

# Financial Market Exposure Increases Generalized Trust, Particularly Among the Politically Polarized\*

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How can we build trust, especially in polarized societies? We propose that exposure to broad financial markets—where individuals place their assets in the hands of large groups of unfamiliar agents who nonetheless have the incentive and ability to promote their interests—can contribute to generalized trust. In a randomized controlled trial, we encourage Israelis to hold or trade stocks for up to seven weeks. We find that participation in financial markets increases the probability of expressing generalized trust by 5.9 percentage points, equivalent to 25% of the control group mean. The effects are stronger among political partisans, robust to negative price changes, and unrelated to changes in political preferences.

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Virtually every economic transaction has within itself an element of trust (Arrow, 1972), and a large literature argues that trust is “the cause or precondition of much of what is good and valuable in society” (Nannestad, 2008). Generalized trust, conceptualized as a belief in the goodwill of others and an expectation that others have an incentive and ability to promote one’s interests (Sapienza, Toldra-Simats and Zingales, 2013; Levi and Stoker, 2000), is widely seen as a facilitator of economic activity (Sapienza and Zingales, 2011; Guiso, Sapienza and Zingales, 2004, 2008), and a central component of social capital (Putnam, 1992). Generalized trust has been associated with economic growth (Zak and Knack, 2001; Algan and Cahuc, 2010, 2013), good governance (Knack, 2000; Bjørnskov, 2010), and compliance with public health guidance (Siegrist and Bearth, 2021; Bargain and Aminjonov, 2020). However, trust is far from common around the world (Figure 1). In a majority of countries sampled by the World Values Survey, less than one in four survey respondents agrees that “most people can be trusted” (Inglehart et al., 2020). Low levels of trust are especially common in politically polarized societies (Rapp, 2016), a concerning pattern given the rising levels of political polarization in many democracies worldwide (Boxell, Gentzkow and Shapiro, 2022).

How can societies build trust? Research suggests that societal levels of trust are a consequence of contemporary civil society strength and political institutions (Putnam, 1992; Weingast, 1998), historical legacies of extractive institutions (Nunn and Wantchekon, 2011), personal traumatic experiences (Drelichman, Vidal-Robert and Voth, 2021), patterns of discrimination (Alesina and La Ferrara, 2002), levels of education (Guiso, Sapienza and Zingales, 2008), religiosity (Chuah et al., 2016), economic inequality (Gustavsson and Jordahl, 2008), and exposure to various outgroups (Alesina and La Ferrara, 2002; Finseraas et al., 2019). In line with these studies, it is commonly thought that generalized trust is a personal disposition largely acquired through early socialization (Dohmen et al., 2012), and that durably increasing generalized trust is extremely challenging (Uslaner, 2002). This is especially the case in an era of enhanced political polarization (Boxell, Gentzkow and Shapiro, 2022), where social and political groups are perceived by many citizens to be in opposition to one another.

Building on studies examining the relationship between market integration and prosocial behavior (Henrich et al., 2004; Baldassarri, 2020; Enke, 2023; Agneman and Chevrot-Bianco, 2023), and based on the expectation that personal experiences with risk can inform people’s level of trust (Malmendier and Nagel, 2011; Nee, Holm and Opper, 2018), we propose that exposure to broad financial markets, and specifically

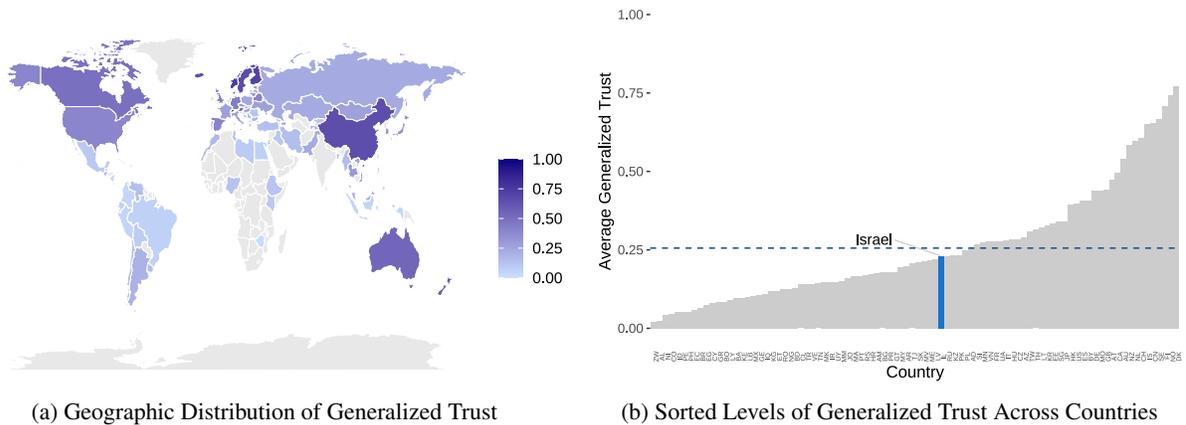


Figure 1: **Generalized trust around the world.** This figure reports cross-national patterns of generalized trust from the World Values Survey (Wave 7). For each country, we report the share of respondents who state that most people can be trusted. Since Israel is not included in the most recent wave of the World Value Survey, we report average generalized trust based on data from the 2004 World Value Survey.

opportunities to invest in and trade stocks, can increase generalized trust. To test this expectation, we turn to Israel, a highly polarized society (Bassan-Nygate and Weiss, 2020) with low levels of generalized trust (see Figure 1), and report results from a randomized controlled trial in which we encouraged Israelis to invest in and trade a specific stock for up to seven weeks. We included no additional political or social information. We then elicit respondents’ levels of generalized trust and identify the effects of exposure to financial markets on generalized trust. We find that trading stocks durably increases generalized trust, particularly among polarized partisans. Consistent with our hypothesis that a positive experience of entrusting one’s assets to unfamiliar others who nonetheless have an incentive to make one better off, we find that the effect is larger among participants whose investments did particularly well.

## Financial Markets and Trust

The finance literature emphasizes that generalized trust is a necessary condition for the development of financial markets where individuals exchange sums of money for a promise of future economic gains (Sapienza and Zingales, 2011). In line with this theoretical insight, existing studies report a positive relationship between societal levels of trust and the development of financial markets (Sapienza and Zingales, 2011; Xu, 2020), and other work shows that individual’s level of trust increases their participation in financial markets

(Guiso, Sapienza and Zingales, 2004, 2008; Balloch, Nicolae and Philip, 2015; Georgarakos and Pasini, 2011; Leblang, Smith and Wesselbaum, 2022). Moreover, scholars have shown that corporate scandals that reduce individuals' trust in the stock market also reduce their stock market participation (Sapienza and Zingales, 2012; Giannetti and Wang, 2016; Gurun, Stoffman and Yonker, 2018).

