

Valuing Peace: The Effects of Financial Market Exposure on Votes and Political Attitudes

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Abstract

Can participation in financial markets lead individuals to re-evaluate the costs of conflict, change their political attitudes and even their votes? Prior to the 2015 Israeli elections, we randomly assigned Palestinian and Israeli financial assets to likely voters, and incentivized them to actively trade for up to seven weeks. No political messages or non-financial information were included. The treatment systematically shifted vote choices towards parties more supportive of the peace process. This effect is not due to a direct material incentive to vote a particular way. Rather, the treatment reduces opposition to concessions for peace, and changes awareness of the broader economic risks of conflict. While participants assigned Palestinian assets are more likely to associate their assets' performance with peace, they are less engaged in the experiment. Combined with the superior performance of Israeli stocks during the study period, the ultimate effects of Israeli and Palestinian assets are similar.

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1 Introduction

Public attention in societies facing violent conflict often focuses on ethnic animosities, fatalities, territorial disputes and military considerations, rather than on the economics. In this paper, we test whether a historically important, but nowadays relatively neglected, mechanism—exposure to *financial markets*—can lead individuals to reevaluate the costs of conflict and to change their political choices to support peace initiatives.

The basic idea is straightforward: compared to commonplace daily transactions, financial markets expose individuals to the broader economy, and from a broader economic perspective, conflicts tend to be very costly (eg Blattman and Miguel, 2010, World Bank, 2011). Indeed, the hypothesis that market exposure affects attitudes towards conflict is very old, dating back at least to Montesquieu (1748): “Commerce is a cure for the most destructive prejudices; it is almost a general rule that wherever the ways of man are gentle there is commerce; and wherever there is commerce, there the ways of men are gentle.” Theoretically, financial markets may change political attitudes as they can demonstrate the shared risks from conflict and the returns from peace. Empirically, however, measuring the causal effect of financial markets is very difficult, as individuals’ investment opportunities and decisions are associated with numerous factors that could potentially affect political choices. This paper presents results from the first study to experimentally assign individuals financial assets, allow them to trade in those assets, and trace the effects on their political views and behavior.

Our setting is the Israeli-Palestinian conflict, a geopolitically important and highly persistent conflict. Opposing interests and ethnic animosities—reinforced by more than eighty years of recurrent violence—have led many to consider the conflict intractable. Yet, the potential *economic* gains from peace are large.¹ A month and a half prior to the highly contested 2015 Israeli elections, we randomly assigned 1345 Jewish Israeli voters to either a financial asset treatment or a control group. Individuals in the treatment group received either vouchers that could be used to invest in specific stocks, or endowments of assets that tracked the value of specific indices or company stocks from both Israel and the Palestinian Authority. Participants were given incentives to learn about the performance of their asset and to make weekly decisions to buy or sell part of their portfolio. The initial value of the portfolio was either \$50 or \$100.

Individuals also participated in a parallel series of surveys that allowed us to track not

¹The Rand Corporation estimates that a two-state solution, which it regards as the most likely to succeed, will yield Israelis an economic dividend of \$123 billion over ten years, and Palestinians \$50 billion (Anthony, Egel, Ries, et al., 2015). In contrast, a return to widespread conflict would lower Israeli GDP by \$250 billion and Palestinian by \$46 billion over the same period. See also Eckstein and Tsiddon (2004).

only their investment behavior but also their political attitudes and their vote choices. Importantly, the surveys were designed so that participants answered the political surveys separately, and they did not associate them with the financial study. This helps rule out potential social desirability biases or experimenter demand effects that often plague studies on peacemaking. Section 3 details how this was achieved and verified.

Our main result is that exposure to financial markets causes large and systematic shifts in vote choices in the 2015 elections (Section 5).² Exposure to the stock market reduces the probability of voting for parties skeptical of peace negotiations—known in Israel as the *right*—by about 4 to 5 percentage points (relative to their vote share of 36% in the control). In particular, it reduces support for the incumbent *Likud* party, headed by Benjamin Netanyahu, by 4 to 5pp (relative to 20% in the control). At the same time, it increases the probability of voting for parties that support restarting the peace process—the *left*—by 4 to 6pp (relative to 25% in the control). This mainly reflects a 3 to 5pp increase in the probability of voting for the chief opposition group, *The Zionist Union*, which includes the Labour party. Consistent with random assignment, these estimates are unaffected by controlling for individuals’ vote choices in the recently held 2013 elections, as well as education, income levels, region, religiosity, risk and time preferences, initial financial literacy and other characteristics. In terms of magnitude, the effects are comparable to estimated effects of changes in security risks—e.g., living within the range of rockets from Gaza—on Israeli voters (Getmansky and Zeitzoff, 2014).

Section 6 examines the underlying mechanisms. The analysis here is more exploratory in nature, as we move away from a one-treatment one-outcome framework to multiple (potentially under-powered) sub-treatments and multiple outcomes, some of which are self-reported attitudes rather than chosen behavior. Nonetheless, we believe the results are illuminating. We start with two key alternatives: that our results reflect a direct personal *pocketbook* incentive to change one’s vote, or that it induces a change in one’s policy preferences based upon *sociotropic* (national rather than personal) considerations.³ Given that peace overtures tend to raise both Israeli and Palestinian asset prices (Zussman, Zussman, and Nielsen, 2008), individuals holding stocks on election day may have a direct material incentive to vote for parties that favor the peace process even if their political views remain unaffected. Inconsistent with the pocketbook channel, however, we find that the treatment effect is at least as strong for participants already divested by election day. Instead, the evidence suggests that individuals exposed to financial markets

²Since the entire country is a single constituency, our study had no effect on the election outcomes themselves.

³See Healy, Persson, and Snowberg, 2017 for a recent study finding evidence for both motivations, and Lewis-Beck and Stegmaier, 2007 for a review.

develop different policy preferences over peace initiatives. They increase their support not only for the general principle of a two-state solution, but also for specific, and costly, concessions for peace. These effects are specific to the peace process: if anything, preferences over redistributive policies and government intervention in markets shift slightly to the *right*.

Unpacking the mechanisms further, a substantial part of the treatment effect on support for peace initiatives appears to reflect two sociotropic factors: an increased salience of the economic dimension of conflict as well as changing predictions of the corresponding economic benefits of peace. Consistent with increased salience, treated individuals become (somewhat) more likely to emphasize economic over security issues in their importance for Israel. Further, treated individuals raise their perceptions of the benefits from a peace agreement to Israel’s economy relative to the status quo. The latter effect is greater for the *risk-averse*, suggesting that treated individuals perceive greater risks associated with status quo policies rather than lessened risks from a peace agreement. By themselves, these two variables—increases in salience of economic issues, and changes in the perceived returns to Israel’s economy of a peace settlement over status quo policies—can explain 30.2% of the average treatment effect of financial market exposure on support for peace initiatives.

Beyond these two factors, the increases in the perceived importance of economic and financial risks are also reflected in changes in consumption of financial information, as well as understanding of financial principles. Treated individuals report being more familiar with the stock market, are more likely to follow financial media, and have better knowledge of how the market performed recently. In our companion paper (Jha and Shayo, 2019), we further find that the treatment increases performance on standard financial literacy tests, including on hard-to-teach principles such as the relative riskiness of individual stocks versus mutual funds. Treated individuals also show increased propensities to invest in the stock market after the experiment.

Section 7 examines whether the treatment effects are transitory, perhaps reflecting short-term attention, or long-term, potentially reflecting mutual feedback between salience, awareness and continued learning. We find that the effects on voting intentions *do* persist a year after the intervention. In fact, we cannot rule out the possibility that the treatment changes *more* minds in the long run than in the immediate run-up to the election.

Finally, Section 8 studies the differences between being assigned to in-group (Israeli) vs out-group (Palestinian) assets. On the one hand, the out-group assets could have larger effects as they expose individuals to new sets of considerations and shared risks,

and are more likely to demonstrate the connection between financial markets and the peace process. On the other hand, out-group assets are less familiar, and there may also be stigma and psychological costs associated with “trading with the enemy”. Indeed, we find that individuals assigned domestic stocks are more likely to take up assets and are more engaged. Our prior was that the former factors would dominate. Ultimately, however, domestic assets turned out to have greater returns, strengthening their effects, and the overall effects are similar.

Unlike campaigns that distribute potentially contentious information, an important feature of our intervention is that it is arguably empowering rather than paternalistic. It encourages individuals to learn about stock markets on their own and leaves them to draw their *own* conclusions about the economic costs of different policies. Further, while the treatment is rather intensive, it does not require prohibitively high stakes or long durations: assigning \$50 worth of assets is almost as effective as assigning \$100, and meaningful effects emerge after four weeks of exposure. These elements, along with the fact that it is not necessary to expose individuals to the assets of the opposing side, raise the potential for implementing the intervention at scale and in a wide range of settings.

This paper links to a large literature on conflict and underdevelopment. An important body of work shows that places that experienced violence historically tend to be more prone to future violence, often due to changes in culture or a polarization of social identities (eg Besley and Reynal-Querol, 2014, Shayo and Zussman, 2011, 2017, Sambanis and Shayo, 2013). A parallel literature examines how economic interests may offset such *passions* and mitigate violence (Hirschman, 1977, Martin, Mayer, and Thoenig, 2008, Rohner, Thoenig, and Zilibotti, 2013, Jha, 2013, Grosfeld, Sakalli, and Zhuravskaya, 2017, Becker and Pascali, 2016). Indeed, exposure to novel financial assets appears to have had historical success at mitigating social conflict in three revolutionary states that subsequently led the world in economic growth: England, the United States and Japan (Jha, 2012, 2015). Motivated by these historical cases, our contribution lies in examining whether properly designed financial exposure can have meaningful effects in a contemporary environment. Further, unlike this literature, we show that exposure to financial markets can affect policy preferences even without directly creating a significant personal financial stake.

The paper also makes two methodological contributions. First, we implement random assignment to empirically identify the causal effects of both exposure to financial assets and opportunities to trade those assets, on individual political behavior, knowledge and attitudes.⁴ We develop a simplified trading platform that allows inexperienced individuals

⁴The previous literature on the effects of financial market exposure on political attitudes uses ob-

to hold and trade assets that track real stocks at their actual market prices. Notably, participants do not need to go through the process of purchasing the assets themselves, as everything is done through our platform. This offers a method of conducting experiments with an important set of factors that have thus far proven very hard to randomize, certainly at scale. Second, we use double-blinded samples in parallel surveys to measure treatment effects. This mitigates problems that arise when subjects modify their self-reports in response to the treatment. Such social desirability biases can be particularly problematic in studies of peacemaking. Our use of online panels can be scaled easily, particularly as internet penetration expands, reach broad representative samples, and can potentially be applied to questions well beyond the political economy of conflict.

2 Institutional and Political Context

Our study focuses on the March 2015 Israeli general elections. Israel is a parliamentary democracy with proportional representation. Elections are called at least every four years. However, disagreements within the ruling coalition led the 2015 elections to be held just a little over two years after the January 2013 elections. The intervening two years also witnessed asset price rises during peace negotiations brokered by John Kerry, and falls after their collapse, which culminated in the 2014 Gaza War (Figure B2). This recent history is particularly valuable because the 2013 elections provide a recent measure of participants' (pre-treatment) vote choices.⁵ We focus on Jewish voters, who comprise around 80% of the population.

It is important to stress that, rather than economic policies, the main dividing line between the right and the left in Israeli politics has traditionally been about the Israeli-Palestinian conflict.⁶ The Israeli right (led by the *Likud* party) largely favors the status quo, viewing concessions for peace as highly risky and likely to lead to a major deterior-

servational data. The closest paper to our's, substantively, is Jha (2015), who studies the effect of shareholding on support for parliamentary supremacy in the English Revolution. More broadly, the micro-finance and financial inclusion literature in development has made extensive use of random assignment of different financial services, such as savings accounts (Karlan and Morduch, 2010, provides a useful overview). Methodologically, a closely related paper is Bursztyn, Ederer, Ferman, and Yuchtman (2014), who assign a financial asset randomly among those that chose to purchase it through a brokerage firm, and find effects on take up by peers.

⁵One caveat: the effects we see in early elections may differ from those of full-term elections. The elections were also polarizing, with the vote share of the left and the right in the control increasing at the expense of center parties (see Figure 3). In that sense our results may be more germane to polarized and less stable political environments.

⁶Within our sample, in an OLS regression of ordered vote choice in 2015 on pre-treatment indices of individual attitudes towards peace concessions and towards economic policies, both indices are highly significant, with an R^2 of 0.296. However, of this R^2 , the peace index is responsible for 94.1%, while the economic policy index only accounts for 5.4%.

ration of the security situation. In contrast, the left (led in 2015 by *The Zionist Union* party) sees status quo policies, including permitting settlements in the West Bank, as already costly and likely to put Israel's security and democracy at further risk. Instead it favors restarting the peace process with the goal of finding a permanent solution to the conflict. Finally, while many Israeli parties can be clearly classified as left or right based on this dimension, other parties—which we will refer to as *center*—tend to focus on different issues and are widely seen as potential members of a coalition led by either the *Likud* or by *The Zionist Union*. These include the religious ultra-orthodox parties *Shas* and *Yahadut HaTorah*, as well as parties focused on civic and economic issues, *Yesh Atid* and *Kulanu*.

Of the three largest parties in our data, the ruling party, the *Likud*, won 23% of the vote in 2015. Though it did not publish an explicit platform in 2015, the *Likud* has been in power since 2009 and is strongly associated with the status quo and skepticism towards the peace process. The day before the elections, on March 16, 2015, *Likud* leader and Prime Minister Benjamin Netanyahu, argued that “Whoever moves to establish a Palestinian state or intends to withdraw from territory is simply yielding territory for radical Islamic terrorist attacks against Israel”, and stated that he would not allow a Palestinian state if elected (Reuters, 2015).

The second major party, *The Zionist Union*, combined the historic Labour party with the smaller, more centrist *Hatnuah* party. *The Zionist Union* won 19% of the 2015 vote. Its 2015 platform stated that “reaching a diplomatic settlement [of the conflict] is a foremost Israeli interest and a necessary condition for securing [Israel's] future as a Jewish and democratic country, enjoying widespread international support.” The platform further committed to restarting negotiations “with the aim of reaching a permanent settlement with the Palestinians, based on the principle of two states for two peoples” (The Zionist Union, 2015, p. 5).

The third party is the *Yesh Atid*, which won 9% of the vote. In 2015 *Yesh Atid* focused primarily on economic and civic issues that appeal to the secular middle class, with much less emphasis and no clear position on the conflict.⁷

⁷Whereas the *Zionist Union*'s platform opens with detailed specific national security and peace initiatives, *Yesh Atid*'s 2015 platform begins with corruption, followed by chapters on the cost of living, housing, education, health and welfare. National security is discussed in Chapter 8 (just before small businesses). The chapter lists the threats Israel faces but does not commit to a clear policy, concluding that “Israel needs to develop a comprehensive national security conception, based on the development and reinforcement of military, political and economic power resources – and a proactive and active foreign policy – and act accordingly” (Yesh Atid, 2015, p. 110). Regarding the remaining parties: *Meretz* and the *Arab Joint List* are both clearly to the left of the *Zionist Union* in terms of support for peace negotiations and willingness to withdraw from territories occupied in 1967. At the other extreme, *Haam Itanu* and *Habayit Hayehudi* are highly supportive of Jewish settlements in the occupied territories and

3 Experimental Design

3.1 Population

Our population consists of Jewish Israeli citizens who voted in the past and who participate in an internet panel that includes about 60,000 participants. The panel is nationally representative in terms of age and sex, and is commonly used for commercial market research, political polling and academic studies. We over-sampled non-orthodox center voters (i.e. people who voted for *Yesh Atid*, *Hatnuah* or *Kadimah* in 2013) at twice their vote share, as these are considered Israel’s swing voters.

1418 individuals completed our two baseline surveys. We screened out those who provided incomplete or inconsistent answers or who did the surveys extremely quickly (see Figure B1 for details). This left 1345 participants to be randomly assigned to the various treatments and the control group. This sample approximates the broader Jewish population of Israel in terms of geographical region and sex, but tends to be somewhat more educated and secular, with fewer individuals over the age of 55 and in the top-most income deciles (Table A1). To help assess how this may affect the interpretation and generalizability of the results, we will below discuss the treatment effects by various subpopulations.

1036 participants were assigned to trade assets. Of these, 840 completed the instructions session and agreed to continue. The incomplete takeup probably reflects some self-selection and differential willingness to hold different assets, as we discuss below. We thus took special care to survey the outcomes of non-takers so we can estimate both Treatment on the Treated and more conservative Intent to Treat effects.

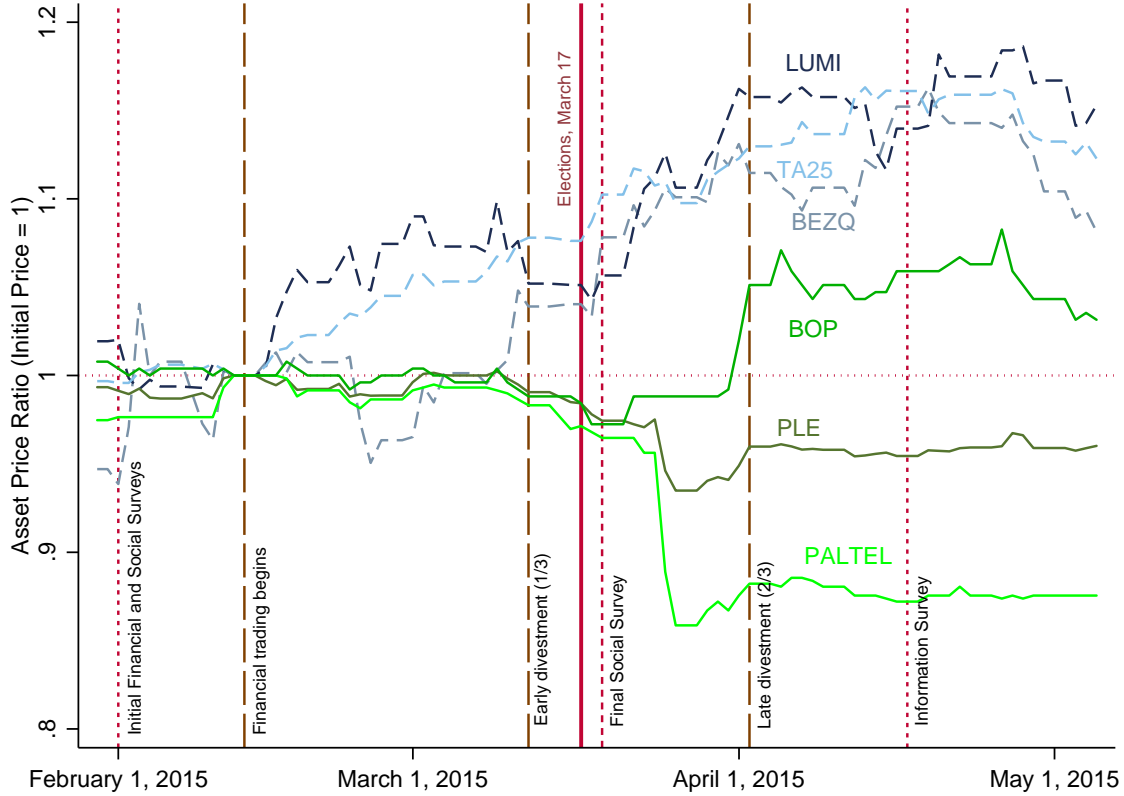
Post-treatment, we observe the vote choice of 1311 individuals (97.4% of the treatment group and 97.7% of the control). The response rate on questions measuring attitudes towards the peace process is 95% (1277/1345). Table A4 provides attrition rates for the other surveys, and Table B12 provides additional balance checks.

