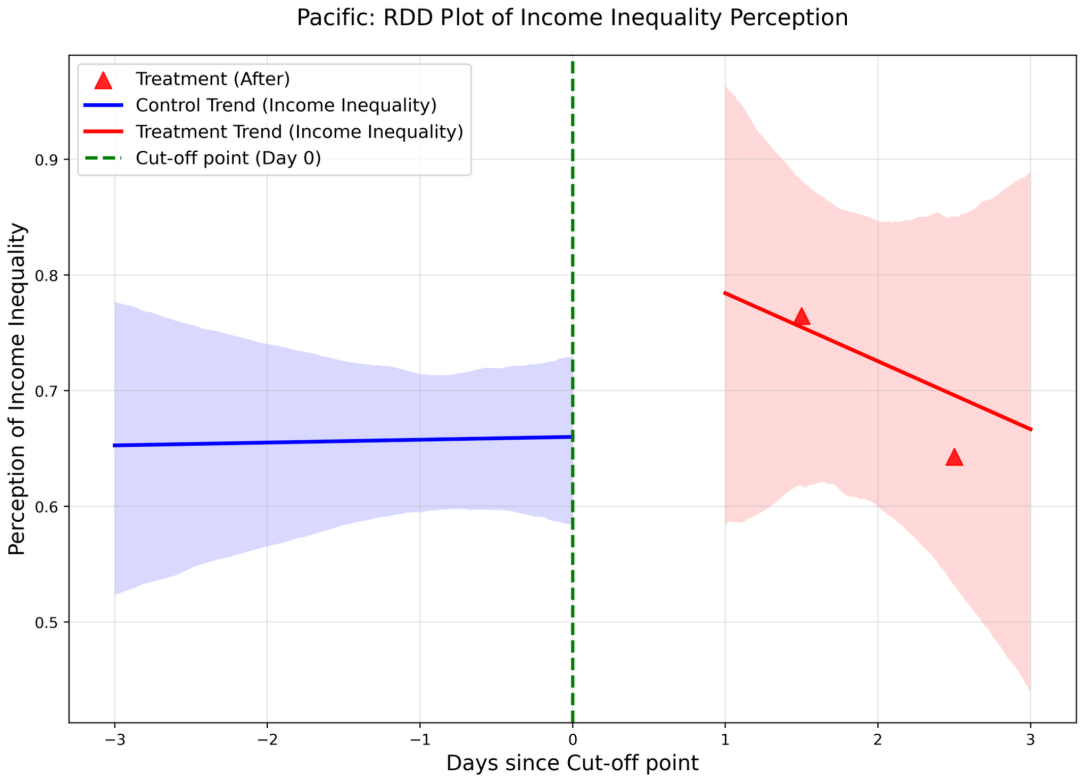


Figure F4. Discontinuity of Perception of Income Inequality (Increase) Around Cutoff in the US.

Abstrakt

Vnímání a přijímání příjmové nerovnosti zásadně závisí na přesvědčení o spravedlnosti ekonomického systému. Události, které přitahují značnou pozornost veřejnosti a narušují toto přesvědčení, jako jsou medializované případy kriminality bílých límečků, proto mohou výrazně měnit společenské preference. V této studii na případech kriminality bílých límečků empiricky ověřuji, jak jejich zveřejnění ovlivňuje vnímání nerovnosti veřejností.

Zjišťuji, že dopad těchto kauz je podmíněn výchozí mírou důvěry v instituce. V prostředí s nízkou důvěrou veřejnost chápe tyto kauzy jako důkaz systémové korupce a častěji považuje rozdělení příjmů za nespravedlivé a příjmové rozdíly za nadměrné. V prostředí s vysokou důvěrou je naopak zveřejnění těchto případů spojeno s poklesem vnímané nerovnosti. Tento rozdíl interpretuji jako doklad toho, že tam, kde veřejnost institucím důvěřuje, vnímá trestní stíhání pachatelů z řad elit jako signál, že stát má kapacitu prosazovat právo a vyvozovat odpovědnost. Při nízké důvěře veřejnost v těchto kauzách spatřuje potvrzení systémové korupce.

Výsledky naznačují, že kauzy kriminality bílých límečků představují „zátěžový test“ kapacity státu a mohou posilovat či oslabovat vnímanou legitimitu ekonomického řádu. Studie tak přispívá k lepšímu pochopení toho, jak integrita institucí utváří přesvědčení ve společnosti a podporu veřejnosti pro redistribuční politiky.

Working Paper Series
ISSN 2788-0443

Individual researchers and the on-line versions of CERGE-EI Working Papers (including their dissemination) are supported by RVO 67985998 from the Economics Institute of the CAS

Specific research support and/or other grants are acknowledged at the beginning of the paper.

(c) Zhasmen Gevorgyan, 2026

All rights reserved. No part of this publication may be reproduced, stored in a retrieval system, or transmitted in any form or by any means, including electronic, mechanical, or photocopy, recording, or other without the prior permission of the publisher.

Published by
Charles University, Center for Economic Research and Graduate Education (CERGE)
and
Economics Institute of the Czech Academy of Sciences (EI)
CERGE-EI, Politických vězňů 7, 111 21 Prague 1, Czech Republic
Phone: + 420 224 005 153
Email: office@cerge-ei.cz
Web: https://www.cerge-ei.cz/

Editor: Byeongju Jeong

The paper is available online at https://www.cerge-ei.cz/working-papers/.

Electronically published November 19, 2026

ISBN 978-80-7343-632-2 (Univerzita Karlova, Centrum pro ekonomický výzkum a doktorské studium)