Thus far, the literature mainly focused on trust as a *cause* of financial participation rather than the reverse. However, recent findings suggest that market interactions shape morals and prosocial behavior (Henrich et al., 2004; Jha, 2013; Baldassarri, 2020; Margalit and Shayo, 2021; Enke, 2023). Hence, causality might also work in the opposite direction. Market transactions are facilitated by norms of fairness, trust, and pro-sociality towards “a generalized other” (Arrow, 1972; Sapienza and Zingales, 2011). Accordingly, evidence from a range of cross-cultural (Henrich et al., 2004; Enke, 2023) and local-contemporary (Baldassarri, 2020; Agneman and Chevrot-Bianco, 2023) studies point to a robust association between market integration and generalized trust. However, market integration, and specifically participation in financial markets, is naturally associated with a myriad of potentially confounding factors (Duflo and Saez, 2002; Conlin et al., 2015; Almenberg and Dreber, 2015). In this study, we therefore examine whether randomly assigned exposure to markets can increase generalized trust.

Specifically, the experience of investing in the stock market requires an individual investor to trust other parties to handle their investment conscientiously and reliably (Guiso, Sapienza and Zingales, 2004). By investing in financial markets, citizens expose themselves to a degree of risk (Sapienza and Zingales, 2011). However, over time, investments can yield mutually beneficial returns. This can have a particularly pronounced effect among those political partisans for whom distrust of the other side is especially salient.

In contrast to previous studies that emphasize how financial markets help citizens internalize the costs of conflict and increase their support for peace-promoting parties (Jha and Shayo, 2019), the mechanism linking financial markets and generalized trust is fundamentally different. Participation in financial markets, we argue, increases generalized trust because it provides people with an opportunity to directly experience a setting where general and unknown other agents have the incentive and the ability to promote one's—i.e. the investor's—interests.

## The Randomized Controlled Trial

Identifying the effects of exposure to financial markets on personal levels of generalized trust with observational data is challenging for multiple reasons. First, financial market participation is arguably endogenous to trust. Indeed, recent studies suggest that generalized trust is a salient determinant of investment in financial markets (Georgarakos and Pasini, 2011). Second, generalized trust is often viewed as a disposition acquired through socialization (Dohmen et al., 2012). Accordingly, various unobserved factors that correlate with generalized trust (e.g., one's upbringing) also correlate with participation in financial markets (Almenberg and Dreber, 2015; Conlin et al., 2015).

To overcome this identification challenge, we implemented a large-scale randomized controlled trial in Israel in the run-up to the 2015 national elections, in which we incentivized Jewish Israelis to hold or trade a specific stock from the Israeli or Palestinian stock exchanges for a period of up to seven weeks. To the best of our knowledge, this is the first randomized controlled trial to assign stock portfolios to potential investors and evaluate the social consequences of exposure to financial markets. Our randomized controlled trial was rolled out in three main stages.

### Pre-Treatment Survey and Treatment Assignment

Using an online panel, we surveyed 1,418 Israelis screening 73 respondents who provided incomplete information, inconsistent information or finished the survey in an unusually quick time. As part of our baseline surveys, we collect demographic information as well as pre-treatment measures of generalized trust, asking respondents the following question originating in the World Values Survey: “*Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?*” Possible responses included 1) “*Most people can be trusted*” 2) “*Need to be very careful with other people*” and 3) “*I don't know.*”<sup>1</sup>

After collecting baseline data, we block-randomized the remaining 1,345 survey respondents to treatment and control conditions (treatment = 1,036, control = 309). Treated individuals were incentivized to participate in an instructions survey that detailed the rules of the study, informed respondents about their allocated assets, and quizzed respondents to ensure that they understood how the value of their assets would be

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<sup>1</sup>This measure is suitable for capturing the belief (rather than preference) element of generalized trust (Sapienza, Toldra-Simats and Zingales, 2013).

determined. Participants were assigned either 200NIS (~\$50) or 400NIS (~\$100). These endowments are substantial when compared to many experiments in social science, or to the average *daily* wage of 312NIS in Israel in 2014. We consider the 840 treated individuals who completed the instructions survey as compliers (52%), and report both intent to treat (ITT) and treatment effect on the treated (TOT) estimates, using treatment status as an instrument for actual take-up of treatment. Appendix Table A1 reports descriptive statistics and compares our sample to the general population. Figure A3 reports balance checks between treatment and control.

### **Treatment Rollout: Holding and Trading Stocks**

Through an accessible and simplified survey-based platform, compliers received weekly updates about the price of their assigned assets, as well as a description and valuation of their current portfolio after markets closed on the last day of the week.<sup>2</sup> Treated participants were given a weekly opportunity to decide to hold their existing portfolio or buy or sell up to 10% of that portfolio before markets opened the following week. To incentivize engagement, if they did not register any decision, including a decision to hold, they lost the 10% that they could have traded. A third (two-thirds) of treated participants were randomly selected to trade on the platform for four (seven) weeks with three (six) trading periods. 69% of compliers engaged in trading during every opportunity, and 80% of compliers traded in all but one week.

### **Post-Treatment Surveys**

Following the trading period compliers and control participants reported outcomes of interest as part of our endline survey. We collected non-compliers outcomes in a later follow-up survey. Endline surveys included a measure of generalized trust, as well as other outcomes of interest relating to vote choice and financial literacy explored in companion papers (Jha and Shayo, 2019, 2023).

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<sup>2</sup>Foreign assets were listed in foreign currency. We thus fixed the exchange rate for the duration of the experiment so that there was no exchange rate risk. Assets in the experiment represented derivative claims on the authors' research account: the treatment did not include direct ownership of the underlying asset.

## Results

Table 1 reports our main results, showing both ITT and TOT estimates.<sup>3</sup> First, we show that, as expected, treatment status does not affect *pre-treatment* levels of generalized trust. Indeed, when employing pre-treatment trust as an outcome in the first column of Table 1, the treatment point estimate is very small, amounting to 0.8 percentage points, and statistically indistinguishable from zero. The second column of Table 1 shows that, after the trading period, participants randomly assigned to trade financial assets are significantly more likely to say that “most people can be trusted”. The effect is stable at around six percentage points when we also control for the randomization strata and for demographic, political and attitudinal controls (measured pre-treatment). This effect is equivalent to 25% of baseline trust in the control group. Importantly, levels of trust in our control group (23.8%) are similar to levels of trust measured by the World Values Survey in Israel during the most recent wave fielded in 2004 (23.02%) (Inglehart et al., 2020). Not surprisingly, the IV-TOT results (which employ treatment assignment as an instrument for compliance) yield slightly larger point estimates. In substantive terms, the effect of trading stocks on generalized trust is 1.5 times larger than the gender difference in trust. In Appendix C, we address concerns about differential attrition, and in Appendix E, we demonstrate that our results are not an artifact of an “income shock,” as treatment effects are identical when participants are assigned to endowments of different values.

Skeptics might worry that financial markets can increase trust only when citizens benefit financially, but negative experiences with investing might actually reduce generalized trust (Malmendier and Nagel, 2011). To explore this possibility, we leverage data on the price change of assets on the day on which participants were divested from their stock and measure participants’ gains and losses. Since participants were randomly assigned to a single asset, with randomly assigned divestment dates, the price changes of the underlying stock from initial allocation to an individual’s divestment day are exogenously determined.

In Figure 2, we explore the consequences of negative exogenous price performance of the stock until divestment. We split our sample to include control group respondents and the subset of treated respondents whose stock price increased (decreased) by divestment day. Our sample included 327 (709) respondents whose stock price decreased (increased) by their divestment day, with price changes ranging between -

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<sup>3</sup>For the purpose of other studies in our randomized controlled trial (Jha and Shayo, 2019), we oversampled centrists in our surveys. We reweigh our sample to resemble the party shares of the Jewish voting population in 2013, to examine the general Jewish Israeli electorate, and specifically Jewish Israeli partisans.