3.2 Sequence

Individuals were invited to a study on investor behavior that would include several surveys on various issues (the invitation and survey instruments are available on our websites). They were told they would enter a lottery to win either a financial asset or an initial

oppose any withdrawals, and as such are to the right of the *Likud*. Finally, Avigdor Lieberman’s *Israel Beitenu* is harder to place as, occasionally, Lieberman positions himself as a centrist on some issues. Nonetheless he has always been either part of a right wing coalition or opposing it from the right. Classifying *Israel Beitenu* as center does not affect our results.

Figure 1: **Experimental Sequence and Asset Prices**



The study included the following surveys: 1. Initial Financial Survey (Fielded Feb 1, 2015); 2. Initial Social Survey (Feb 3, 2015); 3. Weekly Financial Surveys (with investment decisions, several); 4. Final Financial Survey (at divestment, either March 12 or April 2); 5. Final Social Survey (March 19, 2015); 6. Information Survey (April 16, 2015); 7. Financial Followup Survey (July 19, 2015); 8. Social Followup Survey (April 12, 2016). Israeli stocks shown in dashed blue lines (Bezeq Telecoms (BEZQ), Bank Leumi (LUMI) and the Tel Aviv 25 (TA25)). Palestinian stocks in solid green (Palestine Telecoms (PALTEL), Bank of Palestine (BOP) and the Palestinian General Market Index (PLE)).

voucher of cash to invest in a financial asset, and that these assets would track the value of specific stocks from the region.⁸ Among those that consented to participate, we conducted two parallel sets of surveys: financial and social. The financial surveys included questions on prior investment history (including whether they had traded stocks in the last six months), and a battery of questions measuring financial literacy, risk aversion and time preference. The social surveys included questions on political behavior, social and political attitudes, and well-being. Figure 1 shows the timeline of the experiment.

Participants assigned to the treatment group were further invited to an online in-

⁸To reduce social desirability biases, each individual had some chance of being assigned stocks from Cyprus, Egypt, Jordan and Turkey in addition to Israeli and Palestinian stocks.

structions session in which they were informed of their asset allocation (Figure B3 shows a screenshot), given detailed explanations about the rules, and quizzed to make sure they understood how the value of their assets would be determined. Those who completed the session and agreed to continue received weekly updates about the price of their assigned asset and a statement of the composition and current value of their financial portfolio. We also provided links to the Hebrew version of *investing.com* to allow individuals to independently track and verify the historical performance and current price of their stocks. Invitations were sent out after markets closed on the last business day of the week (usually on Thursdays). Every week, participants could reallocate up to 10% of their holdings by buying or selling a particular financial asset, commission-free. This was to encourage continuous engagement with the stock market rather than simply choosing the entire portfolio (or selling all stocks) immediately. Furthermore, participants who did not enter a decision lost the 10% that they could have traded that week. They could decide to neither sell nor buy, but they had to enter a decision to avoid the loss. All trades were implemented via a trading platform incorporated into our surveys.⁹

Two days after the elections we surveyed all individuals on their vote and political attitudes. This provided data on the vote choice of 1291 participants. We were able to augment and compare the voting data to our own Information Survey in April 2015 as well as using participants' routine updates to the survey company on their demographic and voting data (July 2015). There were few discrepancies.¹⁰ As a result, we benefit from very little attrition in our main outcome variable.

3.3 Treatments

Table 1 shows the treatment assignment. 309 participants were assigned to the control group, and 1036 were assigned to the treatment group.¹¹ To help disentangle the effects

⁹In particular: once the markets closed, we calculated for each individual: (1) the current number of stocks they own given previous trading decisions, (2) the value of these stocks given current prices and (3) the amount of cash at their disposal. We then informed them of their trading possibilities, namely how much they could buy (depending on the amount of cash at their disposal) and how much they could sell (depending on the amount of stocks owned). All trades were implemented at the current price, which was constant during the decision window. Figure B4 shows a sample trading screen. 69% of the 840 compliers entered a trading decision at every opportunity they had and 80% did so in all but one week.

¹⁰Of the 1040 participants who answered both our post election survey and the survey company's, 95.6% reported voting for the same party in both. The probability of reporting the same vote is uncorrelated with the treatment.

¹¹We employed a stratified block randomization procedure designed to increase balance across treatment groups in political and demographic variables. Specifically, we create 104 blocks of 13 (less for one block), stratified sequentially on: 2013 vote choice (with parties ordered from left to right), sex, a dummy for whether the individual traded stocks in the last 6 months, a dummy for whether the individual would recommend to a friend to invest in stocks from Arab countries, geographical region,

Table 1: **Assignment to Treatments**

	<u>Total</u>	<u>Redeem pre-elections</u>			<u>Redeem post-elections</u>		
		All	Low (\$50)	High (\$100)	All	Low (\$50)	High (\$100)
Treatment	1036						
Israeli Stocks	414	141	70	71	273	136	137
Palestinian Stocks	416	141	71	70	275	137	138
Voucher to Invest	206	64	32	32	142	71	71
Control	309						

of material incentives generated by holding stocks on election day from the effects of exposure to financial markets, a third of the treatment group were fully divested of their assets the weekend prior to the March 17 elections. The others could continue to trade in their assets until two weeks after the elections. We further varied the initial value of the endowment to be either NIS 200 (around \$50) or NIS 400 (around \$100). While these sums are not large (the average Israeli daily wage in December 2014 was NIS 312), they are significant compared to typical stakes in experimental economics, or to the standard pay of NIS 0.1 per question paid by the survey company.

In terms of the assets individuals could trade, 830 individuals were assigned a specific stock or index fund that they could sell (and later buy back). Of these, 414 were assigned assets from Israel, randomly distributed between the *Tel Aviv 25* Index, a commercial bank (*Bank Leumi*) and a telecoms company (*Bezeq*). The other 416 were assigned assets from the Palestinian Authority, similarly distributed between the Palestine Stock Exchange General Index, a commercial bank (*Bank of Palestine*) and a telecoms company (*PALTEL*).¹² These sub-treatments were meant to compare the effect of exposure to in-group vs. out-group assets. However, as it turned out, all the Israeli assets have outperformed the Palestinian assets during the time of the intervention (Figure 1). As we discuss below, this confounds that comparison.

Finally, 206 individuals were assigned vouchers they could use to buy (and later sell) a specific index fund (funds not used for buying the index earned zero return). The vast majority (202) of them could trade in the Tel-Aviv 25 Index. In addition, four traded

discrepancies in their reported voting in the 2013 elections and a measure of their willingness to take risks. This generates relatively homogeneous blocks. Within each block we then randomize individuals into the treatments.

¹²The assets were in fact a derivative claim on the authors' research funds rather than an actual purchase of the underlying asset. Thus the study could not affect asset prices even if very thinly traded. The specific companies were selected along two criteria: lack of overt connection to the Israeli-Palestinian peace process and comparability. As the Palestinian and other foreign assets were listed in foreign currency such as Jordanian Dinars, we fixed the exchange rate for the duration of the experiment so that there was no exchange rate risk for non-Israeli stocks.

for indices from Cyprus, Egypt, Jordan and Turkey.¹³ Both the stocks and the voucher groups traded on the same platform and received the same information. However, given our 10% limit on weekly trading, stock holdings up to and including election day would necessarily be greater for those endowed with stocks. Furthermore, the main trading possibility in the voucher group was to buy stocks while in the stocks group it was to sell.

3.4 Minimizing experimenter demand effects

Experimenter demand effects are potentially an important issue.¹⁴ We included three features to minimize the chance that participants would associate the social surveys with the investment and financial surveys. First, our surveys were among 110 sent to panelists by anonymous sources during February and March 2015. Second, we avoided any questions related to the elections or the Israeli-Palestinian conflict in the financial surveys, and similarly avoided any financial questions in the social surveys. Third, the assets participating in the study were broad indices or stocks of banks and telecoms companies, rather than companies with extensive business in the West Bank or with overt ties to national defense.¹⁵

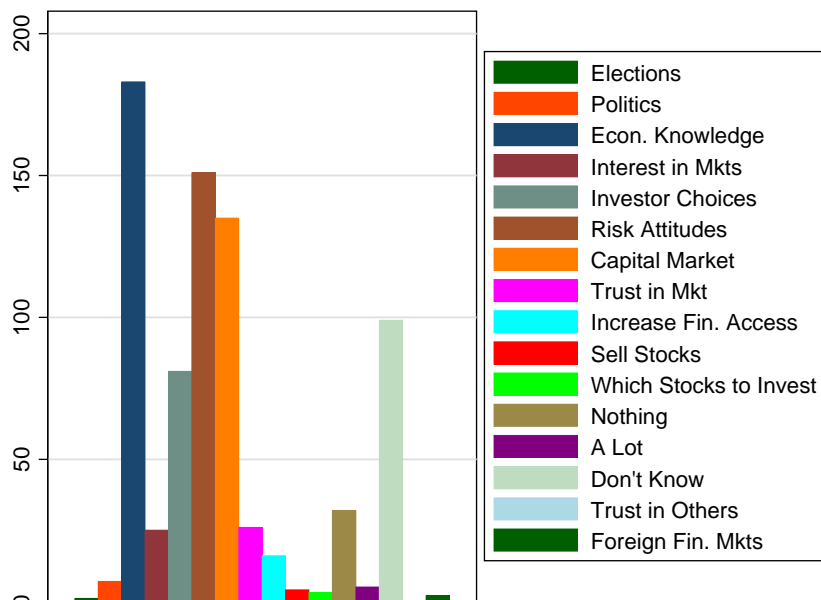
To verify whether these measures were effective, an open-response question at the end of the trading period (March 12 or April 2) asked: “What do you think the researchers can learn from the study?” The results are in Figure 2. Despite the surveys running around the time of the polls, only one respondent mentioned the elections and only seven mentioned any other relationship to politics. Of these, six thought the study could inform how political views affect investment behavior, rather than the reverse. The modal responses were that the study was about gauging economic knowledge, risk attitudes, capital market behavior and investor choices. These are accurate responses given that we study these as well (Jha and Shayo, 2019).

¹³We included these four indices to be consistent with the information provided to participants, that the stocks participating in the study are from the entire region (see footnote 8).

¹⁴See Podsakoff et al., 2003 for a discussion of common biases in this class. Existing methods of addressing this problem include filler questions to distract individuals from the purpose of the study; list experiments; and proxy outcome measures (like the Implicit Association Test) that are considered less susceptible to conscious processes.

¹⁵The only defense company in the Tel Aviv 25 (TA-25), Elbit Systems, had a weight of 3.26%. Note also that we would expect the effect of a stock market intervention on support for peace initiatives to be weaker if we give large stakes in companies that benefit from conflict, or if we endow individuals with large short positions of the broader index.

Figure 2: **What can the researchers learn from this study?**



4 Data

Table 2 compares the treatment and control groups across a broad range of pre-treatment characteristics. We restrict attention to the 1311 individuals for whom we have the 2015 vote outcome.¹⁶ Column 3 reports the raw mean difference while Column 5 reports mean differences within the 104 stratification bins. As expected from random assignment with low attrition, for almost all variables there are no significant differences across treatment and control. Most importantly, we know how individuals voted just two years prior to the 2015 elections that we study. As the top two rows show, about 24% of our sample voted for right parties and about 13% voted for left (pro-peace process) parties in 2013, with similar proportions across treatment and control groups. Figure 3 (top left) shows balance party-by-party.

Attitudes towards making concessions for peace at baseline, and attitudes towards left or right economic policies—measures that we describe in more detail below—are also similar across treatment and control. Around 36% of our sample in both the treatment and control groups reported having traded stocks in the six months prior to the experiment. The groups are also balanced by basic demographic characteristics, including sex, marital status, education, religiosity, geographical location and income. The groups have similar time preferences (based on standard hypothetical choices) and similar financial

¹⁶A comparison of this sample to the general Israeli population is in Table A1.

Table 2: Descriptive Statistics and Balance Tests

	Mean		Difference in Means				Obs.
	Treatment	Control	Without FEs		With Strata FEs		
			Diff.	P-value	Diff.	P-value	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Voted Right '13	0.241 [0.428]	0.245 [0.431]	-0.004 (0.028)	0.881	0.000 (0.006)	0.964	1,311
Voted Left '13	0.137 [0.344]	0.126 [0.332]	0.011 (0.022)	0.625	0.005 (0.004)	0.213	1,311
Peace Index	0.051 [0.823]	0.004 [0.784]	0.047 (0.053)	0.378	0.038 (0.044)	0.399	1,311
Economic Policy Index	0.007 [0.574]	-0.005 [0.596]	0.012 (0.038)	0.757	0.011 (0.036)	0.752	1,311
Bought/Sold Shares in Last 6 Mths [0/1]	0.355 [0.479]	0.368 [0.483]	-0.013 (0.031)	0.686	-0.018 (0.017)	0.290	1,311
Male	0.521 [0.5]	0.513 [0.501]	0.008 (0.033)	0.806	0.009 (0.012)	0.470	1,311
Age [Yrs]	39.289 [13.394]	41.530 [14.293]	-2.240 (0.892)	0.012	-2.142 (0.844)	0.011	1,311
Post Secondary Education	0.230 [0.421]	0.232 [0.423]	-0.002 (0.028)	0.946	0.002 (0.027)	0.953	1,311
BA Student	0.148 [0.355]	0.152 [0.36]	-0.005 (0.023)	0.842	-0.005 (0.024)	0.834	1,311
BA Graduate and Above	0.426 [0.495]	0.427 [0.495]	-0.001 (0.032)	0.976	-0.005 (0.031)	0.860	1,311
Married	0.598 [0.491]	0.629 [0.484]	-0.032 (0.032)	0.326	-0.033 (0.031)	0.295	1,311
Religiosity: Secular	0.627 [0.484]	0.636 [0.482]	-0.008 (0.032)	0.791	-0.014 (0.025)	0.582	1,311
Traditional	0.164 [0.37]	0.172 [0.378]	-0.009 (0.024)	0.723	-0.005 (0.024)	0.823	1,311
Religious	0.124 [0.33]	0.119 [0.325]	0.005 (0.022)	0.828	0.005 (0.018)	0.780	1,311
Ultra- Orthodox	0.085 [0.279]	0.073 [0.26]	0.012 (0.018)	0.493	0.014 (0.012)	0.222	1,311
Region: Jerusalem	0.091 [0.288]	0.096 [0.295]	-0.005 (0.019)	0.799	-0.004 (0.017)	0.800	1,311
North	0.097 [0.296]	0.089 [0.286]	0.008 (0.019)	0.689	0.009 (0.017)	0.595	1,311
Haifa	0.142 [0.349]	0.123 [0.328]	0.019 (0.023)	0.395	0.021 (0.020)	0.291	1,311
Center	0.290 [0.454]	0.298 [0.458]	-0.008 (0.030)	0.798	-0.007 (0.023)	0.766	1,311
Tel Aviv	0.194 [0.396]	0.212 [0.409]	-0.018 (0.026)	0.500	-0.024 (0.022)	0.276	1,311
South	0.104 [0.305]	0.116 [0.321]	-0.012 (0.020)	0.560	-0.010 (0.018)	0.596	1,311
West Bank	0.081 [0.273]	0.066 [0.249]	0.015 (0.018)	0.392	0.015 (0.016)	0.341	1,311
Monthly Family Income [NIS]+	10996 [5,567]	11162 [5,324]	-165.192 (365.176)	0.651	-231.199 (352.004)	0.511	1,286
Willing to Take Risks [1-10]	4.716 [2.265]	4.344 [2.24]	0.371 (0.148)	0.012	0.366 (0.139)	0.009	1,311
Time preference median or above	0.657 [0.475]	0.642 [0.48]	0.015 (0.031)	0.638	0.014 (0.031)	0.645	1,311
Financial literacy: % correct	70.664 [23.359]	69.726 [23.917]	0.938 (1.541)	0.543	0.870 (1.455)	0.550	1,311

Notes : Standard deviations in brackets in columns 1-2. Standard errors in parentheses in columns 3,6. Each entry in Columns 3-4 and 5-6 is derived from a separate OLS regression where the explanatory variable is an indicator for treatment group. Columns 5-6 control for 104 randomization strata fixed effects. +: mid-point of SES income categories.

literacy scores. Two variables show small but statistically significant differences. Individuals in the treatment group are somewhat younger on average (39.3 vs 41.5 years old) and slightly more willing to take risks. We control for these and other demographic variables in our regressions (including a quadratic for age).¹⁷

5 Main Results

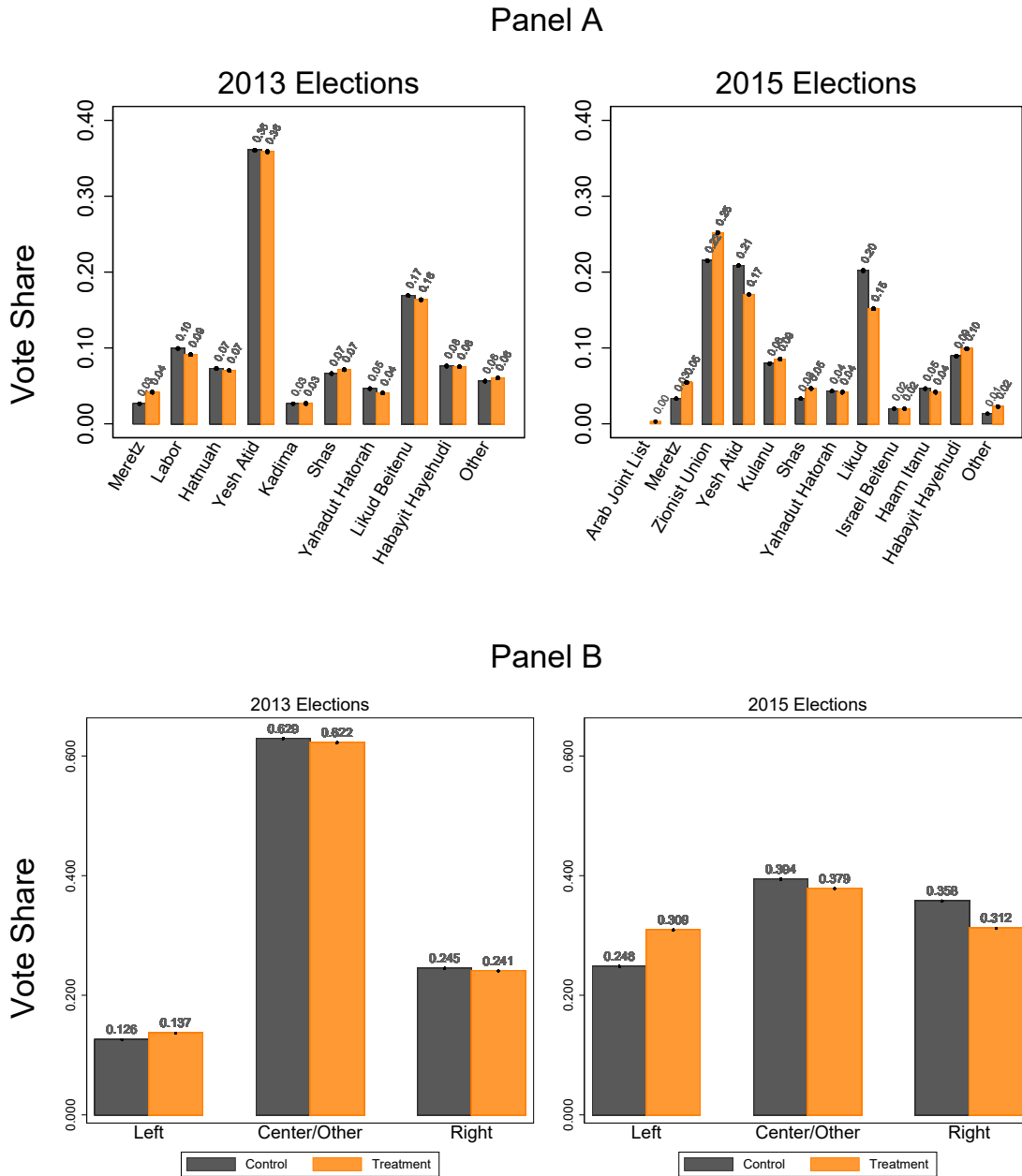
Does exposure to financial markets change political choices? Figure 3 shows the raw vote shares across the treatment and control groups. Panel A shows the detailed party vote while Panel B combines the parties into blocks (see Section 2 for party positions). The histograms to the left show vote shares in the 2013 elections (prior to our intervention). Notice the over-sampling of the center parties *Hatnuah*, *Yesh Atid* and *Kadima*. These three parties won, respectively, 5%, 16% and 2% of the overall votes for non-Arab parties in 2013, but are represented at about twice those shares, totalling 45% of our sample. Note also that the treatment and control groups have almost identical distributions of votes across parties in 2013.