Table 1: Trading Stock Increases Generalized Trust

Outcome: Generalized Trust (0/1)					
	Pre-Treat Trust	ITT	ITT Block FE	ITT Block FE + Cont.	IV-TOT
Treatment	0.008 (0.031)	0.059** (0.028)	0.057** (0.028)	0.057** (0.028)	0.060** (0.030)
Left Wing	0.129*** (0.040)	0.107*** (0.035)	0.109 (0.147)	0.109 (0.164)	0.111 (0.166)
Right Wing	0.009 (0.030)	0.031 (0.027)	-0.037 (0.148)	-0.112 (0.160)	-0.117 (0.161)
Pre-Treat Trust		0.515*** (0.031)	0.497*** (0.034)	0.472*** (0.034)	0.472*** (0.034)
Num.Obs.	1245	1245	1245	1245	1245
R2	0.013	0.268	0.330	0.357	0.358
Control Mean	0.257	0.238	0.238	0.238	0.238
Control SD	0.437	0.426	0.426	0.426	0.426

Block FE account for 104 blocks in which treatment was assigned. We created blocks by stratifying sequentially by respondents' 2013 vote choice, sex, experience trading stocks, an indicator for whether respondents would recommend Arab stocks to a friend, region, discrepancies in the 2013 vote, and willingness to take risks. Controls in columns 4-5 include measures of left-wing support, right-wing support, pre-treatment generalized trust, gender, age, education, marital status, religiosity, geographical location, income, news consumption willingness to take risk, patience, a measure of survey timing, and a financial literacy score. All models include weights to match the party shares of the Jewish vote in 2013.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

11.785% and 15.761%. We find no evidence of a negative treatment effect across our different subsamples.

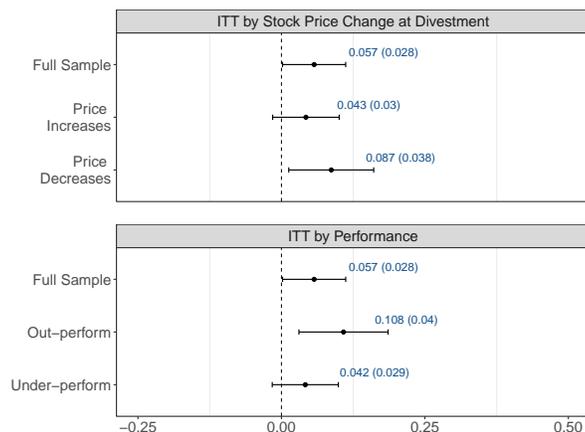


Figure 2: **Treatment effects are not dampened by exogenous price shocks and are stronger among investors who out-perform in their decisions.** This figure reports ITT point estimates, robust standard errors, and corresponding 95% confidence intervals of our main specification that includes covariates and block fixed effects. We focus on our full sample, as well as various subsamples, to explore the mechanism and scope of our effect.

Furthermore, one might expect that our identified effects will be larger among successful investors who made decisions that outperformed the price of their assigned stock. We interrogate this expectation in the bottom panel of Figure 2. Specifically, we split our sample to include all control respondents and the subset of out-performing (under-performing) treated respondents. Our sample included 265 (771) out-performing (under-performing) respondents. The financial consequences of respondents' performance ranged between -59.827 NIS and 11.784 NIS. We find that our general effects are stronger among investors who out-performed the exogenous price performance of their original stock. We interpret these patterns to suggest that better decision-making with more favorable realized outcomes is associated with larger effects on generalized trust, but poorer decision-making does not lead to backlash.

In Figure 3, we explore conditional average treatment effects on several subsamples of interest. The top panel of Figure 3 suggests that our main point estimate is larger among polarized respondents – supporters of left and right-wing parties rather than centrist respondents. This is notable in the age of partisan polarization (Graham and Svobik, 2020), in which many partisans perceive inherent opposition between social and political groups. The middle panel of Figure 3 shows that our main treatment has positive effects both for respondents who were trusting and non-trusting prior to the treatment but is precisely estimated only

among non-trusting individuals. Finally, the bottom panel of Figure 3 suggests that treatment effects are larger among men.

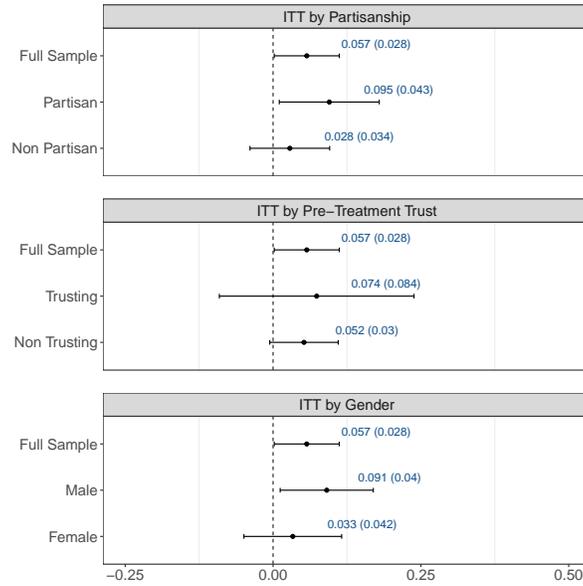


Figure 3: **Treatment effect magnitude varies as a consequence of partisanship, gender, and levels of pre-treatment trust.** This figure reports ITT point estimates, robust standard errors, and corresponding 95% confidence intervals of our main specification that includes covariates and block fixed effects. We focus on our full sample, as well as various subsamples to explore effect heterogeneity.

## Discussion

In this paper, we argue that financial markets provide citizens with opportunities to share risks and realize the mutually beneficial gains of placing resources in the hands of other parties, and this informative experience increases their willingness to trust others. Reporting results from a novel randomized controlled trial, we substantiate this expectation: exposure to financial markets increases generalized trust, particularly among polarized partisans.

We make three contributions to the existing literature. First, we contribute to the existing scholarship on generalized trust (Alesina and La Ferrara, 2002; Sapienza, Toldra-Simats and Zingales, 2013; Bjørnskov, 2010; Nunn and Wantchekon, 2011; Moscona, Nunn and Robinson, 2017; Putnam, 1992) by identifying an approach for increasing trust in a highly polarized society. We show that empowering people and providing them with opportunities to invest in financial markets can increase generalized trust. This result is

especially important, given the prevalence of low levels of trust worldwide, particularly in our current age of polarization (Boxell, Gentzkow and Shapiro, 2022). Our approach to trust-building is notable for being non-paternalistic and scalable. Rather than informing participants how they should regard others, we allow them to develop their preferences based on their own experiences. This non-paternalistic intervention could be scaled by integrating financial market exposure into at-scale cash transfer programs (Egger et al., 2022) and encouraging non-investors towards informed participation in financial markets.