However, voting decisions in 2015 (post-treatment) reveal substantial differences (Panel A, right). The right-wing ruling Likud party won 20.2% of the votes in the control group but only 15.2% in the treatment group. In contrast, the main left-wing party, The Zionist Union, won 21.5% of the votes in the control but 25.2% of the votes among the treated. In the center, Yesh Atid won 21% of the votes in the control but 17% in the treatment group. Panel B shows the same result consolidating parties into Left and Right blocks. Within the control group, 24.8% voted Left (a proportion similar to the 25.3% share of the 2015 vote for Jewish left parties in the Israeli population as a whole), but this share increases to 30.9% in the treatment group. At the same time, right parties won 35.8% of the votes in the control group, but only 31.2% in the treatment group.

Table 3 estimates the treatment effect on the probability of voting for left (Cols 1-4) and right parties (Cols 5-8) in 2015. We mostly report conservative Intent to Treat (ITT) estimates. We use robust standard errors: the clustering problem does not arise in our setting as we randomize at the individual level. Columns 1 and 5 replicate the raw

¹⁷These slight age differences actually work *against* the main effect, as, unlike in the US, younger voters in Israel are *less* likely to vote for the left (see also Table B8). Similarly, as we show below, the effects are *stronger* for the *risk-averse*. To further check whether the number of significant differences might indicate a potential problem with the realization of our randomization procedure, we do the following. We randomly assign the sample of 1311 individuals in Table 2 to fictitious treatment and control groups, with the same proportions as those of the actual groups. We then perform the tests from Cols 3-4 and count the significant differences, repeating this procedure 500 times. Less than 6% of the simulations have zero significant differences and less than 28% have less than two (the number we obtain). See also Figure B5.

Figure 3: Vote in Treatment and Control Groups in 2013 and 2015



N=1311. The 'other' bars include 71 and 17 individuals who voted for other parties in 2013 and 2015, respectively, as well as 1 and 27 individuals who did not vote in 2013 and 2015, respectively.

Party Blocks in Panel B: **2013:** Left includes: *Meretz* & *Labour*. Center: *Hatnuah*, *Yesh Atid*, *Kadima*, *Shas* & *Yahadut HaTorah*. Right: *Likud Beitenu* and *Habayit Hayehudi*. **2015:** Left: The *Zionist Union*, *Meretz* & the *Arab Joint List*. Center: *Yesh Atid*, *Kulanu*, *Shas* and *Yahadut HaTorah*; Right parties: *Likud*, *Israel Beitenu*, *Haam Itanu* & *Habayit Hayehudi*.

Table 3: Treatment Effects on Left and Right Vote in 2015

	Vote for Left Party in 2015				Vote for Right Party in 2015			
	ITT	ITT	ITT	TOT	ITT	ITT	ITT	TOT
	reweighted				reweighted			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.061 (0.029)	0.059 (0.023)	0.043 (0.020)	0.073 (0.027)	-0.045 (0.031)	-0.044 (0.024)	-0.051 (0.027)	-0.054 (0.028)
Voted Right '13		-0.254 (0.091)	-0.201 (0.083)	-0.272 (0.089)		0.492 (0.122)	0.473 (0.127)	0.505 (0.114)
Voted Left '13		0.596 (0.091)	0.614 (0.090)	0.608 (0.085)		-0.222 (0.088)	-0.249 (0.088)	-0.231 (0.087)
Bought/Sold Shares in Last 6 Mths [0/1]		0.018 (0.040)	0.015 (0.035)	0.015 (0.039)		0.030 (0.040)	0.024 (0.043)	0.032 (0.039)
Traditional		-0.138 (0.032)	-0.155 (0.029)	-0.133 (0.031)		0.102 (0.032)	0.128 (0.036)	0.099 (0.030)
Religious		-0.166 (0.032)	-0.162 (0.031)	-0.165 (0.030)		0.241 (0.049)	0.232 (0.049)	0.240 (0.046)
Ultra-Orthodox		-0.221 (0.039)	-0.208 (0.037)	-0.222 (0.038)		0.056 (0.086)	0.033 (0.088)	0.057 (0.082)
Post Secondary		0.068 (0.033)	0.063 (0.027)	0.066 (0.032)		-0.060 (0.034)	-0.046 (0.037)	-0.059 (0.032)
BA Student		0.088 (0.038)	0.072 (0.032)	0.088 (0.036)		-0.041 (0.039)	-0.025 (0.042)	-0.041 (0.037)
BA Graduate & Above		0.062 (0.030)	0.038 (0.026)	0.062 (0.029)		-0.044 (0.032)	-0.021 (0.035)	-0.045 (0.030)
Willing to Take Risks [1-10]		-0.001 (0.005)	0.002 (0.004)	-0.001 (0.005)		0.007 (0.005)	0.008 (0.005)	0.007 (0.005)
Time preference above median		0.012 (0.022)	0.009 (0.018)	0.010 (0.020)		0.004 (0.021)	0.004 (0.024)	0.005 (0.020)
Financial Literacy, %Correct		0.000 (0.000)	0.000 (0.000)	0.000 (0.000)		-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.000)
Strata FE	NO	YES	YES	YES	NO	YES	YES	YES
Demographic Controls	NO	YES	YES	YES	NO	YES	YES	YES
F(excluded instruments)				3129				3129
Observations	1,311	1,311	1,311	1,311	1,311	1,311	1,311	1,311
R-squared	0.003	0.447	0.570	0.443	0.002	0.518	0.556	0.518

Notes: OLS (ITT) and 2SLS (TOT) estimates of the treatment effect on the probability that an individual voted for a left or right party in 2015.

Robust standard errors in parentheses. 2SLS estimates use assignment to treatment as instrument. Data in Cols 3,7 are reweighted to represent the vote share of Jewish parties in 2013. Cols 2-4, 6-8 include fixed effects for 104 blocks constructed to stratify sequentially on: 2013 vote, sex, traded stocks, would recommend Arab stocks, geographical region, discrepancies in 2013 vote across surveys, and subjective willingness to take risks.

'Demographic controls' include sex, age, age squared, four education categories, marital status, six regional dummies, four religiosity categories, five income categories (and a dummy for missing), time preference above the median, financial literacy score and subjective willingness to take risks.

mean differences in Figure 3-B. Columns 2 and 6 control for other factors that may shape vote choices (all measured pre-treatment): vote in the 2013 elections, prior experience in trading stocks, sex, age, education, income, religiosity, geographical region, marital status, willingness to take risks, patience, financial literacy, as well as 104 strata fixed

effects. These controls are meaningful determinants of voting: the R^2 increases from 0.003 to 0.447 for voting Left, and from 0.002 to 0.518 for voting Right. Consistent with random assignment, however, the mean treatment effects are essentially unaffected. They again indicate a 6pp increased probability of voting for the left and a 4.4pp reduction in the probability of voting for the right (p -values 0.011 and 0.066, respectively).

The magnitude of the effects may be accentuated by the fact that we over-sampled centrist voters that are more likely to change their voting decisions. Columns 3 and 6 re-weigh the sample to match the actual vote share of Jewish parties in 2013. The point estimate is smaller for the probability of voting left (a 4.3 pp increase), but larger (a 5.1 pp decrease) for voting right. This is consistent with the fact that the treatment mostly moves individuals over by a single block: from the right to the center, and from the center to the left (see transition matrices in Table B1). Reducing the relative weight on *ex-ante* centrist voters, puts less weight on those that move from the center to the left and more on those that move from the right to the center.

Finally, we measure the treatment effect on those individuals who actually completed the instructions session and accepted their assigned assets. Columns 4 and 8 present estimates of the treatment effect on the treated (TOT), using assignment to treatment as an instrument for participating. Not surprisingly, the TOT estimates are larger than the ITT, suggesting that for treated individuals the probability of voting left increased by 7.3pp and the probability of voting right declined by 5.4pp.

Appendix Table B2 estimates the treatment effect party by party. Table B3 reports multinomial logit estimates. Consistent with the raw data in Figure 3, the treatment significantly increases the likelihood of voting for the main left-wing party, *The Zionist Union*, by 3.7 to 5.3 pp in the ITT and TOT specifications, respectively. It significantly reduces the likelihood of voting both for the main right-wing party, the *Likud* (by 4-5 pp) and the centrist *Yesh Atid* (by 3-4 pp). Again, reweighing the sample strengthens the negative effect on the *Likud* and attenuates the positive effect on the *Zionist Union*. There is no appreciable effect on turnout, perhaps because our sample consists of past voters.

Henceforth, we summarize the voting decision in a single ordered vote choice variable, paralleling the blocks in Figure 3-B. This will be useful for studying mechanisms. We normalize the values to range from 0 for Right, 0.5 for Center or Other, and 1 for Left, for comparability with the binary outcomes. Treatment effects using this measure are reported in Table B4. The linear effects on the ordered vote choice reveal a 0.052 leftward shift in the unweighted ITT, 0.047 in the reweighted ITT and 0.064 in the TOT (p -values 0.006, 0.013, 0.004, respectively). The effects tend to be higher when restricting attention

to those without experience trading in stocks in the six months prior to the experiment.¹⁸

The Appendix includes two useful robustness checks. First, we exploit the fact that we observe voting before the experiment, in 2013, and after, in 2015, to examine within-individual changes in voting behavior over time. Table B5 reports the results of this difference-in-difference exercise. The effect on the ordered vote choice is unaffected by the inclusion of either individual controls or individual fixed effects (Cols 1-3). Second, in Table B7, we take out voters of each of the 2013 parties, one party at a time. The treatment effect is not driven by the voters of any one particular party.

Looking at heterogeneous effects (Table B8), we find that the effect on the vote is similar across gender and education groups and appears stronger for older participants. Looking across regions (Table B9 and Figure A1) reveals that the point estimates are positive throughout the country, with two notable exceptions: the point estimates are zero in the West Bank (i.e., among Jewish settlers) and in the Jerusalem region, which both includes territories occupied in 1967, and where religiosity is very high. Indeed, as we show in Table B8, the treatment mainly affects secular and traditional voters, and has a weaker and statistically insignificant effect on the votes of the religious and ultra orthodox. This is not surprising, since the latter overwhelmingly vote for ethnic and religious parties. As we will show, however, even though the treatment does not change *voting* in these groups, it does affect their attitudes towards the peace process.

6 Mechanisms

So far we have demonstrated that exposure to financial assets moves voters towards parties that are more supportive of the peace process. This result is important in and of itself, and appears to confirm Montesquieu’s conjecture discussed in the introduction. In this section we exploit the rich set of sub-treatments and attitudinal measures to try to shed more light on the *mechanisms* through which this occurs. The analysis is exploratory in nature, but may offer valuable insights. We defer discussion of the effects of Israeli vs Palestinian stocks to Section 8.

The Appendix reports balancing tests across sub-treatments. As before, sub-treatments are balanced relative to the control on almost all dimensions (Table A2). The sub-

¹⁸Since we stratified on past experience, the strata fixed effects in Table 3 absorb much of the relationship between past financial experience and vote choice. When we remove the strata fixed effects (Table B6), two patterns emerge. First, even without the treatment, those with past experience in the financial markets were 9-10pp more likely to vote for a left party in 2015, but not less likely to vote for the right. Second, the estimated treatment effect on inexperienced traders tend to be larger, and mimic these patterns. Thus, it appears that the treatment leads those inexperienced in financial markets to become more like experienced traders in their political choices.

treatments are also well balanced between themselves (e.g. late divestors vs early divestors; high initial endowment vs low initial endowment, etc., see Table A3).

6.1 Economic incentives or changes in policy preferences?

Peace overtures tend to raise both Israeli and Palestinian asset prices (Zussman, Zussman, and Nielsen, 2008). Thus, even if holding stocks does not lead individuals to change their views on any policy issues, it may still provide a direct *pocketbook* incentive to vote for parties that favor the peace process.¹⁹ In Table 4 we employ three strategies that give us exogenous variation in the degree of asset exposure on election day. First we separate the effect on individuals who were exogenously divested in the week *prior* to the elections from those who divested after (Cols 1-2). The effect on those already divested is actually 0.039 *higher*, not lower.²⁰ Next we compare individuals initially assigned a portfolio purely of stock to those initially given vouchers with which to buy stock. Given our trading restrictions, the former held at least 66% of their assets in stock on election day, compared to 35% at most for those endowed with vouchers. However, the coefficient on the voucher treatment is statistically insignificant (Col 3). Finally, we examine the effects of the actual asset holdings of each participant on election, including instrumenting for election-day asset holdings based on the portfolio of a passive investor who registers a decision every week to hold her initial asset allocation (Col 4-5). However, there is no evidence for an additional effect of actual stock holdings beyond the average treatment effect.²¹

Thus the voting results do not appear to reflect any direct material incentives generated by the stocks we provided. Two points are worth stressing, however. First, our results do not rule out the possibility that larger financial stakes could induce direct incentives to change political behaviour. Second, the incentives generated by the stock positions we provide could be either undone by participants trading outside the experiment or they may even be augmented as individuals become more familiar and engaged with stock markets. Similarly, individuals exogenously divested by election day might

¹⁹Even within the period of experimental trading leading up to the elections, changes in opinion polls that predict a 1% increase in the right vote share are associated with a 1.59% fall in the asset prices of our participating Israeli stocks (Table B15).

²⁰In Appendix A.1, we unpack this further to find that those that had realized losses in the stock market prior to the election were more likely to change their voting decision than those still invested but with only paper losses, a pattern consistent with an increased sensitivity to risks among those with realized losses (Imas, 2016).

²¹In Appendix A.2 we examine two related mechanisms. One possibility is that receiving a financial portfolio worth \$50 or \$100 might itself have a *wealth effect*. Receiving a portfolio could also affect well-being or increase stress that might change voting decisions. We examine these possibilities using a range of approaches and find no supportive evidence.

Table 4: **Effects of Election Day Stockholdings on Ordered Vote Choice**

	OLS	OLS	OLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)
Treatment	0.052 (0.019)	0.038 (0.020)	0.045 (0.019)	0.057 (0.020)	0.059 (0.020)
Divest Before Election		0.039 (0.019)			
Voucher Treatment			0.033 (0.022)		
Stock value- actual on election day (100s NIS)				-0.005 (0.006)	-0.006 (0.007)
F(excluded instruments)					1100
Strata FE	YES	YES	YES	YES	YES
Demographic Controls	YES	YES	YES	YES	YES
R-squared	0.549	0.550	0.550	0.549	0.549
Observations	1,311	1,311	1,311	1,311	1,311

Notes: Dependent variable is vote choice, ordered from Right (0), Center/Other (0.5) to Left (1). Col 5 provides IV-2SLS estimates, instrumenting for the stock value on election day using the stock value of a purely passive investor who made no trades. The instrument is calculated based on the asset allocation, the redemption date (pre- or post- elections), the initial value (high or low) and the price change of the specific asset by election day. All regressions include the full set of controls from Table 3, Col 2. Robust standard errors in parentheses.

undo this treatment by investing privately. Anticipating such possibilities, we explicitly stratified on those that had traded stocks within six months prior to the experiment as they would be better positioned to undo the treatment. In the weekly investment surveys we also asked participants whether or not they had traded outside the experiment. Overall, there indeed seems to be an increase in the propensity to trade outside as the study proceeds (see Figure B7 and Jha and Shayo 2019). However, this variation explains little of the treatment effect (Figures A2 and A3).

Rather than the direct material incentives provided by stockholdings, individuals might change their vote choices if exposure to financial markets induces them to change their policy preferences over the peace process or in other domains. We therefore asked participants two sets of questions: on attitudes toward the peace process, and on economic policy. The questions on the peace process are drawn from a national survey conducted since 2003 (Smootha, 2015). They include both a broad question on their level of approval (on a four point scale) for a two-state solution, as well as on specific concessions for peace, including the 1967 borders as the borders between the two states, the splitting of Jerusalem, and the return of Palestinian refugees to the state of Palestine.²² On

²²The proportions approving of these principles in our sample closely resemble the numbers in the representative sample of the Jewish population in the most recent survey, conducted in 2013. The overall trends in the population reveal either stable or falling support for these principles between 2003-

economic policy, we include questions from the World Values Survey assessing attitudes towards income inequality and governmental responsibility for the poor. To these we add a question on the privatization of services and industries, and a question on reducing the capital gains tax on investment in the stock market. We use these questions to form both a *Peace Index* and an *Economic Policy Index*, following Kling, Liebman, and Katz (2007), where higher values indicate more of a *left* position.

Table 5 presents the overall effect of exposure to financial assets on the two indices, as well as the effect component-by-component. Overall, the treatment has a strong positive effect on the summary index of agreement with the four principles underlying a potential peace deal (Col 2). The effects appear stronger for the more specific and less widely accepted concessions. As with the effects on the vote, the point estimates tend to be more pronounced among those less experienced in financial markets prior to the experiment (Col 5). Strikingly, attitudes towards peace appear to change as much for the religious and ultra-orthodox as for secular and traditional voters (Table B8 Cols 2,5).

In contrast, the overall effect on the economic policy index is insignificant, and if anything slightly negative, indicating that financial market exposure moved individuals slightly rightwards on these issues. This comes mainly from a change in policy preferences towards increased individual—rather than governmental—responsibility for addressing poverty.

We also assess whether the exposure to financial markets affected preferences over inter-ethnic inclusion. Specifically, we assessed individuals’ acceptance of cooperating and interacting with Israeli Arabs in political, social and business domains (Table B18). While the point estimates of the average treatment effect are positive on all three domains, the effect is statistically significant only for the political domain (the acceptance of Arab parties in the governing coalition).