Second, we contribute to the literature on market exposure and pro-sociality. A range of existing studies analyzes rich data to document the links between market exposure and pro-social behavior (Henrich et al., 2004; Jha, 2013; Baldassarri, 2020; Agneman and Chevrot-Bianco, 2023; Enke, 2023). Through our randomized controlled trial, we complement existing studies and provide causal evidence that exposure to financial markets increases generalized trust because it gives investors hands-on learning experiences that emphasize the benefits of cooperation under uncertainty. Finally, we contribute to the literature on generalized trust and economic behavior (Guiso, Sapienza and Zingales, 2004, 2008; Sapienza and Zingales, 2011, 2012), by demonstrating that trust is not only a determinant of financial market participation but also its direct causal effect.

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*Supplementary Information*

<b>A</b>	<b>The RCT: Additional Information</b>	<b>SI-1</b>
<b>B</b>	<b>Sample Characteristics</b>	<b>SI-1</b>
<b>C</b>	<b>Attrition</b>	<b>SI-1</b>
<b>D</b>	<b>Ruling Out Alternative Mechanisms</b>	<b>SI-2</b>
<b>E</b>	<b>Additional Analyses</b>	<b>SI-2</b>
<b>F</b>	<b>Ethics Statement</b>	<b>SI-3</b>

## **A The RCT: Additional Information**

We provide a description of our experimental protocol in the main text. All survey instruments can be accessed through the following link [Redacted]. Here we further provide illustrative examples of the survey platform through which treated respondents received exposure to financial markets. In Figure A1, we present a screenshot of the portion of our financial survey in which respondents receive information about their initial endowment. In Figure A2, we further present a screenshot of the portion of the weekly financial survey in which treated respondents received information about their current portfolio performance. As depicted in Figure A2, at that moment, respondents were able to sell 10% of their portfolio and buy other assets (if they had available funds in their account).

## **B Sample Characteristics**

In Table A1, we report key descriptive statistics of our sample and benchmark descriptive statistics against the Jewish Israeli population. Note that in our main specification, we employ survey weights to ensure our sample resembles the distribution of partisan voters in Israel. We further report a balance test in Figure A3. Given our randomization procedure, our sample is well-balanced along a range of covariates.

## **C Attrition**

In Table A2, we show that treatment is positively correlated with non-response to our generalized trust outcome measure. This raises concerns regarding differential attrition, which might bias our main estimates. We address this concern in two separate ways. First, in Figure A4, we show that non-attributing respondents are well-balanced on a range of demographics between treatment and control conditions. Second, we report Horowitz-Manski bounds for our main point estimates in Figure A5. Doing so, we show that our estimate remains positive even in a very extreme instance where all treated respondents with missing outcomes report 0 in our post-treatment trust item and all non-treated respondents with missing outcomes report 1 in our post-treatment trust item.

## **D Ruling Out Alternative Mechanisms**

In the main manuscript, we argue and provide additional evidence that exposure to financial markets increases generalized trust because it facilitates personal experiences in which investors are exposed to shared risks and potential benefits of large-scale economic cooperation. However, given the underlying correlation between pre-treatment left-wing support and levels of generalized trust (Table 1), and given recent evidence that exposure to financial markets increases Israelis' support for left-wing parties and the Israeli-Palestinian peace process (Jha and Shayo, 2019), a possible alternative mechanism linking exposure to financial markets with shifts in generalized trust could relate to changes in partisan and political preferences.

To distinguish our learning-by-doing mechanism from a political preference mechanism, we report a series of mediation analyses (Imai et al., 2011), estimating the indirect effect of our treatment on generalized trust as a consequence of shifts in partisan preferences (panels a-b) and support for peace (panel c). Results from Figure A6 suggest that shifts in partisan and political preferences are unlikely mediators of our main effect. Specifically, across all panels in Figure A6, the indirect effect of our treatment on generalized trust via partisan and political preferences is very small and imprecisely estimated, in contrast to our treatment's direct effect, which is large (for the most part precisely estimated), and similar in magnitude to the total effect. This pattern further increases our confidence that exposure to financial markets increases generalized trust because of the learning experience it facilitates and not because it changes peoples' partisan and political preferences.

## **E Additional Analyses**

In our original study, we oversampled centrist voters (doubling their 2013 vote share) in order to identify treatment effects on the political preferences of Israeli centrist voters. However, in this study, our main interest is examining treatment effects on generalized trust among the general population of Jewish-Israeli voters. For that reason, in our main specification, we employ weights that match our sample to the party shares of the Jewish vote in 2013. In Table A3, we report additional analyses, identifying the effects of our treatment on generalized trust with the centrist voter over-sample and without employing survey weights. These results are largely similar to our preferred estimates in the main text. However, in our unweighted analyses, the magnitude of our point estimates is smaller, and our findings only approach conventional levels

of statistical significance (e.g., in our ITT estimate in Table 3, we obtain a  $p.value = 0.1$  (two-tailed test)). Regardless, it is worth pointing to the substantial difference between our ITT on pre- and post-treatment levels of trust in this context. Indeed, as one might expect, the effects of our treatment on pre-treatment levels of generalized trust are very close to 0 ( $\beta = 0.006$ ). In contrast, point estimates on post-treatment levels of trust are over 6.5 times that magnitude amounting to a point estimate of  $\beta = 0.04$ . This emphasizes that our treatment had a substantively meaningful effect on post-treatment levels of generalized trust, even when focusing on our unweighted specification.

In Figure A7, we examine potential heterogeneity in our main results as a factor of asset type. Specifically, we consider two important dimensions. First, we consider the value of an initial portfolio, which was exogenously assigned at either 200NIS or 400NIS. Second, we consider whether the assigned assets were Israeli or Palestinian companies or indices. We find that effects are largely consistent across different types of assets. Finally, in Figure A8, we consider the correlation of partisanship with generalized trust, and report our ITT estimates for partisan subsamples.

## **F Ethics Statement**

This study follows the principles for human subjects research published by the American Political Science Association. Throughout the study, we did not collect any identifiable information, and our experimental procedures were reviewed and approved by the relevant Institutional Review Boards in our home institutions. As part of the study, subjects were informed that they were taking part in a voluntary research study and that they could exit the study at any time. To ensure subjects were able to give informed consent (and understood all aspects of the survey), all survey materials were translated into Hebrew by members of the research team. Our research procedures did not involve deception, and this study did not intervene in political processes as described in Principle 10 of the APSA Principles and Guidance for Human Subjects Research. Finally, participants were compensated according to the survey firm's terms and conditions. Moreover, a subset of our sample received additional compensation in the form of an investing portfolio tracking particular assets. The value of portfolios was clearly described to all treated respondents at the start of the study.

בטבלה הבאה מופיעה הרשימה המלאה של הנכסים הפיננסיים שישתתפו במחקר. הרשימה כוללת גם מניות של חברות מסיימות וגם מדדים (index funds).

המניות כוללות בנקים וחברות תקשורת.

המדדים עוקבים אחר הערך של כמה מהחברות הציבוריות הגדולות בכל מדינה (בדרך כלל מדד ממוסד כולל בין 20 ל-30 חברות).

שימו לב במיוחד לנכס שבו זכית ולמספר המניות שברשותך. אותו מספר המניות יעמוד לרשותך גם בשבוע הבא. לפיכך, אם המחיר של הנכס יעלה - ערך הנכסים שלך יעלה בהתאם. אם המחיר של הנכס יורד - ערך הנכסים שלך יורד בהתאם. הרשימה מסודרת בסדר אלפביתי לפי סימול המניה או המדד באנגלית.