To summarize, the effect of financial market exposure on voting decisions appears to reflect a change in sociotropic preferences over policy rather than direct material incentives, and the change in policy preferences stems chiefly from attitudes towards the peace process (and potentially inter-ethnic inclusion), rather than economic policies.

4 and 2013. Specifically, support for the two state solution among the Jewish population fell from 71.3% in 2003 and 66.7% in 2012 to 61.5% in 2013. Support for the more specific principles has been either stable or falling since 2003-4, reaching roughly the same levels seen in our data. In 2013, support for 1967 borders with land swaps was 40.3% (44.2 in 2003), for the splitting of Jerusalem it was 22.6% (23.3 in 2004) and for the return of refugees it was 48.2% (62.6 in 2003). See Smootha (2015) for details.

Table 5: Treatment Effects on Attitudes

Sample	Full Sample				Inexperienced		
	Mean	Treatment Effect	Obs.	R ² / Pseudo R ²	Treatment Effect	Obs.	R ² / Pseudo R ²
	[SD]						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Indices (OLS)							
Peace Index	0.066 [0.833]	0.110 (0.044)	1,277	0.455	0.157 (0.054)	819	0.479
Economic Policy Index	-0.019 [0.598]	-0.026 (0.041)	1,111	0.210	-0.104 (0.054)	697	0.209
Specific Outcomes (Ordered Probits):							
Two states for two peoples	2.522 [1.140]	0.101 (0.079)	1,277	0.231	0.230 (0.102)	819	0.265
1967 borders with a possibility of land exchanges	2.164 [1.083]	0.164 (0.079)	1,277	0.213	0.278 (0.102)	819	0.238
Jerusalem will be split into two separate cities - Arab and Jewish	1.822 [1.039]	0.189 (0.086)	1,277	0.206	0.213 (0.110)	819	0.238
Palestinian refugees will get compensation & allowed to return to Palestine only	2.135 [1.075]	0.194 (0.077)	1,277	0.079	0.262 (0.099)	819	0.084
Incomes in Israel should be made more equal (vs. need larger diffs as incentives).	4.249 [2.302]	-0.009 (0.076)	1,110	0.044	-0.057 (0.102)	697	0.050
Services and industries should be owned by the Government (vs. privatized).	4.530 [2.429]	0.033 (0.073)	1,111	0.052	-0.037 (0.097)	697	0.070
Government responsible for helping the poor (vs. people should take care of themselves).	3.299 [2.087]	-0.162 (0.077)	1,110	0.052	-0.291 (0.101)	696	0.062
Oppose reducing capital gains tax on investments in the stock market (vs. support).	2.652 [0.999]	0.053 (0.080)	1,104	0.073	-0.029 (0.107)	692	0.076

The top panel reports OLS (ITT) estimates of the treatment effect on attitude indices. The peace questions were asked in the March 19 survey. The economic questions were asked in the July 19 survey [The effect on the economic policy index for compliers vs control, asked March 12 (early divesters)/ April 5 (late divesters) is also negative and insignificant (-.0274 [0.039])]. The bottom panel reports ordered probit estimates of the treatment effect on the specific questions composing the indices. Col 1 provides means and standard deviations [in brackets]. Each summary index is the average of z-scores of its components, with the sign of each measure oriented so that attitudes commonly associated with the left have higher scores. The z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation (Kling et al. 2007). Robust standard errors in parentheses. All regressions include the full set of controls from Table 3 (Col 2).

6.2 Salience, Information, and Peace Dividends

We now unpack why preferences over the peace process may have changed. One plausible reason is that financial markets expose individuals to new economic considerations or make existing economic considerations more salient. To assess the latter, we asked: *“Some people say the most important issue in Israel today is the socio-economic situation. Other people say it is security and the political process. What do you think is the most important issue in Israel today?”* Answers ranged from “Only the socio-economic situation” through “Both are equally important” to “Only security and the political process”. Though the treatment effects are imprecisely estimated, the point estimates suggest that

Table 6: **Salience, Information, and the Economic Consequences of the Peace**

Sample:	All		Inexperienced	
	Mean	[SD]	Treatment Effect (SE)	Treatment Effect (SE)
A. What is the Most important Issue in Israel today? (OLS)[March 2015]				
Solely or Mainly Socio-Economic [0/1]	0.4074	[0.492]	0.039 (0.030)	0.023 (0.039)
Solely or Mainly Security and the Political Process [0/1]	0.1387	[0.346]	0.007 (0.022)	0.005 (0.027)
			1291	828
B. Economic and Political Facts (OLS) [Apr 2015]				
Political Platforms & Facts Score [Prop Correct of 13]	0.694	[0.212]	0.002 (0.013)	-0.010 (0.018)
Economic Facts Score [Prop Correct of 5]	0.533	[0.276]	0.017 (0.016)	0.020 (0.021)
Stock mkt perform. answer within 3pp of actual	0.393	[0.489]	0.066 (0.033)	0.091 (0.042)
Observations			1,238	782
C. Media Consumption (OLS) [July 2015]				
Which of the following newspapers/websites do you usually read?				
Number of financial outlets [0-3]	1.117	[1.120]	0.203 (0.074)	0.195 (0.093)
<i>TheMarker</i> [0/1]	0.338	[0.473]	0.079 (0.032)	0.086 (0.040)
<i>Globes</i> [0/1]	0.398	[0.490]	0.087 (0.033)	0.072 (0.042)
<i>Calcalist</i> [0/1]	0.380	[0.486]	0.038 (0.034)	0.038 (0.042)
Number of non-financial outlets [0-5]	1.393	[1.032]	-0.080 (0.075)	-0.135 (0.097)
<i>Haaretz</i> [0/1]	0.151	[0.358]	0.005 (0.023)	-0.028 (0.029)
<i>Israel Hayom</i> [0/1]	0.431	[0.495]	-0.052 (0.035)	-0.066 (0.045)
Observations			1,120	705
D. Consequences of a Two-State Agreement (OLS/Ordered Probits) [March 2015]				
Suppose Israel reaches a permanent agreement with the Palestinians based on the principle of two states for two peoples. How do you think this will affect... [1 (worsen a lot), 2 (worsen somewhat), 3 (no change), 4 (improve somewhat), 5(improve a lot)]				
Sociotropic Index (OLS)	0.011	[0.948]	0.041 (0.054)	0.130 (0.068)
Israel's economy? (O. Probit)	3.294	[1.329]	0.126 (0.073)	0.223 (0.094)
Israel's security? (O. Probit)	2.956	[1.392]	-0.010 (0.076)	0.097 (0.097)
Personal Index (OLS)	-0.013	[0.929]	0.003 (0.056)	0.030 (0.070)
your personal economic situation? (O. Probit)	3.048	[1.047]	-0.013 (0.077)	0.005 (0.101)
your personal security? (O. Probit)	2.888	[1.237]	-0.002 (0.075)	0.059 (0.094)
Observations			1281 / 1282	823

Notes: Treatment effects from separate regressions. Dependent variable in the first column. All regressions include the full set of controls and strata FE from Table 3, Col 2. Robust standard errors in parentheses. On March 19, 2015, we asked individuals whether the main issue in the elections was "socio-economic" or "security and the political [diplomatic] process" relative to "both" (Panel A). We also asked individuals to predict the effects of a two state solution at two levels--national and personal--and on two dimensions: security and the economy (Panel D). On April 17, we asked individuals 13 political knowledge questions, of which 2 were questions on salient events in the run-up to elections, 2 were questions on Netanyahu's public statements on the two-state solutions, 4 were questions on the positions taken by the leader of the *Zionist Union* (Herzog), and 5 were on political facts. Economic knowledge questions asked individuals to provide estimates on the unemployment rate, inflation rate, whether the stock market rose and fell and its change in value, and the change in housing prices. All answers were scored correct if they were within 3pp of the correct answer (Panel B). On July 19, we asked individuals which newspapers they usually read from among the following: *Globes*, *The Marker*, *Haaretz*, *Vesti*, *Yediot Ahronoth*, *Israel Hayom*, *Calcalist* and *Maariv*. Of these, *TheMarker*, *Globes* and *Calcalist* are financial outlets (Panel C).

treated individuals are on average 3.9pp more likely to say that the main issue is only or mainly the socio-economic situation (Table 6, Panel A). The effect is driven mainly by those that had considered both sets of issues equally important.

Drawing attention to economic considerations and risks could change political prefer-

ences through a number of channels that we now test. A key set of potential mechanisms relates to the acquisition of political information. For example, the treatment could have distracted individuals from political news and platforms, potentially making them *less informed* (Falck, Gold, and Heblich, 2014). Alternatively, individuals following financial markets may also consume more non-financial news, becoming *more informed* about politics. A third possibility is that the treatment may change not the amount of news but the *slant* of the news sources they follow (DellaVigna and Kaplan, 2007, Enikolopov, Petrova, and Zhuravskaya, 2011).

To tease out these mechanisms, a month after the elections, we designed and fielded an information survey assessing participants’ objective political knowledge on factual issues (e.g. the identity of cabinet members), on the political platforms of the leaders of the *Likud* and *Zionist Union*, and on events that took place during the election campaign.²³ However, we find no evidence that the treatment systematically raised or lowered individuals’ political knowledge of their political facts or the main platforms of the candidates (Table 6, Panel B). This is also true, looking question by question (Table B10).

Similarly, we also asked participants five questions assessing their knowledge about prevailing economic conditions, such as the unemployment and inflation rates. The treatment did not have an effect on the extent of their economic knowledge, with one notable exception: treated individuals had more accurate knowledge about the recent performance of the Israeli stock market.

This lack of enhanced political knowledge is also reflected in media consumption. In the financial followup survey in July, we asked individuals which news outlets they read regularly. There is no significant change in consumption of non-financial news (Panel C). Nor is there any difference in the *slant* of the non-financial media consumed by the treatment and control: they are as likely to read left-leaning news sources (*Haaretz*) and right-leaning outlets (Sheldon Adelson’s *Israel Hayom*). As we return to below, however, treated individuals do significantly increase the number of financial outlets that they follow.²⁴

²³These included 13 questions on the positions of the candidates (eg *what is Herzog’s position concerning the establishment of a Palestinian state as part of a political agreement?*), events during the run-up to the elections (eg *what was the main subject of Netanyahu’s Congress speech?*), and simple factual questions (eg *who was Minister of Defense in the previous government (until December 2014)?*). In the same survey we also included questions on perceptions of Netanyahu’s positions on five issues in which he has *not* expressed a consistent position. The results do not change if we add or remove those questions (Table B10).

²⁴It is possible that financial media may have a different way of covering political news. For example, consumption of *TheMarker*—a financial newspaper that is actually physically bundled with left-leaning *Haaretz* in its print form—increased by 7.9pp, a figure rivalling that of the arguably more neutral financial newspaper, *Globes* (8.7pp). However controlling for consumption of any financial news or that of each of the three financial newspapers—*TheMarker*, *Calcalist* or *Globes*—separately can explain only

Rather than changes in political knowledge, financial markets can also expose individuals to new economic considerations and new means to evaluate the returns and risks of different policies. We therefore asked individuals a set of questions on the predicted benefits or costs of a peace agreement. These included two *sociotropic* questions—how an agreement with the Palestinians would affect the economy and national security—and two questions on the effects on their *personal* economic situation and safety (see Table 6, Panel D for the wording).²⁵

Panel D shows the treatment effect on the sociotropic and personal indices, as well as on responses to each question. Individuals in the treatment group—especially among the financially inexperienced—predict greater benefits from a peace agreement for Israel, and *Israel’s economy* in particular. In contrast, the treatment has no appreciable effects on the extent to which individuals predict that they personally would benefit from a two state solution. As with the results in section 6.1 above, this is consistent with the effect being driven by *sociotropic* rather than *pocketbook* considerations.

In fact, controlling for the reassessment of the gains to Israel’s economy leads to the largest attenuation of the treatment effect on the peace index of any individual variable (Figure A3), which in turn explains the largest share of the treatment effect on the vote (Figure A2). By themselves, changes in two mechanisms— increases in salience of economic issues, and changes in the perceived returns to Israel’s economy of a peace settlement over status quo policies— can explain 30% of the average treatment effect of financial market exposure on support for peace initiatives, and together these three variables explain 40% of the treatment effect on the vote (see Appendix A.4).

Thus, the treatment (somewhat) heightens the salience of economics and changes assessments of the sociotropic economic gains from the peace process. Further, rather than a change in individuals’ objective political knowledge and consumption of other forms of news, treated individuals increase their consumption of financial news and their knowledge of stock market performance. In fact, as we document in our companion paper, Jha and Shayo (2019), at the end of the trading period, treated individuals not only self- report being more familiar and confident in interacting with the stock market and more aware of risk-return tradeoffs, their performance also improves on objective

a slight component of the treatment effects on the vote or the peace index (see Figures A2 and A3).

²⁵It is important to note that the idea that a peace agreement could benefit the economy—or at least be less harmful to the economy than it might be to security—is not an alien concept to voters. While 58% of individuals provide the exact same answer to both the sociotropic questions, 33% of them say an agreement will have a more beneficial (or less harmful) effect on the national economy than it will on national security. Only 9% of individuals say an agreement with the Palestinians will be better for security than for the economy. Importantly, similar patterns emerge for both right and left voters, as well as among individuals who answered positively (negatively) to *both* questions (see Table B11).

Table 7: Differential Effects by Risk Aversion

	Ordered Vote Choice	Peace Index	Econ Pol. Index	Effects of a Peace Settlement	
				Sociotropic Index	Personal Index
	(1)	(2)	(3)	(4)	(5)
Treatment	0.016 (0.032)	-0.079 (0.075)	-0.099 (0.073)	-0.098 (0.093)	-0.129 (0.095)
Risk Averse	-0.027 (0.037)	-0.176 (0.086)	-0.043 (0.083)	-0.140 (0.104)	-0.126 (0.108)
Treatment * Risk Averse	0.055 (0.041)	0.291 (0.095)	0.115 (0.089)	0.218 (0.116)	0.205 (0.120)
Demographic Controls	YES	YES	YES	YES	YES
Strata FE	YES	YES	YES	YES	YES
Observations	1,311	1,277	1,111	1,282	1,281
R-squared	0.550	0.458	0.212	0.395	0.349

This table shows the differential treatment effects on risk averse individuals, defined as those with ex ante subjective willingness to take risks at the median or below. The outcomes are the 2015 vote choice, ordered Right (0) Center/Other (0.5) Left (2), the Peace Index and the Economic Policy Index (Cols 1-3), and indices for whether a peace settlement will improve Israel's economy and/or security (Col 4) and the individual's personal safety and/or economic situation (Col 5). Indices constructed following Kling et al 2007. All regressions are OLS, and control for the full set of controls and strata FE in Table 3, Col 2, except that we replace the willingness to take risk measure with a dummy for being risk averse. Robust standard errors in parentheses.

financial literacy tests. The effect is particularly accentuated for hard-to-teach principles such as the relative riskiness of individual stocks versus mutual funds. Treated individuals also show increased propensities to invest in the stock market after the experiment.

6.3 Risk aversion

Could part of the mechanism then be that treated individuals become more aware of both the economic returns and *the risks* of restarting the peace process relative to the status-quo? To examine this, we exploit data on individuals' pre-treatment risk aversion that we measured using a question validated by Dohmen, Falk, Huffman, Sunde, Chupp, and Wagner (2011) both in the field and in large-sample incentivized experiments.²⁶

An increase in the propensity to support peace concessions could reflect a decrease in the perceived riskiness of concessions for peace (emphasized by the right) or a rise in the perceived riskiness of status quo policies (emphasized by the left). If risk-averse individuals respond to the treatment more, it is consistent with an increase in the perceived risk of status quo policies, rather than a decrease in the perceived risk of potential peace initiatives (see Appendix A.3 for the theoretical intuition).

Table 7 estimates the treatment effect, interacted with pre-treatment risk aversion, on

²⁶The specific question is: "How do you see yourself? Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?" (on a scale of 1 (not at all willing to take risks) to 10 (very willing)). This question was also highly correlated with another set of questions we employed using hypothetical lotteries (please see the survey instruments).

voting, policy preferences and predictions about the effects of a peace settlement. Notice that ex-ante risk averse individuals—in both treatment and control—are not significantly different from their more risk-tolerant counterparts in either their ordered vote choice or in their economic policy preferences (Cols 1 and 3, respectively). However, while risk averse individuals in the control group are significantly *less* supportive of peace concessions, risk averse individuals that were exposed to financial markets show significantly greater increases in support for peace concessions (Col 2). Similar differences show up in perceptions of how a peace settlement would affect both Israel’s economic and security situation, and the individuals’ own. These heightened treatment effects on the risk averse are consistent with exposure to financial markets causing individuals to perceive a larger risk of continuing with status quo policies relative to the risk from negotiating for peace.

7 Persistence

Does the treatment effect persist? If the effect is solely due to short-term attention grabbing, it should not. There are, however, at least three mechanisms by which the effects could last. The first is *habit formation*: having decided to support a particular position, and given costs to re-optimizing, an individual may reasonably stick with her previous decisions. A second is *cognitive dissonance*: having voted for a particular party, an individual comes to prefer that party (Mullainathan and Washington, 2009)). A third possibility is that treated individuals continue to pay attention to economics and to accumulate financial understanding, and this continues to influence their policy preferences. Note that, unlike the first two reasons for persistence, the third means the treatment could have additional effects, beyond its immediate effect on vote choices during the 2015 elections.

A year after the experimental intervention, in April 2016, we surveyed the original participants about their current political positions and, most importantly, “*If elections were held today, which party would you vote for?*” We were able to re-sample 943 participants (70% of the original 1345 assigned to treatments). This sample is overall well-balanced across treatment and control on pre-treatment vote choice, policy preferences and other characteristics (Table B12). Table 8 reports the results on voting intentions. Columns 1-2 suggest that those exposed to the financial asset treatment continue to show a 0.040 (ITT) to 0.047 (TOT) difference in their ordered vote choice in favor of left parties (*p-values* 0.047 and 0.032, respectively). This reflects an increased propensity to vote for the left by 4.9pp (ITT) to 5.7pp (TOT), and a reduction of intended vote for the right by 3.1pp-3.7pp (Table B13). We also observe a positive but statistically insignificant effect

Table 8: **Voting Intentions, One Year Post-Intervention**

	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)
Treatment	0.040 (0.020)	0.047 (0.022)	0.025 (0.016)	0.029 (0.018)
Voted Right '15			-0.266 (0.027)	-0.266 (0.025)
Voted Left '15			0.202 (0.024)	0.203 (0.022)
Demographic Controls	YES	YES	YES	YES
Strata FE	YES	YES	YES	YES
F(excluded instruments)		2622		2564
Observations	943	943	939	939
R-squared	0.530	0.529	0.657	0.657

Notes : Dependent variable is individuals' responses, in April 2016, to the question: "If elections were held today, which party would you vote for?" ordered from Right (0), Center/Other (0.5) to Left (1). The list of parties is identical to the list of parties in the 2015 elections. All regressions include the full set of controls from Table 3, Col 2, including controls for the vote choice in 2013. Cols 3-4 include indicators for an individual's vote for the left and the right in 2015. Robust standard errors in parentheses.

on the 2016 Peace Index.²⁷

As a heuristic exercise, Columns 3-4 add controls for individuals' (post-treatment) vote choice in 2015. The results are consistent with the possibility that the treatment may have additional effects beyond the immediate effect in the 2015 elections. Overall, the data seem to suggest a persistent effect rather than either a short-term attention effect, habit formation or cognitive dissonance alone.²⁸

²⁷In Table B14 we report treatment effects on the individual components of the peace index as well as on other outcomes. Most are imprecisely estimated, but it might be worth noting that the point estimates are positive on all components of the peace index, as well as items related to inter-ethnic inclusion. The differences between treatment and control on the two state solution's effects on the economy are attenuated relative to 2015, but the increase in means relative to Table 6 suggest that this is not because the treated group fell in their assessments of the benefits of a peace settlement but rather that the perceived benefits among the control rose. Attention in both groups shifted somewhat to security, possibly related to the 'Stabbing Intifada' between 9/2015 and 10/2016. Finally, we asked a novel question in the 2016 follow-up on whether a peace settlement is viewed as zero sum. The results are suggestive: as Figure B6 shows, while 29.27% of the control believed that a peace settlement would benefit "only the Palestinians", this falls to 26.27% in the treatment group.

²⁸Treated individuals also continue to be 6.06pp [0.0363] more likely to read financial news outlets compared to those in the control with similar demographics, pre-treatment financial literacy and other characteristics (mean= 40.1%). One year out, there is again little change (2.26pp [0.0246]) in the probability of following non-financial news outlets (mean= 88.8%).

8 In-Group vs. Out-Group Assets

A major motivation for our project was to examine if exposure to assets of the opposing group in the conflict—Palestinian stocks in our case—would have particularly strong effects on attitudes towards the peace process. Out-group assets expose individuals to more novel sets of considerations and, importantly, to common risks and benefits. However, out-group assets are less familiar, and there may also be stigma and psychological costs associated with “trading with the enemy” that can affect participation – both on the extensive margin, in the takeup of financial assets (Huberman, 2001), and the intensive margin, in the levels of engagement. The price performance of the different assets may also influence willingness to participate and risk sensitivity (Imas, 2016, Malmendier and Nagel, 2011).

Columns 1-2 in Table 9 separate the overall treatment effect into the effect of being assigned Palestinian versus other assets. We examine both the vote (Panel A) and the Peace Index (Panel B). The effects appear rather similar in magnitude. For the vote choice, exposure to non-Palestinian assets may have a somewhat stronger effect, though the difference is not significant. The effects on the Peace Index are almost identical. These broad similarities, however, may mask differences in asset price performance, in participation, and in the inferences people make from their asset exposure. We consider each in turn.

Recall that the participating Israeli assets all out-performed the Palestinian assets (Figure 1). It may thus be hard to disentangle the effects of in-group vs. out-group assets from the effect of price changes. Columns 3-4 estimate the effect of the price change of the assigned asset up until the day before the election (March 16), beyond the effect of being assigned to the treatment. The treatment effect on vote choice is higher for assets that performed better, but performance does not appear to affect willingness to support concessions for peace. Including both price change and the assets’ nationality (Cols 5-6), the Palestinian asset effects are somewhat stronger relative to Cols 1-2—both on voting and on the peace index—and the effects of the non-Palestinian assets are attenuated. However, the differences between the effects of Palestinian and non-Palestinian assets remain statistically insignificant.

Next, consider participation. Those assigned Palestinian stocks are less likely to take up the treatment (78.6% relative to 82.7% for the non-Palestinian). Further, even among those that took up assets, participants with Palestinian stocks tend to be less engaged: they spend less time on the weekly surveys, answer fewer factual questions about the asset and its past price performance correctly, and are not as good at predicting the next week’s price performance (Table B16, Panels A,B). Though individuals assigned

Table 9: Effects of In-Group vs Out-Group Financial Assets

	ITT (1)	TOT (2)	ITT (3)	TOT (4)	ITT (5)	TOT (6)
Panel A: Ordered Vote Choice						
Palestinian Assets	0.032 (0.022)	0.042 (0.026)			0.042 (0.024)	0.055 (0.030)
Non-Palestinian Assets	0.065 (0.020)	0.078 (0.023)			0.038 (0.036)	0.043 (0.040)
Treatment			0.041 (0.020)	0.051 (0.023)		
Price change of asset by elections (basis points)			0.454 (0.222)	0.517 (0.258)	0.507 (0.557)	0.660 (0.616)
F(excluded instruments)		1454		1504		958.7
Observations	1,311	1,311	1,311	1,311	1,311	1,311
R-squared	0.550	0.547	0.550	0.548	0.550	0.548
Panel B: Peace Index						
Palestinian Assets	0.111 (0.051)	0.142 (0.061)			0.120 (0.058)	0.155 (0.068)
Non-Palestinian Assets	0.110 (0.047)	0.131 (0.054)			0.086 (0.086)	0.098 (0.094)
Treatment			0.109 (0.046)	0.136 (0.055)		
Price change of asset by elections (basis points)			0.044 (0.520)	-0.023 (0.597)	0.442 (1.297)	0.632 (1.428)
F(excluded instruments)		1482		1522		978.6
Observations	1,277	1,277	1,277	1,277	1,277	1,277
R-squared	0.455	0.455	0.455	0.455	0.455	0.455
Demographic Controls	YES	YES	YES	YES	YES	YES
Strata FE	YES	YES	YES	YES	YES	YES

Notes: This table presents OLS (ITT) and 2SLS (TOT) estimates of the treatment effect on an individual's vote choice, ordered Right (0) Center/Other (0.5) Left (1) (Panel A) and the Peace Index (Panel B). The price change is the change in basis points measured from the day of assignment to the trading day preceding the election (March 16). Non-Palestinian Assets include Israeli stock and vouchers. All regressions include the full set of strata FE and controls from Table 3, Col 2. Robust standard errors are in parentheses.

Palestinian stocks did actively trade more in the weeks prior to the elections, this is because they are more likely to sell their asset, not buy.

Finally, there is some suggestive evidence that those assigned Palestinian assets make different inferences, that might counterbalance the lower level of engagement. In particular, they are 40pp more likely than those that received Israeli assets to credit peaceful relations with neighbors as the most important driver of their assets' value (rather than company management, workers, national economic policies and conditions, and domestic political factors – see Table B16, Panel C). And those compliers who saw their financial asset's value as being driven more by peaceful relations are also more likely to support

peace concessions (Table B17).

Thus, there appear to be several parallel channels at play. Individuals exposed to domestic assets are more likely to take up assets and are more engaged, increasing the intensity of treatment. In addition, domestic assets performed better during the time of our study. Individuals exposed to out-group assets, however, appear more likely to make the direct link between their financial asset and the peace process, and those that do are more likely to support the peace process. In our setting, the overall effects ended up being quite similar.

9 Conclusion

This is the first paper to measure the causal effects of providing incentives for individuals to trade in the stock market on their attitudes towards peace and their electoral choices. We find that providing individuals with both means and incentives to trade in the stock market systematically shifts their voting choices towards parties more supportive of the peace process. These effects appear to persist a year after the experiment ended. The evidence suggests that the treatment effects are not primarily driven by direct monetary incentives but rather by changes in policy preferences. Furthermore, the change in policy preferences largely reflect a combination of an increase in salience of economic issues and a reassessment of the risks and returns to the economy of concessions for peace relative to status quo policies. We note that financial exposure may also affect voting decisions through additional channels that were not captured by the survey measures and subtreatments we included.

Contemporary policy suggestions in areas of persistent ethnic conflict tend to focus either on diplomacy or on international peacekeeping. Our results suggest that an alternative approach that has been largely neglected in recent times—exposure to financial markets—might have promise as well. The treatment effects we uncover are substantial despite the context of persistent ethnic conflict, and they emerge without the need for prohibitively high stakes or the need to expose individuals to the assets of the other party to the conflict. This last feature is less likely to elicit a backlash by either politicians or participants. Our intervention is also arguably empowering rather than paternalistic. It helps individuals to learn about stock markets on their own and leaves them to draw their own conclusions about the economic costs of different policies. This should also help make it more widely acceptable than information campaigns that might sometimes be perceived as propaganda.

One intriguing possibility is that rather than focusing on providing aid to govern-

ments or even directly to populations in conflict zones, donors could examine providing individuals with resources earmarked to invest in stock in their national or regional exchanges, which can only be sold gradually over time. Beyond the direct aid provided, such policies might potentially lead recipients to internalize and take more account of the gains and risks of conflict and peacemaking to society more generally. In so doing, carefully designed financial exposure may provide a useful channel for fostering peace.

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A APPENDIX (For Online Publication)

Valuing Peace by Saumitra Jha and Moses Shayo

Note: Appendices denoted A appear both on the journal’s and on the authors’ websites. Appendices denoted B appear only on the authors’ websites.

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A.1 Effects of paper vs realized losses

Recall from 6.1 that the treatment effect on those divested before the election is not smaller than the effect on those who had experimentally-assigned *skin in the game* on election day. This is inconsistent with direct material incentives explaining the effect. However, it remains an intriguing question why individuals who were divested before the elections actually appear to respond *more* in their voting decisions (Col 2). One possibility is that knowing that they were committing to a shorter duration, made early divesters more likely to take up the treatment to begin with. It may have also made them more engaged in trading and in other parts of the study during the period prior to elections, increasing the treatment intensity. However, early divesters are only 0.011 (se=0.026) more likely to take up the treatment, and do not appear to engage in more trades, have more accurate knowledge of their stock’s performance, spend more time on the survey or be otherwise more engaged prior to the elections (Table B16).

Instead, we unpack the results in light of a distinction highlighted by Imas (2016): that differences in risk-related behavior across settings can be reconciled by the differential effects of realized losses versus paper losses. In particular, Imas shows that individuals experiencing realized losses tend to become more averse to risks, whereas those experiencing paper losses become more risk-seeking. If this is true, and if the treatment operates in part through exposing individuals to broader economic risks, then the effects should be greater for those with realized losses relative to paper losses. We examine this in Table A5. The first three columns replicate the results from Table 4 in the paper. Column 4 examines whether the treatment effect differs for early and late divesters according to whether the price of their assigned asset rose or fell prior to the early group’s divestment. The results appear to confirm Imas’s interpretation: while those whose assets did well show similar effects among both early and late divesters, among those whose prices fell, the effect is 0.084 (se=0.029) for those who divested before the elections while it is

0.005 (se=0.024) for those who did not realize these falls in price. Column 5 uses the price change to instrument for realized versus paper portfolio gains and losses, showing a consistent picture: those with realized losses by election change their vote while those with paper losses are less sensitive.

Finally, Columns 6 and 7 in Table A5 repeat this exercise for the subset of individuals who reported (pre-treatment) a willingness to take risks that is at or below the sample median. Consistent with the risk sensitivity interpretation, the difference between those with realized and paper losses is further amplified for the risk-averse. As we show in section 6.3 in the paper, the risk-averse appear to respond more to the treatment in their attitudes towards the peace process as well.

A.2 Testing for effects due to wealth and affect

One possibility is that receiving a financial portfolio worth \$50 or \$100 might have some form of wealth effect that could change policy preferences directly. It could also affect well-being or increase stress. It is worth observing, however, that the initial amounts we provide are unlikely to change an individual’s overall wealth meaningfully enough to influence voting a month later. Further, as we just saw, economic policy preferences move, if at all, slightly to the *right*, rather than to the left.

However, we can test whether the effects of asset exposure are larger for the poor, as one might expect with a direct wealth effect. Table A6 (Cols 1,3,5) estimates the interaction of the treatment with an indicator for below average pre-treatment income on the vote choice, peace index, and economic policy index. As expected, poorer individuals do support more left-leaning economic policies in our sample (Col 5). However, the interaction term shows no significant difference in the treatment effect for this group for any of these outcomes.

A related test of a potential wealth effect is to see if the effects are greater for those that received the high allocation. As Column 2 suggests, while the effect of being assigned \$50 of financial assets is 0.044 on the ordered vote choice, the effect of being assigned \$100 is only 0.016 larger (a statistically insignificant difference).

Another possibility is that the provision of financial assets causes meaningful changes in individuals’ well-being, mood or affective states of mind, potentially associated with winning a lottery or with having to make financial decisions. In other settings, the positive effect of such chance events has tended to favor incumbent parties, which should, if anything, attenuate our results Healy, Malhotra, and Mo (e.g. 2010). To examine this directly, we asked individuals immediately after the elections not only about their overall life satisfaction but also a battery comprising the top predictors of well-being based on

Benjamin, Heffetz, Kimball, and Szembrot (2014, Table 2). As we show in Table A7, however, the treatment did not significantly change *any* individual indicator of subjective well-being or a combined index of all indicators. Taken together, our treatment effects do not appear to be due to a wealth effect nor to a change in mood or affective state.

A.3 Differential effects by risk aversion: theoretical intuition

If the treatment primarily attenuates an individual’s perceived risk of pursuing a peace initiative, either by lowering the probability of bad outcomes or by increasing the returns in the various states, then the treatment effect should be larger among the less risk averse individuals, who may now be willing to take the risk of pursuing such an initiative.

To see the intuition more clearly, consider a simple example. Suppose that absent the treatment, the payoff from the status quo (SQ) is 55 while a peace initiative (PI) is a gamble yielding 100 with probability 0.5 and 0 with probability 0.5. In this case, both a risk averse and a risk neutral individual would prefer SQ to PI. Now suppose the treatment leads individuals to reevaluate the odds of the good and the bad states under PI. Specifically, PI now yields 100 with probability 0.6 and 0 with probability 0.4. Note that a risk neutral individual would now prefer PI to SQ. However, a sufficiently risk averse individual would still prefer SQ. Alternatively, suppose the treatment leads individuals to reevaluate the returns in the various states under PI. Specifically, PI now yields 107 with probability 0.5 and 7 with probability 0.5. Again, a risk neutral would now prefer PI but a sufficiently risk averse individual would prefer SQ.

If, on the other hand, the treatment causes individuals to perceive greater risks from continuing with the status quo (i.e. the treatment leads the perceived returns under the status quo to be second order stochastically dominated relative to the control), then the treatment effect should be stronger among the more risk averse. Continuing the example, suppose that absent the treatment, the payoff from the SQ is 55 and from PI 50. But now suppose the treatment leads individuals to perceive a risk associated with SQ. Specifically, now SQ is seen as a gamble yielding 0 with probability 0.5 and 110 with probability 0.5. A risk neutral would continue to prefer SQ but a sufficiently risk averse individual would switch to preferring PI.

A.4 How much of the treatment effect can be explained by different mechanisms?

As a heuristic exercise, this appendix examines how much of the estimated treatment effect is explained when we control for each of the candidate channels discussed in Section

6 in the paper. We do not claim to engage in a full-fledged mediation exercise, which requires strong orthogonality conditions (see discussion in Imai, Keele, Tingley, and Yamamoto (2011)). Nevertheless this exercise can help illuminate patterns in the data.

Figure A2 shows the estimated treatment effect on the ordered vote choice, after controlling for different outcome variables. The change in coefficients suggests a consistent pattern that highlights the relationship between asset exposure, attitudes towards peace and a focus on the gains to the broader economy. In the post-election social survey (top-left panel), individuals' attitudes towards peace stand out as a major factor that is both influenced by the treatment and is correlated with the vote choice: holding individuals' post-treatment peace attitudes constant attenuates the treatment effect by 28.6%. Two other factors also stand out: the fact that, as we have seen, treated individuals are (somewhat) more likely to view socio-economics as the main issue in the election and that they also increase their assessment of the potential gains to the Israeli economy from a peace agreement. Both these factors also correlate with a vote for parties supportive of the peace process, and controlling for them attenuates the treatment effect by 9.6% and 17.3% respectively.

In contrast, controlling for other factors that might influence one's vote, such as an increased willingness to socialize with or do business with Israeli Arabs, subjective wellbeing, the security and personal effects of the peace process, a focus on security, or information acquisition of political platforms or economic facts (bottom left panel), do not seem to explain the treatment effect.

Consider next the July financial survey (top-right panel). As we have seen, those exposed to financial assets also somewhat increase their conservatism on economic policy. Since this would encourage a vote for the right, controlling for it increases the estimated treatment effect on vote choice. Similarly, controlling for financial literacy slightly strengthens the estimated effect.

It is perhaps interesting to note that simultaneously controlling for the three most influential channels (peace attitudes, attention to economics and evaluation of the economic effects of the peace process) attenuates the treatment effect by 39.5% (to 0.032 (0.0177)). Controlling for all the channels—including those that strengthen the effect—attenuates it by 25.1% (to 0.041 (0.0195) in the common sample). Yet, the fact that there remains a robust and significant effect of financial asset exposure on voting, even controlling for all these factors, might suggest that financial exposure may operate through additional mechanisms that demand further research.

As one step in this direction, the bottom right panel of Figure A2 compares the extent to which controlling for different responses among the compliers augments or attenuates

the treatment effect. First observe that controlling for those that traded outside the experiment actually strengthens the treatment effect. This suggests that these outside trades might indeed have played a small role in undoing the treatment. Further, we find some suggestive evidence for the parallel channels we discussed in Section 8 (on the Israeli and Palestinian sub-treatments). The more engaged and active in the study (higher for the Israeli asset treatment) are more likely to change their voting decision, thus controlling for engagement attenuates the treatment effect. In parallel, however, as we have seen there is a correlation between compliers that emphasized the role of inter-state peace in driving their asset's value and support for peace (higher for the Palestinian treatment). Controlling for individuals' evaluations of the drivers of their asset also attenuates the treatment substantially. This attenuation is consistent with both engagement in financial activity and the making of a link between financial assets and peace potentially acting as parallel intermediating mechanisms.

Table A1: Comparison of the Sample and the Israeli Population

		Randomization Sample (N = 1,345)	Observed vote (N=1,311)	Israeli Jewish Population	Israeli Population
1. Region: Population in District (%)					
	Jerusalem District	9.4	9.2	11.1	12.5
	Northern District	9.5	9.5	9.5	16.4
	Haifa District	13.7	13.7	10.7	11.7
	Central District	29.2	29.2	28.5	24.4
	Tel Aviv District	19.8	19.8	20.2	16.3
	Southern District	10.6	10.7	14.2	14.4
	West Bank	7.8	7.8	5.8	4.5
2. % Female in Pop., 18+		48.3	48.1	51.4	51.3
3. Age (Population above age 18 (%))					
Male	18-24	10.1	9.5	14.6	16.1
	25-34	29.6	29.1	20.4	21.0
	35-44	28.1	28.6	18.7	19.5
	45-54	15.0	15.3	14.7	14.9
	55-64	9.6	9.8	15.1	13.9
	65+	7.6	7.6	16.5	14.5
Female	18-24	14.2	14.1	13.3	14.6
	25-34	29.7	29.0	19.2	19.9
	35-44	26.3	26.3	17.9	19.0
	45-54	14.0	14.1	14.6	14.9
	55-64	10.5	10.8	15.5	14.3
	65+	5.4	5.6	19.5	17.3
4. Religiosity (Jewish Population aged 20 and over (%))					
	Not religious/Secular	63.1	63.1	43.4	
	Traditional	16.8	16.7	36.6	
	Religious	11.9	12.0	10.6	
	Ultra-orthodox	8.2	8.2	9.1	
5. Schooling (%)					
	Less than high school grad (0 to 10 yrs.)	5.8	5.7	13.7	18.3
	High school graduate (11 to 12 yrs.)	13.7	13.7	33.3	33.9
	Post-secondary/BA Student (13 to 15 yrs.)	38.2	37.9	24.1	22
	College grad and above (16+ yrs.)	42.3	42.6	28.9	25.9
6. Net Monthly Income per Household (NIS)					
	Mean	10,978	11,035		14,622
	Median	12,000	12,000		13,122

Sources for Israeli population data (last two columns): 1: Statistical Abstract of Israel 2015, Table 2.15, 2014 Totals. 2,3,5: Statistical Abstract of Israel 2015, Table 8.72, 2014 Totals. 4: Statistical Abstract of Israel 2015, Table 7.6, 2013 Totals. These religiosity categories are available for the Jewish population only. Survey data for religiosity includes all observations age 20 or over (8 excluded). 6: Statistical Abstract of Israel 2015, Table 5.27, 2013 Total (mean). Median is midpoint between 5th and 6th deciles. Survey data represents midpoint of SES categories.