• Here is a list of all the assets participating...  
 • Both company stocks and index funds (explained).

• Note the asset you won and the # of shares you own.  
 • If the price of your asset increases, the value of your assets will increase accordingly. If the price goes down...

שם	שם באנגלית	סימול	מטבע	מחיר הנכס היום (במטבע מקומי)	מספר המניות שברשותי	ערך הנכסים שלי (במטבע מקומי)	ערך הנכסים שלי (בש"ח)
בנק אקבנק, טורקיה	Akbank Turkey	AKBNK	TRY	8.55			
מדד של בורסת רבת עמון בירדן	Amman SE General Index Fund	AMGNRLX	JOD	2,186.18			
בנק (חברת תקשורת ישראלית)	Bezeq	BEZQ	ILS	663.10			
בנק ירדן	Bank Of Jordan	BOJX	JOD	2.80			
בנק פלסטין	Bank Of Palestine	BOP	JOD	2.78			
מדד של 20 המניות הגדולות בקפריסין	Cyprus/FTSE Top 20 Index Fund	CYFT	EURO	44.44			
מדד של 30 המניות הגדולות בבורסת קהיר במצרים	Egypt EGX 30 Index Fund	EGX30	EGP				
מצרים טלקום	Telecom Egypt	ETEL	EGP				
ירדן טלקום	Jordan Telecom	JTEL	JOD				
בנק לאומי לישראל	Bank Leumi	LUMI	ILS	1,288.00			
פלסטין טלקומניקיישן (חברת תקשורת פלסטינית)	Palestine Telecommunications	PALTEL	JOD	5.94	6.122	36.36	200
מדד של הבורסה הפלסטינית בשכם	Palestine-Stock Exchange Index Fund	PLE	JOD	504.76			
מדד תל-אביב 25	Tel Aviv TA-25 Index Fund	TA25	ILS	1,452.46			
טורקסל (חברת תקשורת טורקית)	Turkcell	TCELL	TRY	14.80			
בנק יוניון הלאומי של מצרים	Union National Bank of Egypt	UNBE	EGP	5.90			
מדד של 30 המניות הגדולות בבורסת איסטנבול בטורקיה	Borsa Istanbul 30 Index Fund	XIU30	TRY	106,359.21			
כסף מזומן	CASH	CASH	ILS	1.00			

לקבלת מידע מפורט ועדכני על כל אחד מהנכסים הללו, באפשרותך להקליד את הסימול של אותו נכס באתר <http://il.investing.com> או באמצעים של הבורסות השונות.

Figure A1: Screenshot of the initial survey providing respondents with information regarding their initial portfolio.

The screenshot shows a Hebrew investment platform interface with the following sections and callouts:

- Link to website with info on assigned stock:** Points to a URL: <http://i.investing.com/equities/bezeq-ord>
- Composition, price and updated value of portfolio:** Points to the "מצב תיק הנכסים שלך" (Your Asset Portfolio Status) section.
- Buying decision (if current portfolio includes cash):** Points to the "החלטות ההשקעה שלך" (Your Investment Decisions) section.
- Selling decision (if current portfolio includes stocks):** Points to the "מכירה" (Sell) section.

The main text in the screenshot includes:

**להלן העדכון על ביצועי תיק ההשקעות שלך.**  
 כידוע לך, הנכס שלך עוקב אחר המחיר של מניית בזק. ניתן לעקוב אחרי מנייה זו באתרי אינטרנט רבים. לדוגמה, באתר הבאי: <http://i.investing.com/equities/bezeq-ord>

**מצב תיק הנכסים שלך**  
 בשבוע שעבר שווי תיק הנכסים שלך היה 200 ש"ח. לרשותך עמדו 0.302 מניות בקירוב ו-0 ש"ח במזומן. מחיר הנכס בשבוע שעבר היה: 663.1 ש"ח. מחיר הנכס המעודכן לפי נתוני הסגירה של יום חמישי הוא: 668.1 ש"ח. לפיכך, השווי המעודכן של נכסך הוא 201.5 ש"ח.

**החלטות ההשקעה שלך**  
 אנא חזן להלן את החלטות הקניה והמכירה שלך. קניה ומכירה של נכסים אינן כרוכות בעמלה.

**קניה**  
 כיום אין ברשותך כסף מזומן ולכן אינך יכול לקנות מניות.

**מכירה**  
 באפשרותך למכור עד 10% מהמניות שברשותך. המכירה תהיה לפי המחיר המעודכן שצויין למעלה, 668.1 ש"ח. הכסף מהמכירה ייצבר לזכותך במזומן ולא יהיה צמוד לשום נכס פיננסי. אנא הקלד את אחוז המניות שברצונך למכור. באפשרותך לבחור כל מספר בין 0 ל-10 [10] (נא להזין מספרים שלמים בלבד) אם אינך מעוניין למכור את המניות או חלקן, הקלד אפס.

המשך

Figure A2: Screenshot of the platform in which treated respondents received their weekly update regarding their portfolio. In this stage, respondents were able sell/buy 10% of their portfolio.

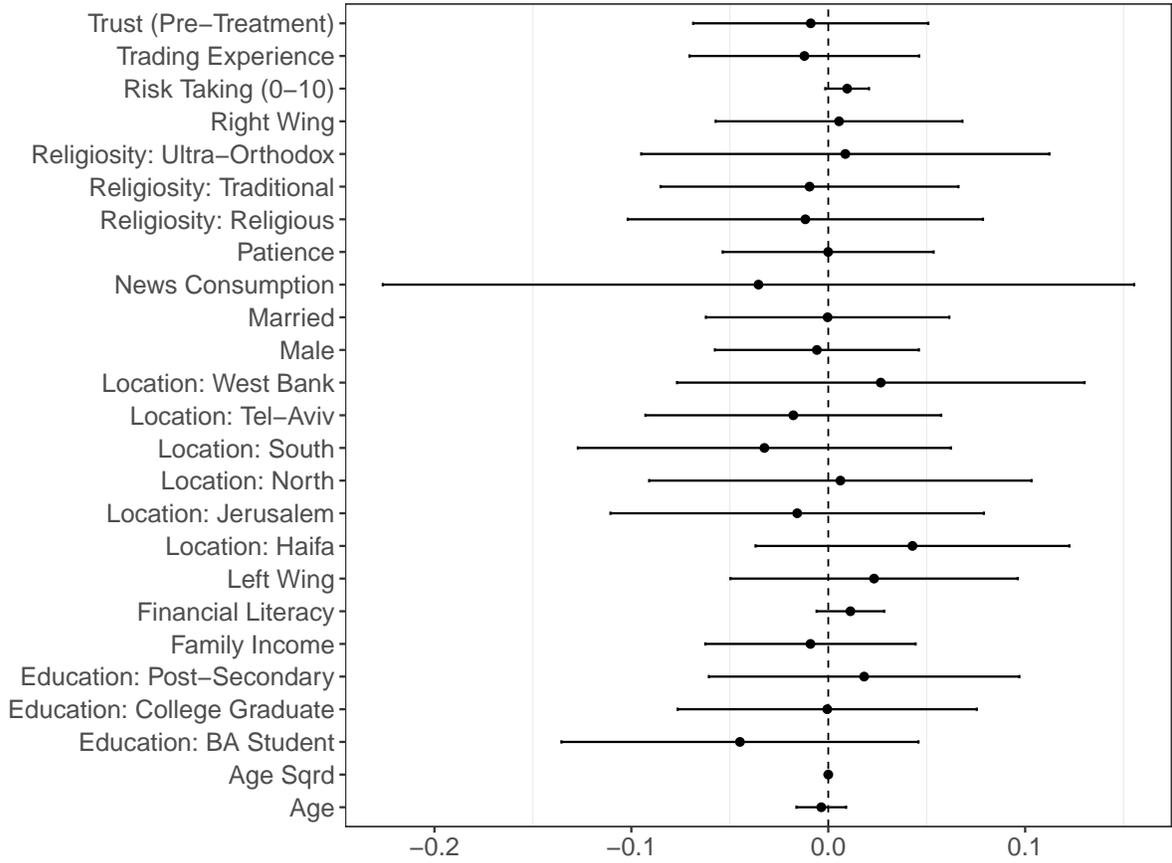


Figure A3: **This Figure reports balance on pre-treatment covariates amongst our full sample.** Point estimates are extracted from a regression in which treatment status is regressed over pre-treatment covariates ( $n = 1345$ ).

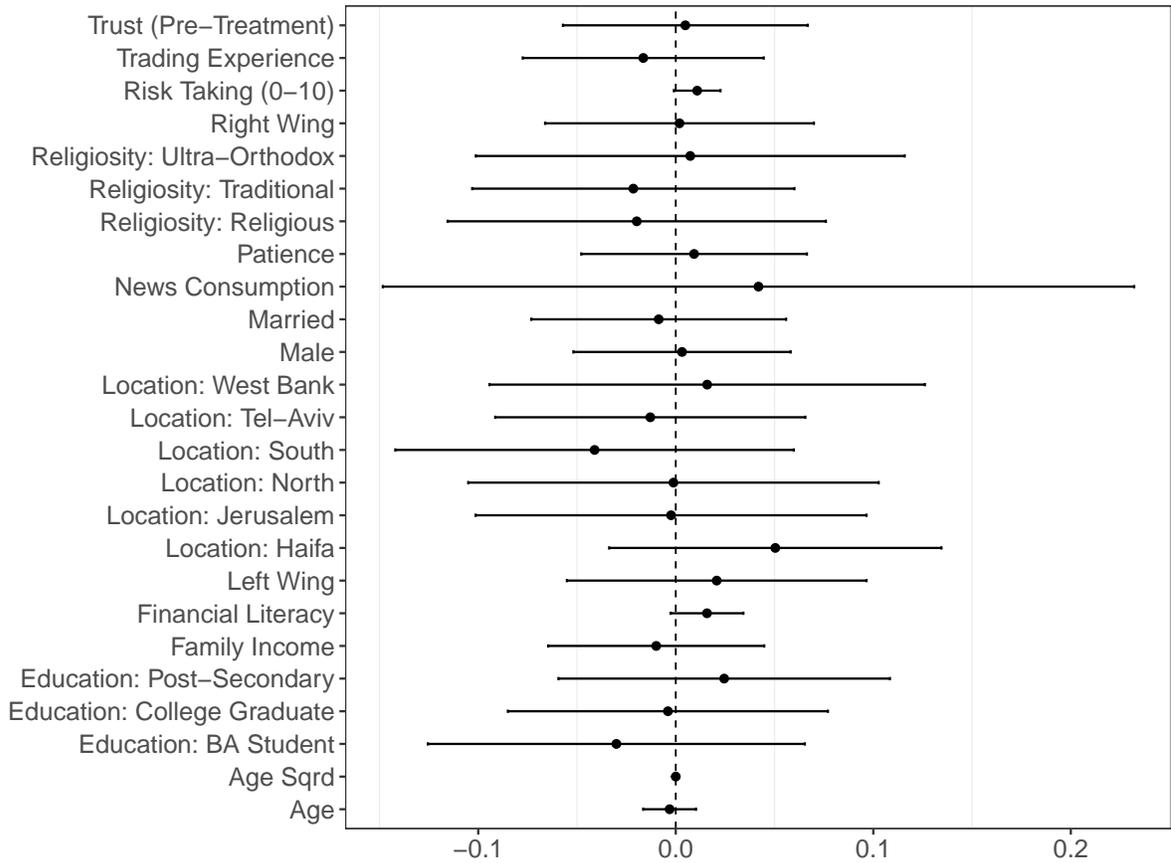


Figure A4: **This figure reports balance on pre-treatment covariates amongst non-atriving respondents.** Point estimates are extracted from a regression in which treatment status is regressed over pre-treatment covariates for respondents for whom we obtain a post-treatment measure of generalized trust ( $n = 1,245$ ).

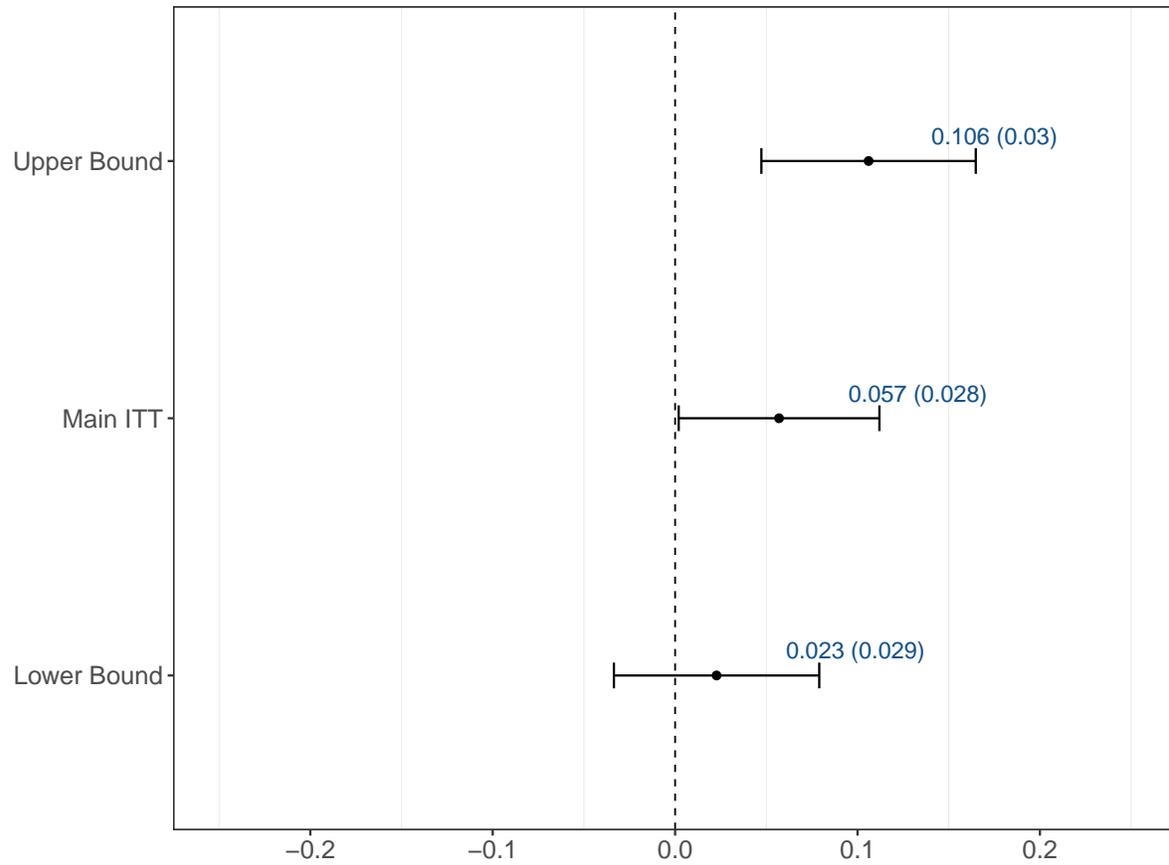


Figure A5: **This Figure reports Horowitz-Manski bounds for our main ITT point estimate.** These results suggest that our estimate remains positive even under the most conservative bounds.