Table A2: Balance by Sub-Treatment

	Control Mean	Late Divest		Voucher		High Allocation		Palestinian		Israeli Stock	
	[SD]	Diff. (SE)	P-value	Diff. (SE)	P-value	Diff. (SE)	P-value	Diff. (SE)	P-value	Diff. (SE)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Voted Right '13	0.245 [0.431]	0.000 (0.03)	0.994	-0.008 (0.039)	0.845	-0.002 (0.031)	0.952	-0.01 (0.032)	0.764	0.003 (0.033)	0.925
Voted Left '13	0.126 [0.332]	0.009 (0.023)	0.696	0.011 (0.031)	0.733	0.011 (0.025)	0.644	0.014 (0.026)	0.592	0.008 (0.026)	0.751
Peace Index	0.004 [0.784]	0.044 (0.057)	0.435	0.034 (0.072)	0.634	0.053 (0.06)	0.382	0.064 (0.061)	0.300	0.037 (0.062)	0.554
Economic Policy Index	-0.005 [0.596]	0.009 (0.04)	0.821	0.012 (0.054)	0.832	0.000 (0.042)	0.993	0.037 (0.043)	0.397	-0.013 (0.045)	0.767
Bought/Sold Shares in Last 6 Mths [0/1]	0.368 [0.483]	-0.017 (0.033)	0.600	0.011 (0.044)	0.800	0.007 (0.035)	0.843	-0.007 (0.037)	0.843	-0.03 (0.036)	0.408
Male	0.513 [0.501]	0.012 (0.035)	0.730	0.032 (0.046)	0.482	0.002 (0.036)	0.946	0.021 (0.038)	0.579	-0.017 (0.038)	0.656
Age [Yrs]	41.53 [14.293]	-2.221 (0.946)	0.019	-3.904 (1.254)	0.002	-2.253 (0.99)	0.023	-2.079 (1.048)	0.048	-1.587 (1.058)	0.134
Post Secondary Education	0.232 [0.423]	-0.021 (0.029)	0.460	0.021 (0.039)	0.596	-0.012 (0.03)	0.688	-0.001 (0.032)	0.965	-0.013 (0.032)	0.673
BA Student	0.152 [0.360]	-0.011 (0.024)	0.641	-0.001 (0.033)	0.981	-0.007 (0.026)	0.780	0.012 (0.028)	0.669	-0.023 (0.026)	0.377
BA Graduate and Above	0.427 [0.495]	0.014 (0.034)	0.695	-0.033 (0.045)	0.462	0.012 (0.036)	0.738	-0.006 (0.038)	0.882	0.019 (0.038)	0.606
Married	0.629 [0.484]	-0.043 (0.034)	0.205	-0.028 (0.045)	0.528	-0.043 (0.036)	0.228	-0.056 (0.037)	0.136	-0.009 (0.037)	0.812
Religiosity: Secular	0.636 [0.482]	-0.026 (0.034)	0.441	0.001 (0.044)	0.989	-0.016 (0.035)	0.646	-0.018 (0.037)	0.623	-0.003 (0.037)	0.935
Traditional	0.172 [0.378]	0.006 (0.026)	0.825	-0.026 (0.034)	0.446	0.000 (0.027)	0.989	0.002 (0.029)	0.949	-0.011 (0.028)	0.701
Religious	0.119 [0.325]	0.013 (0.023)	0.579	0.017 (0.03)	0.573	-0.007 (0.023)	0.748	0.008 (0.025)	0.742	-0.005 (0.024)	0.836
Ultra-Orthodox	0.073 [0.260]	0.007 (0.019)	0.696	0.008 (0.024)	0.743	0.023 (0.021)	0.258	0.008 (0.02)	0.693	0.019 (0.021)	0.369
Region: Jerusalem	0.096 [0.295]	0.003 (0.021)	0.870	0 (0.027)	0.998	-0.012 (0.021)	0.571	-0.005 (0.022)	0.809	-0.007 (0.022)	0.761
North	0.089 [0.286]	0.004 (0.02)	0.839	0.042 (0.028)	0.137	-0.005 (0.02)	0.803	-0.004 (0.021)	0.866	0.002 (0.022)	0.913
Haifa	0.123 [0.328]	0.021 (0.024)	0.370	0.029 (0.031)	0.353	0.023 (0.025)	0.366	0.017 (0.026)	0.505	0.016 (0.026)	0.524
Center	0.298 [0.458]	-0.009 (0.032)	0.783	-0.035 (0.041)	0.392	-0.018 (0.033)	0.592	-0.009 (0.035)	0.799	0.007 (0.035)	0.837
Tel Aviv	0.212 [0.409]	-0.015 (0.028)	0.600	-0.01 (0.037)	0.790	-0.006 (0.03)	0.838	-0.006 (0.031)	0.845	-0.033 (0.03)	0.269
South	0.116 [0.321]	-0.015 (0.021)	0.481	-0.045 (0.027)	0.097	0.006 (0.024)	0.810	0.004 (0.025)	0.864	-0.012 (0.024)	0.623
West Bank	0.066 [0.249]	0.009 (0.018)	0.600	0.02 (0.024)	0.413	0.012 (0.019)	0.521	0.002 (0.019)	0.900	0.026 (0.021)	0.218
Monthly Family Income [NIS]+	11162.16 [5324.78]	-266.078 (380.176)	0.484	273.071 (511.126)	0.593	-196.23 (406.342)	0.629	-481.364 (413.568)	0.245	-58.627 (419.387)	0.889
Willing to Take Risks [1-10]	4.344 [2.240]	0.433 (0.157)	0.006	0.327 (0.208)	0.116	0.446 (0.162)	0.006	0.393 (0.173)	0.024	0.37 (0.168)	0.028
Time preference median or above	0.642 [0.480]	0.002 (0.033)	0.963	0.039 (0.043)	0.364	0.046 (0.034)	0.179	0.029 (0.036)	0.418	-0.012 (0.037)	0.741
Financial literacy: % correct	69.726 [23.917]	0.431 (1.642)	0.793	0.476 (2.194)	0.828	1.927 (1.689)	0.254	0.723 (1.809)	0.690	1.384 (1.764)	0.433

Notes: Includes only individuals for whom we have the 2015 vote outcome. Standard deviations in brackets in Col 1. Standard errors in parentheses in Cols 2-11. Each entry in Cols 2-11 is derived from a separate OLS regression where the explanatory variable is an indicator for treatment.

+: mid-point of SES income categories.

Table A3: Balance Across Sub-Treatments

		Assigned to treatment		Complied with treatment	
		Treatment vs. Control	Treatment vs. Other Subtreatments	Treatment vs. Control	Treatment vs. Other Subtreatments
		(1)	(2)	(3)	(4)
Asset treatment	F	0.91		1.55	
	p-value	0.591		0.044	
	N	1286		1113	
Late Divest	F	0.97	0.83	1.44	0.75
	p-value	0.499	0.702	0.081	0.798
	N	960	990	843	817
High Allocation	F	1	0.87	1.41	0.66
	p-value	0.465	0.643	0.092	0.893
	N	795	990	720	817
Voucher	F	1.29	1	1.64	0.89
	p-value	0.162	0.464	0.03	0.617
	N	489	990	464	817
Palestinian Stock	F	0.76	0.64	1.22	0.7
	p-value	0.784	0.907	0.215	0.857
	N	697	990	614	817
Israeli Stock	F	0.76	0.79	1.07	0.74
	p-value	0.783	0.754	0.375	0.813
	N	692	990	627	817

Notes : Each cell is derived from a separate OLS regression where the dependent variable is an indicator for the subtreatment (indicated in the row name) and the explanatory variables include the full list of pre-treatment variables in Table 2. The table reports the F-statistic and p-value for the hypothesis that all of the coefficients are 0. Column 1 includes individuals assigned to the relevant treatment group or to the control. Column 2 includes individuals assigned to the relevant treatment group or to other treatment groups. Columns 3-4 repeat these exercises but includes only the (selected) sample of individuals who complied with the treatment (or the control in col 3). The samples includes only the individuals for whom we have the 2015 vote outcome.

Table A4: Attrition

	Treatment	Control	Total
Initial assignment	1036	309	1345
Observed vote in March 2015 elections	1009	302	1311
Proportion observed	0.974	0.977	0.975
Observed peace deal attitudes, March 2015	985	292	1277
Proportion observed	0.951	0.945	0.949
Observed economic attitudes, July 2015	854	257	1111
Proportion observed	0.824	0.832	0.826
Observed vote intention, April 2016	735	208	943
Proportion observed	0.709	0.673	0.701

Table A5: Effects of Paper vs Realized Losses

	Full Sample					Risk Averse	
	OLS	OLS	OLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.052 (0.019)	0.038 (0.020)	0.045 (0.019)				
Divest Before Election		0.039 (0.019)					
Voucher Treatment			0.033 (0.022)	0.020 (0.024)	0.025 (0.021)	0.028 (0.031)	0.037 (0.027)
Divest Before x 1(Price Gain by Mar. 12)				0.067 (0.027)		0.088 (0.033)	
Divest Before x 1(Price Loss by Mar. 12)				0.084 (0.029)		0.126 (0.039)	
Divest After x 1(Price Gain by Mar. 12)				0.055 (0.023)		0.073 (0.030)	
Divest After x 1(Price Loss by Mar. 12)				0.005 (0.024)		0.006 (0.032)	
1(Realized Gain before Election)					0.070 (0.025)		0.090 (0.030)
1(Realized Loss before Election)					0.076 (0.028)		0.117 (0.036)
1(Paper Gain before Election)					0.052 (0.022)		0.063 (0.028)
1(Paper Loss before Election)					0.006 (0.023)		0.017 (0.030)
Strata FE	YES	YES	YES	YES	YES	YES	YES
Demographic Controls	YES	YES	YES	YES	YES	YES	YES
R-squared	0.549	0.550	0.550	0.553	0.553	0.574	0.572
Observations	1,311	1,311	1,311	1,311	1,311	817	817

Notes: Dependent variable is vote choice, ordered from Right (0), Center/Other (0.5) to Left (1). Col 4 estimates separate effects according to whether early or late divesters experienced price gains or losses. Col 5 uses the price variables in Col 4 as instruments for whether an agent experienced realized or paper portfolio gains or losses. Cols 6-7 repeat the estimates in Col 5-6 for the sub-sample reporting ex ante median or below willingness to take risks. All regressions include the full set of controls from Table 3, Col 2. Robust standard errors in parentheses.

Table A6: Wealth Effects

	Ordered Vote Choice		Peace Index		Econ. Policy Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.053 (0.025)	0.044 (0.021)	0.104 (0.058)	0.083 (0.049)	-0.017 (0.052)	-0.003 (0.047)
Below Avg Income	0.001 (0.035)		-0.052 (0.089)		0.175 (0.081)	
Treatment x Below Avg Income	-0.004 (0.039)		0.014 (0.094)		-0.028 (0.089)	
High Allocation		0.016 (0.018)		0.055 (0.042)		-0.045 (0.040)
Strata FE	YES	YES	YES	YES	YES	YES
Demographic Controls	YES	YES	YES	YES	YES	YES
Observations	1,311	1,311	1,277	1,277	1,111	1,111
R-squared	0.547	0.549	0.454	0.455	0.207	0.211

Notes : Dependent variables are individual vote choice, ordered from Right (0), Center/Other (0.5), to Left (1); the Peace Index; and the Economic Policy Index. Higher values of the indices imply greater support for peace negotiations and for redistributive policies, respectively. See Table 6. Robust standard errors in parentheses. The table reports the coefficient on the treatment indicator, a dummy for whether an individual had household income below the Israeli average, the interaction with the treatment (Col 1,3,5), and a dummy for whether an individual received a high allocation of 400 NIS in assets vs 200 NIS. All regressions include strata fixed effects and the full set of controls from Table 3, Col 2.

Table A7: Subjective Well-Being and Affect

Sample	All				Inexperienced	
	Mean	SD	Treatment Effect	SE	Treatment Effect	SE
Subjective Well Being Index (OLS)	0.026	[0.727]	0.011	(0.047)	-0.030	(0.060)
Specific Outcomes (Ordered Probits):						
Overall, how satisfied are you with your life? [1-4]	3.057	[0.661]	-0.023	(0.079)	-0.061	(0.101)
On a scale from 0 to 10, how would you rate...						
The overall well-being of you and your family	6.492	[2.100]	0.048	(0.072)	0.026	(0.091)
The happiness of your family	7.618	[1.885]	-0.010	(0.072)	-0.034	(0.094)
Your health	7.777	[1.895]	-0.021	(0.070)	-0.006	(0.093)
The extent to which you are a good, moral person and living according to your personal values	8.558	[1.379]	0.052	(0.071)	0.043	(0.092)
The quality of your family relationships	8.115	[1.765]	0.064	(0.070)	0.012	(0.092)
Your financial security	6.281	[2.304]	0.057	(0.071)	0.053	(0.088)
Your sense of security about life and the future in general	6.564	[2.229]	-0.017	(0.069)	-0.106	(0.089)
The extent to which you have many options and possibilities in your life and the freedom to choose among them	6.795	[2.238]	-0.033	(0.071)	-0.138	(0.090)
Your sense that your life is meaningful and has value	7.724	[2.053]	0.021	(0.071)	-0.096	(0.090)
Observations			1,276		818	

Notes: The table reports the treatment effect from separate regressions with the dependent variable mentioned in the first column. All regressions include strata fixed effects and the full set of controls from Table 3, Col 2, with robust standard errors in parentheses. The outcomes include the top ten aspects that predict personal wellbeing from Benjamin et al. (2014, Table 2), excluding mental health. The first row reports the coefficient on an index constructed from the different measures following Kling et al. 2007.

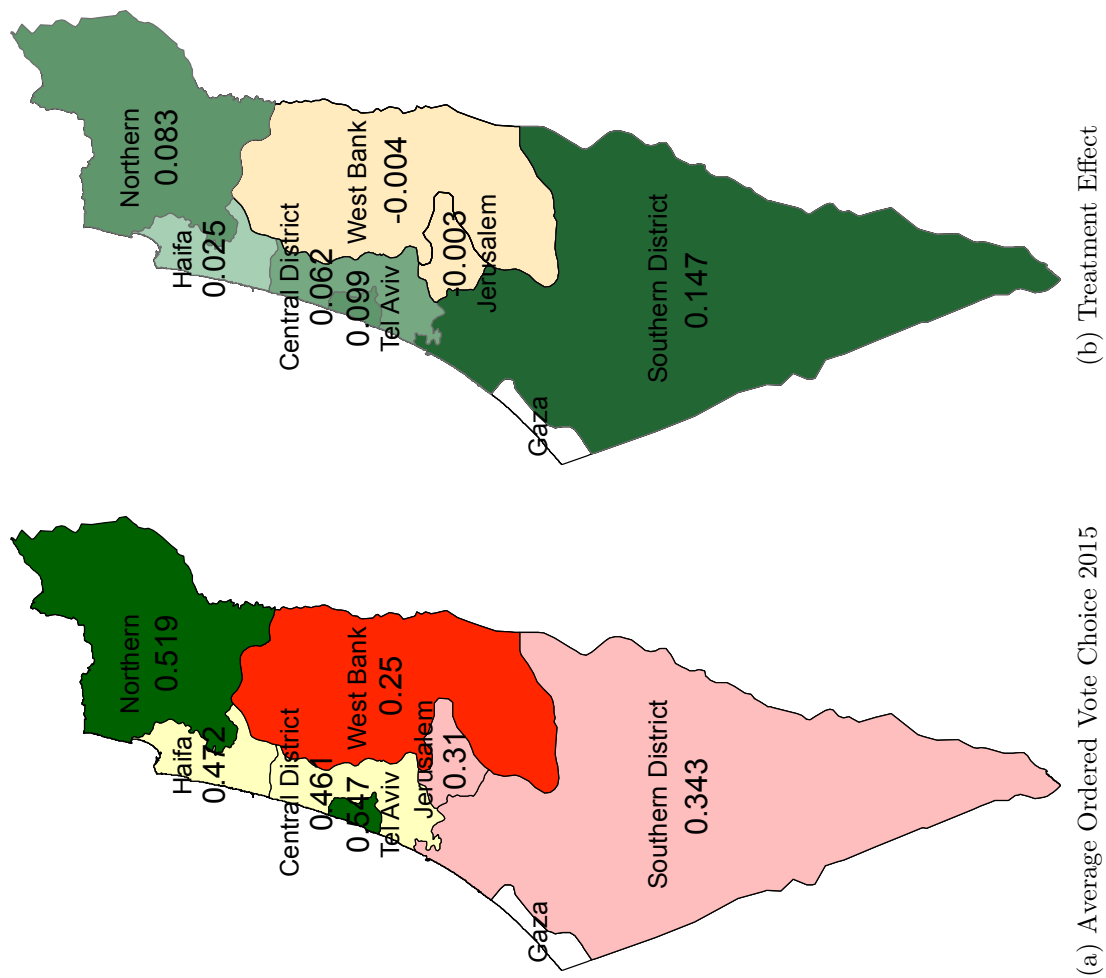
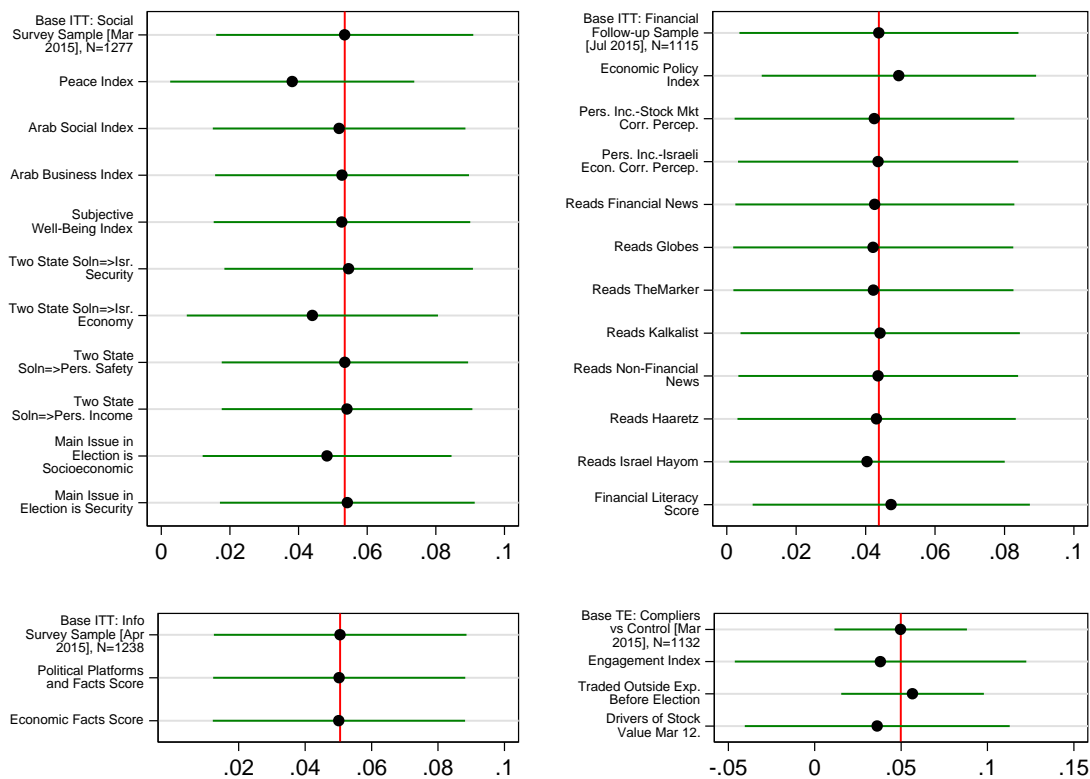


Figure A1: Treatment Effects on the Ordered Vote Choice by Region, 2015 Elections

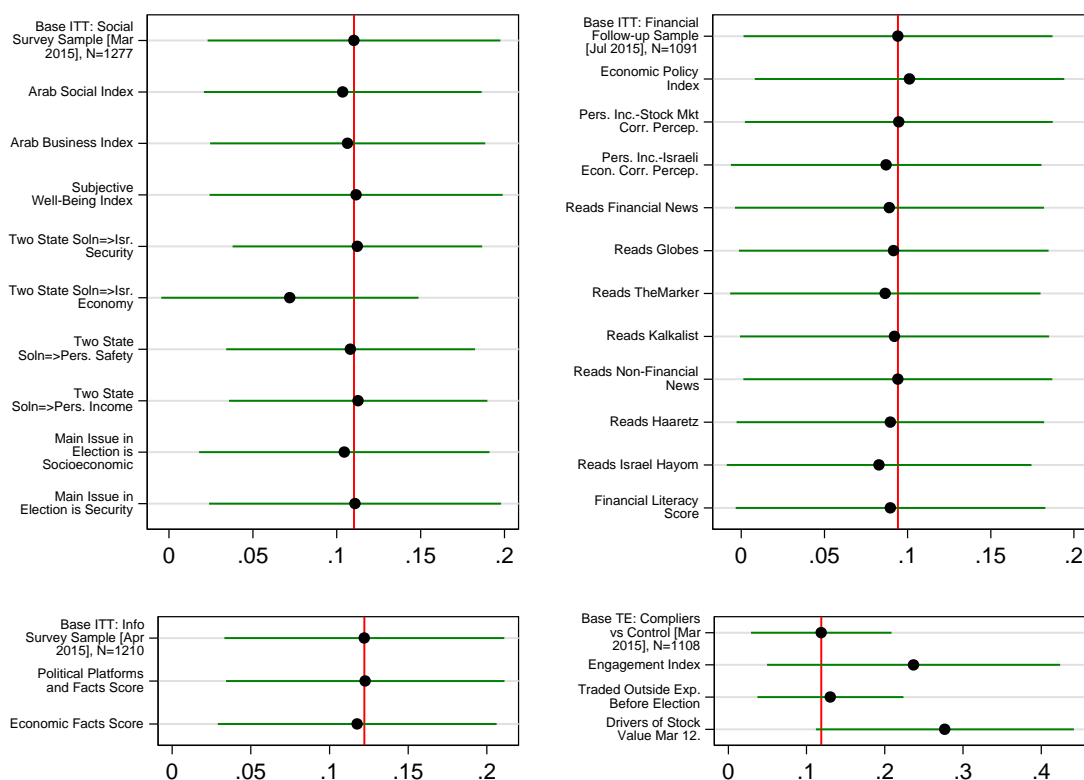
The ordered vote choice is defined as 0=Right, 0.5=Center and 1=Left.

Figure A2: How Much of the Treatment Effect on the Vote Can Be Explained by Different Mechanisms?



These figures show how the estimated treatment effect on the ordered vote choice moves when controlling for different potential channels. Each figure represents a different wave of the survey, and hence a somewhat different sample. The top coefficient in each shows the (ITT) treatment effect (and 95% confidence interval), without controlling for other outcomes. The subsequent coefficients are after controlling for the indicated variable. All regressions control for the full set of controls and strata FE from Table 3, Col 2.

Figure A3: How Much of the Treatment Effect on Support for Peace Can Be Explained by Different Mechanisms?



These figures show how the estimated treatment effect on the Peace Index in 2015 moves when controlling for different potential channels. Each figure represents a different wave of the survey, and hence a somewhat different sample. The top coefficient in each shows the (ITT) treatment effect (and 95% confidence interval), without controlling for other outcomes. The subsequent coefficients are after controlling for the indicated variable. All regressions control for the full set of controls and strata FE from Table 3, Col 2.

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B Supplementary Appendix

Valuing Peace by Saumitra Jha and Moses Shayo

Table B1: Vote Transition Matrices in Treatment and Control, 2013-2015

		Treatment			Control		
		Vote in 2015			Vote in 2015		
		Right	Center	Left	Right	Center	Left
Vote in 2013	Right	83.13	13.99	2.88	100	86.49	10.81
	Center	17.04	52.87	30.1	100	21.58	56.32
	Left	4.35	11.59	84.06	100	7.89	81.58
	Total	31.22	37.86	30.92	100	35.76	24.83
		Total			100	39.4	100

Note: The table shows the % share of individuals voting for specific blocks in 2015 by their vote in 2013. It includes only participants for whom we know their vote in 2015 (1311 out of 1345 assigned to treatments). These include 1009 observations in the treatment group and 302 in the control group.

Table B2: Treatment Effects on Party Vote in 2015

Vote in 2015 elections [0/1]	ITT-No Controls			ITT- Full Controls			ITT- Reweighted			IV-TOT		
	(1) Sample Mean	Treatment Effect	SE	R ²	Treatment Effect	SE	R ²	Treatment Effect	SE	R ²	Treatment Effect	SE
Arab Joint List	0.002	0.003	(0.002)	0.001	0.002	(0.002)	0.148	0.003	(0.002)	0.152	0.002	(0.002)
Meretz	0.050	0.021	(0.013)	0.002	0.014	(0.009)	0.408	0.012	(0.011)	0.444	0.017	(0.011)
Zionist Union	0.243	0.037	(0.027)	0.001	0.043	(0.023)	0.353	0.028	(0.020)	0.437	0.053	(0.027)
Yesh Atid	0.179	-0.038	(0.026)	0.002	-0.032	(0.024)	0.262	-0.018	(0.018)	0.252	-0.039	(0.028)
Kulanu	0.084	0.006	(0.018)	0.000	0.005	(0.018)	0.125	0.011	(0.016)	0.133	0.006	(0.021)
Shas	0.043	0.013	(0.012)	0.001	0.008	(0.010)	0.572	0.010	(0.014)	0.581	0.010	(0.012)
Yahadut HaTorah	0.042	-0.001	(0.013)	0.000	-0.000	(0.008)	0.748	-0.002	(0.010)	0.767	-0.000	(0.009)
Likud	0.163	-0.050	(0.026)	0.003	-0.043	(0.021)	0.391	-0.055	(0.026)	0.434	-0.054	(0.025)
Israel Beitenu	0.020	-0.000	(0.009)	0.000	0.000	(0.009)	0.099	0.001	(0.010)	0.123	0.000	(0.011)
Haam Itanu	0.043	-0.005	(0.014)	0.000	-0.007	(0.013)	0.280	-0.009	(0.017)	0.272	-0.009	(0.015)
Habayit Hayehudi	0.097	0.010	(0.019)	0.000	0.006	(0.015)	0.380	0.013	(0.019)	0.393	0.008	(0.018)
Other	0.013	-0.005	(0.008)	0.000	-0.003	(0.008)	0.102	-0.001	(0.009)	0.100	-0.003	(0.009)
Did Not Vote	0.021	0.010	(0.008)	0.001	0.008	(0.008)	0.102	0.009	(0.009)	0.107	0.009	(0.010)

Notes: N=1311. The table presents OLS (ITT), OLS (re-weighted to reflect 2013 vote share of Jewish parties) and IV(TOT) estimates of the treatment effect on the party voted for in the 2015 elections. Each row within Cols 2-5 represents a separate regression with the dependent variable being an indicator for voting for a particular party (or not voting). Apart from Column 2 (marked "No Controls"), all regressions include the full set of controls and Strata fixed effects from Table 3, Col 2. Robust standard errors in parentheses.

Table B3: Treatment Effect on Party Vote in 2015: Multinomial Logit

Vote in 2015 elections [0/1]	Sample Mean	SD	Multinomial Logit	
			Treatment Effect	SE
Zionist Union	0.243	0.429	reference category	
Yesh Atid	0.179	0.384	-0.439	(0.215)
Likud	0.163	0.370	-0.681	(0.255)
Habayit Hayehudi	0.097	0.296	-0.340	(0.301)
Kulanu	0.084	0.277	-0.218	(0.283)
Meretz	0.050	0.217	0.338	(0.386)
Shas	0.043	0.204	0.014	(0.398)
Haam Itanu	0.043	0.202	-0.492	(0.354)
Yahadut HaTorah	0.042	0.201	-0.371	(0.364)
Did Not Vote	0.021	0.142	0.155	(0.569)
Israel Beitenu	0.020	0.139	-0.356	(0.486)
Arab Joint List	0.002	0.048	14.417	(0.771)
Other	0.013	0.113	-0.509	(0.545)

Notes: N=1311. The table presents Multinomial Logit estimates of the treatment effect on the party voted for in the 2015 elections. The parties are ordered by their vote share in the sample. The multinomial logit includes controls for 2013 vote, age(2), willingness to take risks and traded stocks pre-treatment. Robust standard errors in parentheses.

Table B4: Treatment Effects on Ordered Vote Choice in 2015

	Ordered Logit		OLS		IV-2SLS
	ITT	ITT	ITT	ITT	TOT
		re-weighted		re-weighted	
	(1)	(2)	(3)	(4)	(5)
A. Full sample (N=1311)					
Treatment	1.494	1.472	0.052	0.047	0.064
	(0.233)	(0.254)	(0.019)	(0.019)	(0.022)
R-squared/ Pseudo R2	0.369	0.434	0.549	0.627	0.546
F(excluded instrument)					3129
B. Inexperienced (did not buy/sell assets six months before the experiment (N=842))					
Treatment	1.673	1.637	0.062	0.058	0.079
	(0.343)	(0.366)	(0.024)	(0.023)	(0.028)
R-squared/ Pseudo R2	0.407	0.471	0.582	0.653	0.574
F(excluded instrument)					1585
Strata FE	YES	YES	YES	YES	YES
Demographic Controls	YES	YES	YES	YES	YES

Notes: Dependent variable is individual vote choice, ordered from Right (0), Center/Other (0.5), to Left (1). Robust standard errors in parentheses. Cols 1-2 present ordered logit estimates expressed as odds ratios. Cols 3-4 are OLS. Col 5 shows 2SLS (TOT) estimates using assignment to treatment as instrument for actual participation. All regressions control for the full set of demographic controls, randomization strata and vote choice in 2013 from Table 3 (Col 2). Cols 2,4 re-weight the data to match the parties' share of 2013 Jewish vote.

Table B5: Difference-in-Difference Effects on Ordered Vote Choice in 2015^a

N=1311 x 2 waves.	ITT	ITT	ITT	ITT re-weighted	TOT
	(1)	(2)	(3)	(4)	(5)
Treatment x 2015	0.046 (0.020)	0.046 (0.021)	0.046 (0.020)	0.045 (0.021)	0.055 (0.025)
Treatment	0.008 (0.020)	0.004 (0.007)			
2015	0.005 (0.018)	0.005 (0.018)	0.005 (0.018)	-0.014 (0.019)	0.005 (0.018)
Individual FE	NO	NO	YES	YES	YES
Demographic Controls	NO	YES	NO	NO	NO
F(excluded instrument)					4673
R-squared	0.005	0.649	0.805	0.848	0.805

Notes: OLS (ITT) and 2SLS (TOT) estimates of the difference in the difference in ordered vote choice between individuals in the treatment group and control group over two waves: 2013 and 2015. Standard errors clustered at the individual level in parentheses. *2015* is a dummy for 2015. Col 2 includes the full set of controls from Table 3, Col 2, while Cols 3-5 include individual fixed effects. Col 4 re-weights the sample to match the party shares of the Jewish vote in 2013.

^aA difference-in-difference analysis should be interpreted with some caution. Whereas in the main Tables in the paper (e.g. 3) we simply control for vote in 2013, a difference-in-difference analysis imposes the additional assumption that a left vote is the same regardless of year. However, between 2013 and 2015, there have been changes in the composition of parties and how they fit into the right-left spectrum. Specifically, one of the main center parties in 2013, *Hatnuah*, created a joint list with the Labour Party, thereby moving to the left. The centrist *Kadimah* party disappeared. On the other side, Moshe Kahlon, a former member of the Likud, created a new centrist party called *Kulanu*. The ultra orthodox *Shas* party split, with offshoot *Haam Itanu* adopting an extreme right position. Lieberman's *Israel Beitenu*, split from the joint list it had formed with the Likud in 2013. Thus, voting "left" or "right" could mean different things in 2013 and 2015. With this caveat, our main interest in this table is in the interaction term reported in the top row: the difference in the change in the vote between 2013 and 2015 for the treated individuals relative to the control. Columns 1 and 2 also provide a useful placebo test: individuals in the treatment group have very similar vote choices as the control prior to treatment, especially when we include our standard set of controls. It is only after treatment, in 2015, that they diverge.

Table B6: Financial Experience and Vote Choice, 2015

	Vote for Left Party in 2015			Vote for Right Party in 2015			Ordered Vote Choice in 2015		
	ITT	ITT reweighted	TOT	ITT	ITT reweighted	TOT	ITT	ITT reweighted	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bought/Sold Shares in Last 6 Mths [0/1]	0.096 (0.045)	0.097 (0.038)	0.100 (0.046)	-0.002 (0.047)	-0.013 (0.055)	-0.004 (0.047)	0.049 (0.037)	0.055 (0.039)	0.052 (0.037)
Treatment	0.018 (0.043)	0.003 (0.036)	0.022 (0.050)	-0.042 (0.040)	-0.059 (0.049)	-0.049 (0.047)	0.030 (0.033)	0.031 (0.036)	0.036 (0.039)
Treatment x Inexperienced	0.070 (0.051)	0.071 (0.043)	0.090 (0.061)	-0.002 (0.050)	0.013 (0.059)	-0.007 (0.060)	0.036 (0.040)	0.029 (0.042)	0.048 (0.048)
Strata FE	NO	NO	NO	NO	NO	NO	NO	NO	NO
Demographic Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	1,311	1,311	1,311	1,311	1,311	1,311	1,311	1,311	1,311
R-squared	0.354	0.492	0.349	0.453	0.491	0.453	0.478	0.565	0.474

Notes: OLS (ITT) and 2SLS (TOT) estimates of the treatment effect on the probability that an individual voted for a left or right party in 2015, and the ordered vote choice (0-Right, 0.5-Center, 1-Left). "Inexperienced" is a dummy that equals 1 if an individual had not bought or sold shares in the 6 months preceding the experiment. Robust standard errors in parentheses. 2SLS estimates use assignment to treatment as instrument. Data in Cols 2,5 and 8 are reweighted to represent the vote share of Jewish parties in 2013. "Demographic controls" include dummies for vote for the left and right in 2013, sex, age, age squared, four education categories, marital status, six regional dummies, four religiosity categories, five income categories (and a dummy for missing), time preference above the median, financial literacy score and subjective willingness to take risks. Note that we do not include Strata FE in these regressions as we stratified on past trading experience, and thus strata fixed effects absorb the relationship between past trading experience and political decisions.

Table B7: Are Treatment Effects Driven by the Voters of a Specific Party?

Omitting those who voted for (in 2013):	Meretz	Labour	Hatnuah	Yesh Atid	Kadima	Shas	Yahadut HaTorah	Likud Beitenu	Habayit Hayehudi	Other	Did Not Vote
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Treatment Effect	0.051 (0.019)	0.057 (0.020)	0.046 (0.019)	0.059 (0.023)	0.041 (0.019)	0.052 (0.019)	0.055 (0.019)	0.059 (0.021)	0.052 (0.020)	0.043 (0.019)	0.052 (0.019)
Observations	1,261	1,189	1,218	840	1,276	1,219	1,256	1,095	1,212	1,234	1,310
R-squared	0.526	0.523	0.533	0.681	0.559	0.558	0.551	0.489	0.506	0.564	0.549

Notes: The table presents OLS (ITT) estimates of the treatment effect on individual vote choice in the 2015 elections, ordered from Right (0), Center/Other (0.5), to Left (1). Each column drops the voters in the sample that voted for a specific party (or did not vote) in 2013, one by one. No one in our sample voted for an Arab party in 2013. All regressions include the full set of controls and Strata fixed effects from Table 3, Col 2. Robust standard errors in parentheses.

Table B8: Treatment Effects by Religiosity, Gender, Age & Education

	(1)	(2)	(3)	(4)	(5)	(6)
	Ordered Vote	Peace Index	Econ Index	Ordered Vote	Peace Index	Econ Index
A: Religiosity	Religious and Ultra-Orthodox			Secular and Traditional		
Treatment Effect	0.028 (0.030)	0.088 (0.095)	-0.012 (0.111)	0.053 (0.022)	0.095 (0.051)	-0.040 (0.046)
Sample Mean	0.225	-0.583	-0.050	0.554	0.231	-0.011
Observations	269	259	230	1,042	1,018	881
R-squared	0.649	0.419	0.387	0.518	0.394	0.217
B: Sex	Female			Male		
Treatment Effect	0.059 (0.029)	0.109 (0.063)	-0.062 (0.061)	0.051 (0.026)	0.125 (0.065)	-0.003 (0.059)
Sample Mean	0.494	-0.051	0.056	0.479	0.173	-0.086
Observations	630	610	521	681	667	590
R-squared	0.540	0.429	0.231	0.581	0.499	0.232
C: Age	Age > Median (=37.5)			Age ≤ Median (=37.5)		
Treatment Effect	0.072 (0.029)	0.162 (0.069)	0.015 (0.061)	0.021 (0.027)	0.066 (0.064)	-0.114 (0.062)
Sample Mean	0.519	0.212	-0.026	0.456	-0.069	-0.012
Observations	629	616	559	682	661	552
R-squared	0.582	0.465	0.327	0.609	0.538	0.344
D: Educ Attainment	BA student and above			Less than College		
Treatment Effect	0.050 (0.024)	0.081 (0.060)	-0.051 (0.056)	0.045 (0.031)	0.107 (0.071)	0.004 (0.063)
Sample Mean	0.520	0.158	-0.031	0.441	-0.058	-0.003
Observations	754	732	642	557	545	469
R-squared	0.643	0.550	0.340	0.520	0.468	0.313

Notes: This table shows the treatment effect, subsetting the sample by religiosity, demographics and educational attainment. The outcomes are ordered vote choice (March 2015), Peace Index (March 2015) and Economic Policy Index (July 2015). All regressions include the full set of controls and strata fixed effects from Table 3, Col. 2. Robust standard errors in parentheses.