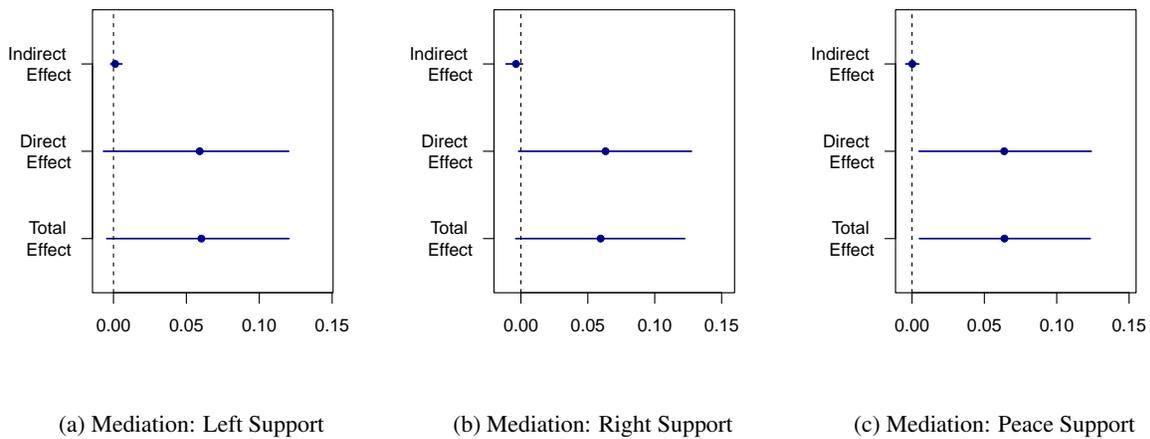


Figure A6: **Partisan and political preferences do not mediate the relationship between financial market exposure and generalized trust.** This figure reports results from mediation analyses (Imai et al., 2011), in which we consider the indirect effect of our treatment on generalized trust via partisan and political preferences (support for left and right-wing parties and support for peace). In panel a, we show that the indirect effect of our treatment on generalized trust mediated by increased support for left-wing parties is very close to 0, substantially smaller than the direct effect of our treatment on generalized trust and imprecisely estimated. The same pattern of result holds in panel b when considering support for right-wing parties as a mediator and in panel c when considering support for peace as a mediator, emphasizing that shifts in partisan and political preferences are an unlikely mechanism accounting for our main result.

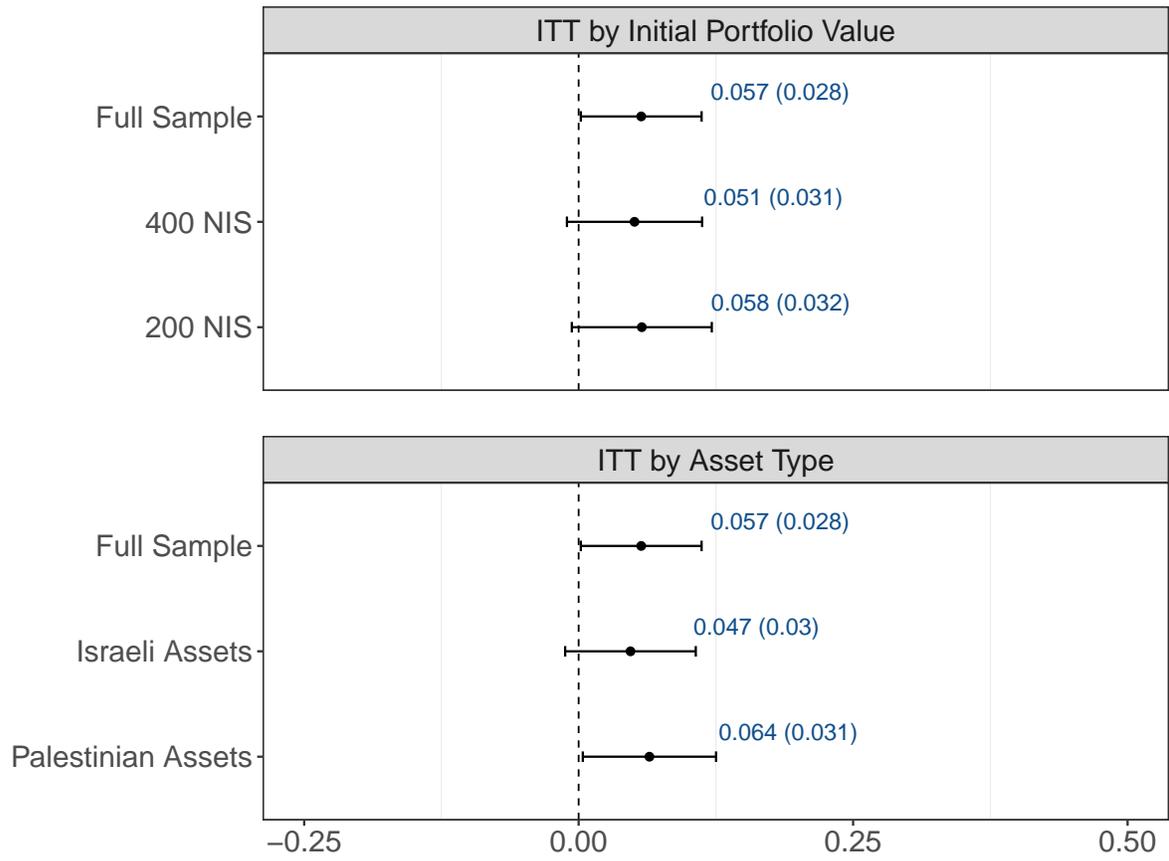


Figure A7: **Treatment effects are similar for respondents assigned to 200NIS and 400 NIS portfolios and to respondents assigned Israeli and Palestinian assets.** This figure reports ITT point estimates, robust standard errors, and corresponding 95% confidence intervals of our main specification. We focus on our full sample, as well as various subsamples, to explore the stability of our main result.

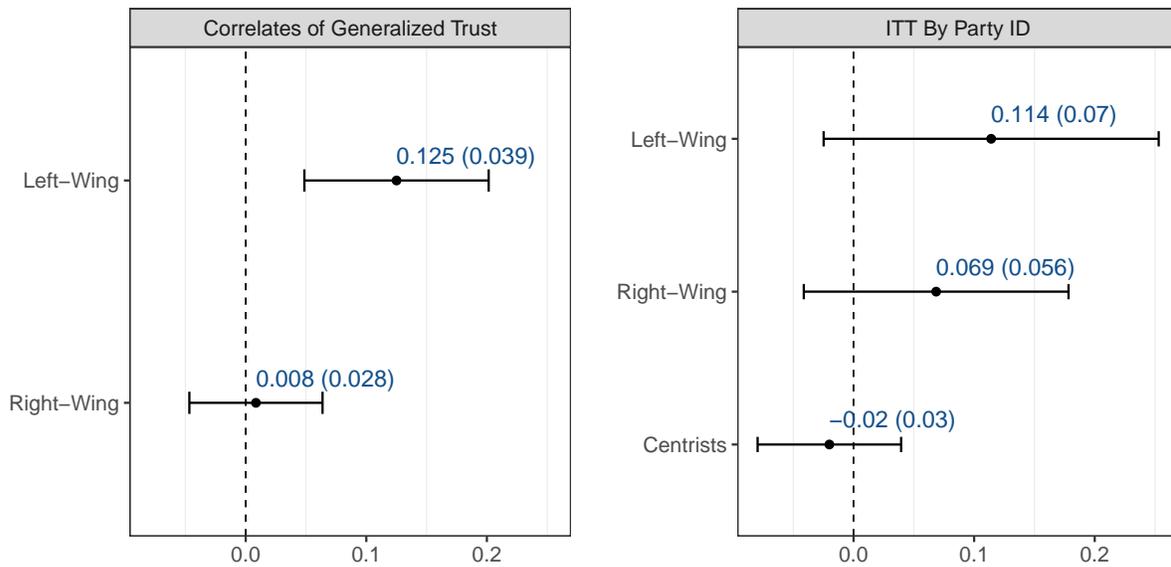


Figure A8: **Partisan identification and generalized trust.** This figure considers the correlation of pre-treatment party identification with generalized trust and the effects of our main treatment on center, right-wing, and left-wing participants. In the left panel, we report point estimates from an OLS model regressing pre-treatment generalized trust over indicators for left and right-wing supporters, showing that left-wing partisans are more likely to report higher levels of generalized trust. In the right panel, we report our ITT estimates for left-wing, right-wing, and centrist subsamples, demonstrating that point estimates are largest for left-wing voters and smallest for centrists.

Table A1: **Descriptive Statistics.** This figure reports descriptive statistics of our sample, and benchmarks our sample against the Jewish Israeli population.

	Baseline Sample (N = 1345)	Israeli Jewish
<b>1. Region: Jewish Population in District (%)</b>		
Jerusalem District	9.4	11.1
Northern District	9.5	9.5
Haifa District	13.7	10.7
Central District	29.2	28.5
Tel Aviv District	19.8	20.2
Southern District	10.6	14.2
West Bank	7.8	5.8
<b>2. % Female in Jewish Pop., 18+</b>	48.3	51.4
<b>3. Age (Jewish Population above age 18 (%))</b>		
<b>Male</b>		
18-24	10.1	14.6
25-34	29.6	20.4
35-44	28.1	18.7
45-54	15	14.7
55-64	9.6	15.1
65+	7.6	16.5
<b>Female</b>		
18-24	14.2	13.3
25-34	29.7	19.2
35-44	26.3	17.9
45-54	14	14.6
55-64	10.5	15.5
65+	5.4	19.5
<b>4. Religiosity (Jewish Population, %)</b>		
Not religious/Secular	63.1	43.4
Traditional	16.8	36.6
Religious	11.9	10.6
Ultra-orthodox	8.2	9.1
<b>5. Education (Jewish Population level of schooling (%))</b>		
Less than high school grad (0 to 10 yrs.)	5.8	13.7
High school graduate (11 to 12 yrs.)	13.7	33.3
Post-secondary/BA Student (13 to 15 yrs.)	38.2	24.1
College grad and above (16+ yrs.)	42.3	28.9
<b>6. Net Monthly Income per Household (NIS)</b>		
Mean	10766	14,622
Median	12000	13,122

The prime-age sample includes only participants who completed at least one of the post-treatment financial surveys.

1. Statistical Abstract of Israel 2015, Table 2.15, 2014 Totals

2. Statistical Abstract of Israel 2015, Table 8.72, 2014 Totals

3. Statistical Abstract of Israel 2015, Table 8.72, 2014 Totals

4. Statistical Abstract of Israel 2015, Table 7.6, 2013 Totals. The data for the Israeli population is for age 20 and over.

5. Statistical Abstract of Israel 2015, Table 8.72, 2014 Totals

6. Statistical Abstract of Israel 2015, Table 5.27, 2013

Total (mean). Median is midpoint between 5th and 6th

Table A2: Treatment Effects on Attrition

Outcome: Non-Response to Trust Outcome (0/1)			
	Base	Block FE	Block FE + Cont.
Treatment	0.058*** (0.015)	0.057*** (0.016)	0.071*** (0.017)
Left Wing	-0.019 (0.019)	0.222 (0.205)	0.224 (0.187)
Right Wing	0.032 (0.019)	-0.031 (0.083)	-0.027 (0.078)
Pre-Treat Trust	-0.017 (0.018)	-0.013 (0.020)	-0.008 (0.020)
Num.Obs.	1345	1345	1345
R2	0.014	0.088	0.141
Control Mean	0.033	0.033	0.033
Control SD	0.179	0.179	0.179

This table reports the correlation of treatment with non-response to our post-treatment outcome of trust. We find evidence for differential attrition.

In Section C we reduce concerns regarding selective attrition by reporting balance tests, and Manski-Horowitz Bounds. Block FE and controls in this table are identical to our main specification in Table 1, and all regressions include weights to match the party shares of the Jewish vote in 2013.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A3: Trading Stocks and Trust (With Centrist Voter Over-Sample)

Outcome: Generalized Trust (0/1)					
	Pre-Treat Trust	ITT	ITT Block FE	ITT Block FE + Cont.	IV-TOT
Treatment	0.006 (0.029)	0.040 (0.024)	0.038 (0.025)	0.040 (0.025)	0.042 (0.027)
Left Wing	0.109*** (0.040)	0.104*** (0.035)	0.116 (0.148)	0.133 (0.163)	0.134 (0.165)
Right Wing	-0.005 (0.029)	0.032 (0.027)	-0.048 (0.143)	-0.124 (0.151)	-0.128 (0.151)
Pre-Treat Trust		0.519*** (0.029)	0.506*** (0.030)	0.483*** (0.031)	0.483*** (0.031)
Num.Obs.	1245	1245	1245	1245	1245
R2	0.008	0.270	0.330	0.352	0.352
Control Mean	0.265	0.253	0.253	0.253	0.253
Control SD	0.442	0.435	0.435	0.435	0.435

This table reports the same specification reported in Table 1 of the main text, with centrist voter oversample and without the survey weights we used to match our sample to the party shares of the Jewish vote in 2013. Our estimates in this specification are slightly noisier but substantively similar to our main estimates in Table 1 of the main text.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$