Table B9: Treatment Effects by Region

Effects by Region	(1)	(2)	(3)	(4)	(5)	(6)
	Ordered Vote	Peace Index	Econ Index	Ordered Vote	Peace Index	Econ Index
	Haifa			Northern District		
Treatment Effect	0.025 (0.064)	0.021 (0.202)	0.292 (0.145)	0.083 (0.092)	0.373 (0.217)	-0.176 (0.239)
Sample Mean	0.547	0.177	-0.108	0.564	0.126	0.101
Observations	180	173	157	125	122	103
R-squared	0.657	0.572	0.499	0.812	0.658	0.640
	Tel Aviv			Central		
Treatment Effect	0.099 (0.054)	0.150 (0.120)	-0.180 (0.120)	0.062 (0.043)	-0.041 (0.095)	-0.091 (0.099)
Sample Mean	0.592	0.176	-0.023	0.488	0.152	-0.060
Observations	260	256	219	383	373	320
R-squared	0.681	0.633	0.515	0.570	0.544	0.349
	Jerusalem			West Bank		
Treatment Effect	-0.003 (0.048)	-0.145 (0.177)	-0.126 (0.254)	-0.004 (0.059)	0.277 (0.192)	-0.032 (0.215)
Sample Mean	0.322	-0.216	0.046	0.230	-0.431	-0.114
Observations	121	117	112	102	101	84
R-squared	0.896	0.796	0.650	0.849	0.824	0.758
	Southern District					
Treatment Effect	0.147 (0.089)	-0.061 (0.188)	-0.131 (0.221)			
Sample Mean	0.464	0.039	0.120			
Observations	140	135	116			
R-squared	0.686	0.677	0.421			

Notes: This table shows treatment effect, subsetting the data by region, on ordered vote choice (March 2015), Peace Index (March 2015) and Economic Policy Index (July 2015). All regressions include the full set of controls and strata fixed effects from Table 3, Col. 2. Robust standard errors in parentheses.

Table B10: Treatment Effects on Knowledge of Political Platforms and Facts, April 2015: Complete Table

Sample:	All		Treatment Effect	Inexperienced Treatment Effect
	Mean	[SD]	(SE)	(SE)
Facts and Political Platforms (OLS) [Apr 17 2015]				
Economic Facts Score [Prop Correct of 5]				
Economic Facts				
1. What is the official unemployment rate today? answer within 3pp of actual	5.30%	0.533	[0.276]	0.017 (0.016)
2. What was the inflation rate in the last year? answer within 3pp of actual	-1%	0.506	[0.500]	-0.013 (0.032)
3. Did the Israeli stock market go up or down in March?	Up	0.614	[0.487]	-0.005 (0.032)
4. By what percent did the Israeli stock market change in March? answer within 3pp of actual	+5.5% (TA100), +7.1% (TA25)	0.787	[0.410]	0.041 (0.029)
5. By what percent did apartment prices rise in Israel in the last year? answer within 3pp of actual	3.7% (Jan-Feb), 4.3% (Dec-Jan)	0.393	[0.489]	0.066 (0.033)
Political Platforms & Facts Score- Preferred [Prop Correct of 13]		0.364	[0.481]	-0.006 (0.033)
Political Platforms Only Score - Preferred [Prop Correct of 8]		0.694	[0.212]	0.002 (0.013)
Political Platforms- Preferred Set		0.662	[0.249]	-0.009 (0.015)
1. Before the last elections, Benjamin Netanyahu was invited to speak at the American Congress. Who invited him?	Speaker of the House of Representatives, John Boehner	0.528	[0.499]	0.024 (0.034)
2. What was the main subject of Netanyahu's speech at the Congress?	Iran's nuclear program	0.913	[0.282]	-0.009 (0.017)
3. How did Netanyahu refer to the "two states for two peoples" principle in his speech in Bar Ilan in 2009?	Agree to the establishment of a demilitarized Palestinian state that would recognize the Jewish state	0.498	[0.500]	0.005 (0.032)
4. How did Netanyahu refer to the "two states for two peoples" principle in the days before the last elections?	Oppose a Palestinian state	0.522	[0.500]	-0.004 (0.034)
How would you evaluate Isaac Herzog's stance on the following issues:				
5. Establishment of a Palestinian state as part of a political agreement	Supports	0.854	[0.353]	-0.006 (0.024)
6. Regulation and restriction of rent increases	Supports	0.573	[0.495]	-0.027 (0.033)
7. Raising the minimum wage	Supports	0.645	[0.479]	-0.002 (0.032)
8. Building in the settlements	Opposes	0.760	[0.427]	-0.053 (0.028)
Political Facts				
9. What is the required election threshold in order to be represented in the Knesset?	3.25%	0.441	[0.497]	0.028 (0.033)
10. Who was the minister of foreign affairs in the last government (until December 2014)?	Avigdor Lieberman	0.893	[0.310]	0.034 (0.031)
11. Who was the finance minister in the last government (until December 2014)?	Yair Lapid	0.939	[0.240]	0.000 (0.016)
12. Who was the defense minister in the last government (until December 2014)?	Moshe Yaalon	0.912	[0.283]	0.026 (0.020)
13. Who was the minister of social affairs in the last government (until December 2014)?	Meir Cohen	0.542	[0.498]	0.029 (0.032)
Political Platforms- Additional Questions				
How would you evaluate Netanyahu's stance on the following issues:				
1. Cutting the defense budget	Opposes*	0.568	[0.496]	0.055 (0.035)
2. Increasing child allowances	Opposes/ No clear view*	0.671	[0.470]	-0.019 (0.032)
3. Tax cuts	Supports*	0.195	[0.397]	0.018 (0.027)
4. No VAT for basic food products	Supports/ No clear view*	0.481	[0.500]	0.030 (0.034)
5. Building in the settlements	Supports / No clear view*	0.846	[0.361]	0.016 (0.024)
Political Platforms & Facts Score - Preferred + Additional [Prop Correct of 18]		0.654	[0.188]	0.007 (0.012)
Political Platforms Only Score - Preferred + Additional [Prop Correct of 13]		0.619	[0.206]	0.002 (0.013)
Observations			1,238	-0.022 (0.018)
				782

Notes: These questions were all asked in an Information Survey fielded on April 17, 2015. Each cell represents a separate regression on getting an individual question correct, or on an aggregate score. All regressions include the full set of controls and strata FE from Table 3, Col 2. Robust standard errors in parentheses. All numerical answers were scored correct if they were within 3pp of the correct answer. The political questions were all multiple choice. *: Netanyahu's position on some of these questions arguably shifted during the course of the campaign as well as prior to our information survey. To address this source of imprecision, we report scores both with and without these questions. The latter we take as our 'Preferred' score, also reported in Table 7B.

Table B11: Respondents Predict Greater Peace Benefits for Israel's Economy than its Security

In the Event of a Peace Agreement: % of Sample Predicting:	Overall	By Vote in 2013		Separating Parallel Responses into:	
		Left	Right	Both Will Improve	Both Will Worsen
A. Worse Effects for Israel's Economy than for its Security	9.13	9.88	8.63	5.34	3.03
B. Similar for Economy as for Security	57.8	63.37	56.55	76.71	77.1
C. Better for Economy than for Security	33.07	26.74	34.82	17.95	19.87
Observations	1,282	172	313	468	297

Notes: On March 19 2015, we asked: Suppose Israel reaches a permanent agreement with the Palestinians on the principle of two states for two peoples. How do you think this will affect: Israel's economy? Israel's security? The allowable answers were: 1 (worsen a lot), 2 (worsen somewhat), 3 (no change), 4 (improve somewhat), 5 (improve a lot). This table shows the distribution of the difference of the responses between Israel's economy and Israel's security, i.e. worse effects for the economy (row A) are predicted if the economic answer was lower than the security answer to this question and better if the reverse was true (row C). The second two columns split the sample by vote in 2013, while last two columns show the pattern for those respondents giving a beneficial (detrimental) response to both questions.

Table B12: Descriptive Statistics and Balance, 2016 Follow-Up Sample

	Mean [SD]		Difference in Means				Obs.
	Treatment	Control	Without FEs		With Strata FEs		
			Diff.	P-value	Diff.	P-value	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Voted Right '13	0.220 [0.415]	0.231 [0.422]	-0.010 (0.033)	0.754	0.001 (0.006)	0.825	943
Voted Left '13	0.136 [0.343]	0.135 [0.342]	0.001 (0.027)	0.957	0.004 (0.003)	0.193	943
Peace Deal Index	0.089 [0.829]	0.123 [0.814]	-0.033 (0.064)	0.603	-0.014 (0.055)	0.795	943
Economic Policy Index	0.014 [0.575]	0.018 [0.601]	0.032 (0.047)	0.497	0.021 (0.045)	0.644	943
Bought/Sold Shares in Last 6 Mths [0/1]	0.384 [0.487]	0.394 [0.490]	-0.011 (0.038)	0.783	-0.008 (0.021)	0.692	943
Male	0.532 [0.499]	0.534 [0.500]	-0.002 (0.039)	0.966	0.005 (0.016)	0.774	943
Age [Yrs]	40.641 [13.785]	42.096 [14.436]	-1.455 (1.122)	0.195	-1.016 (1.094)	0.353	943
Post Secondary Education	0.216 [0.412]	0.245 [0.431]	-0.029 (0.034)	0.389	-0.016 (0.033)	0.641	943
BA Student	0.135 [0.342]	0.115 [0.320]	0.019 (0.026)	0.449	0.014 (0.026)	0.590	943
BA Graduate and Above	0.453 [0.498]	0.476 [0.501]	-0.023 (0.039)	0.560	-0.022 (0.038)	0.557	943
Married	0.599 [0.491]	0.601 [0.491]	-0.002 (0.039)	0.952	0.014 (0.039)	0.726	943
Religiosity: Secular	0.661 [0.474]	0.673 [0.470]	-0.012 (0.037)	0.749	-0.013 (0.030)	0.679	943
Traditional	0.148 [0.356]	0.168 [0.375]	-0.020 (0.029)	0.493	-0.014 (0.028)	0.621	943
Religious	0.113 [0.317]	0.087 [0.282]	0.026 (0.023)	0.246	0.025 (0.019)	0.201	943
Ultra- Orthodox	0.078 [0.268]	0.072 [0.259]	0.005 (0.020)	0.791	0.002 (0.013)	0.906	943
Region: Jerusalem	0.099 [0.299]	0.096 [0.296]	0.003 (0.023)	0.892	-0.003 (0.021)	0.903	943
North	0.095 [0.294]	0.082 [0.275]	0.014 (0.022)	0.537	0.022 (0.019)	0.263	943
Haifa	0.150 [0.357]	0.125 [0.332]	0.025 (0.026)	0.352	0.036 (0.022)	0.112	943
Center	0.294 [0.456]	0.322 [0.468]	-0.026 (0.037)	0.440	-0.034 (0.029)	0.250	943
Tel Aviv	0.196 [0.397]	0.221 [0.416]	-0.025 (0.032)	0.435	-0.043 (0.028)	0.128	943
South	0.094 [0.292]	0.120 [0.326]	-0.026 (0.025)	0.293	-0.019 (0.021)	0.382	943
West Bank	0.072 [0.259]	0.034 [0.181]	0.038 (0.016)	0.015	0.040 (0.015)	0.009	943
Monthly Family Income [NIS]+	11216.066 [5555.706]	11390.244 [5269.586]	-174.177 (421.747)	0.680	-229.985 (417.695)	0.582	927
Willing to Take Risks [1- 10]	4.724 [2.263]	4.380 [2.173]	0.344 (0.172)	0.046	0.396 (0.166)	0.017	943
Time preference median or above	0.678 [0.468]	0.683 [0.467]	-0.005 (0.037)	0.888	-0.009 (0.037)	0.811	943
Financial literacy: % correct	72.264 [23.311]	71.223 [23.684]	1.042 (1.852)	0.574	1.343 (1.728)	0.438	943

Notes: Standard deviations in brackets in columns 1-2. Standard errors in brackets in columns 3-6. Each entry in Columns 3-6 is derived from a separate OLS regression where the explanatory variable is a treatment indicator. Columns 5-6 control for the 104 randomization strata. +: mid-point of SES income categories.

Table B13: Long-Term Effects on Intended Vote and Support for Peace Concessions, 2016 Follow-Up Sample

	Would Vote Left 2016				Would Vote Right 2016				Peace Index, 2016			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
	0.049	0.057	0.029	0.035	-0.031	-0.037	-0.021	-0.024	0.070	0.083	0.034	0.040
	(0.024)	(0.026)	(0.021)	(0.023)	(0.029)	(0.032)	(0.023)	(0.026)	(0.053)	(0.058)	(0.039)	(0.042)
Voted Right '15			0.002	0.002			0.534	0.534				
			(0.023)	(0.021)			(0.045)	(0.041)				
Voted Left '15			0.369	0.370			-0.035	-0.036				
			(0.036)	(0.033)			(0.027)	(0.025)				
Peace Index, March 2015											0.658	0.657
											(0.031)	(0.028)
Strata FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Demographic Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
F(excluded instruments)		2622		2564		2622		2564		2657		2647
Observations	943	943	939	939	943	943	939	939	939	939	922	922
R-squared	0.464	0.462	0.575	0.575	0.460	0.461	0.596	0.597	0.439	0.439	0.675	0.675

Cols 1-8 show treatment effects on answers to the question "If the elections were held today, which party would you vote for" when surveyed a year after the experiment in March 2016. All regressions include the full set of controls from Table 3, Col 2. Cols 3-4, 7-8, 11-12 explore whether the long-term effect exceeds the 2015 effect by adding controls for the post-treatment 2015 vote and peace deals index, respectively. Robust standard errors in parentheses.

Table B14: Long-Term Effects on Other Outcomes, 2016 Follow-Up Sample

	N	Mean	SD	Treatment Effect	(SE)
<i>Peace Index [OLS]</i>	937	0.038	0.815	0.067	(0.053)
Two states for two peoples [1-Disagree, 4- Agree]	937	2.713	1.099	0.058	(0.093)
1967 borders with a possibility of land exchanges [1-4]	937	2.239	1.093	0.089	(0.093)
Jerusalem will be split into two separate cities - Arab and Jewish [1-4]	937	1.998	1.059	0.016	(0.094)
Palestinian refugees will get compensation & allowed to return to Palestine only [1-4]	937	2.218	1.049	0.194	(0.090)
<i>Social Relations Index [OLS]</i>	934	0.054	0.955	0.096	(0.065)
Arabs will live in Jewish neighborhoods [1-4]	934	2.224	1.057	0.139	(0.093)
Arabs will attend Jewish high schools [1-4]	934	2.314	1.094	0.163	(0.093)
<i>Business Index [OLS]</i>	934	0.045	0.954	0.073	(0.065)
Arabs and Jews will form joint businesses [1-4]	934	2.885	1.003	0.089	(0.091)
Arabs will manage Jewish companies [1-4]	934	2.666	1.075	0.131	(0.093)
<i>Arab parties will be part of the governing coalition [1-4]</i>	934	2.208	1.067	0.159	(0.095)
<i>Palestinians are the main culprits in the long conflict between them and the Jews [1-4]</i>	934	2.988	0.997	0.085	(0.094)
<i>Israel should integrate with the West and maintain only necessary contacts with the Arab states. [1-4]</i>	934	2.612	0.843	-0.023	(0.087)
<i>What is the Main Issue in Israel Today? [OLS]</i>					
Mainly or Solely Socioeconomic [0/1] [OLS]	936	0.288	0.453	-0.035	(0.036)
Mainly or Solely Security and Political process [0/1][OLS]	936	0.147	0.355	0.054	(0.026)
<i>Consequences of a Two-State Agreement [1-Worsen substantially, 5- Improve a lot]</i>					
Israel's economy	937	3.572	1.208	0.060	(0.089)
Israel's security	937	3.295	1.353	0.089	(0.085)
Your personal economic situation	937	3.114	0.829	0.003	(0.093)
Your personal security	937	3.221	1.208	0.130	(0.085)
<i>Consequences of not holding negotiations for the foreseeable future [1-Improve a lot, 5- Worsen substantially]</i>					
Israel's economic situation	936	3.324	0.907	-0.051	(0.090)
Israel's security	936	3.412	1.065	-0.107	(0.083)
Your own economic situation	936	3.120	0.609	0.042	(0.088)
Your own personal security	936	3.296	0.831	-0.070	(0.096)

The table reports the treatment effects on all remaining questions not otherwise already reported from the April 2016 follow-up survey, 1 year post-intervention. Each row reports the treatment effect from an ordered-probit regression with the dependent variable indicated in the first column (unless otherwise mentioned). All regressions control for the full set of strata FE and controls from Table 3, Col 2. Robust standard errors in parentheses.

Table B15: Election Polls and Asset Price Performance

Closing Asset Price Each Day (% of Feb 12 price)	(1)	(2)	(3)	(4)	(5)
% Seats Predicted for the Right	0.476 (0.528)	0.669 (0.407)	0.655 (0.381)		
% Seats Predicted for the Left	0.222 (0.240)	0.298 (0.247)	0.306 (0.175)		
% Seats Right x Israeli Stock	-1.593 (0.605)	-1.593 (0.607)	-1.593 (0.613)		
% Seats Right x Palestinian Stock	-0.404 (0.530)	-0.422 (0.526)	-0.414 (0.531)		
% Seats Left x Israeli Stock	-0.653 (0.472)	-0.653 (0.474)	-0.653 (0.478)		
% Seats Left x Palestinian Stock	-0.332 (0.242)	-0.351 (0.234)	-0.333 (0.235)		
% Seats Predicted for the Likud				0.181 (0.143)	0.259 (0.144)
% Seats Predicted for the Zionist Union				-0.162 (0.186)	-0.182 (0.162)
% Seats Likud x Israeli Stock				-0.560 (0.276)	-0.560 (0.280)
% Seats Likud x Palestinian Stock				-0.340 (0.145)	-0.353 (0.136)
% Seats Zionist Union x Israeli Stock				0.525 (0.383)	0.525 (0.388)
% Seats Zionist Union x Palestinian Stock				-0.097 (0.191)	-0.087 (0.200)
Asset Ticker Fixed Effects	Yes	Yes	Yes	Yes	Yes
Quadratic Time Trends	No	Yes	Yes	No	Yes
Week Fixed Effects	No	No	Yes	No	Yes
Observations	324	324	324	324	324
R-squared	0.569	0.575	0.581	0.495	0.508

This is an OLS regression. The dependent variable is the daily closing price of each of the assets in our study, normalized by their value as of February 12. The main explanatory variables include the % of Seats for Left and Right based on the simple averages of all polls on each day linked in "Opinion Polling for the Israeli Legislative Election 2015" in Wikipedia and supplemented by an aggregation website maintained by Haaretz

(www.haaretz.com/st/c/prod/eng/2015/elections/center). The assets include all those participating in the study: Israeli Stocks include LUMI, TA25, BEZQ. Palestinian Stocks include: PLE, PALTEL, BOP. We also include Reference Stocks from the region: AMGNRLX (the Amman Stock Exchange General Index) EGX30 (the Cairo 30 Index), XU030 (the Istanbul Index), CYFT (the Cyprus/FTSE 20). The set of days are all that included at least one poll between January 30 to March 18. All regressions include asset fixed effects. Errors are clustered at the asset level. We sequentially add Quadratic Time Trends and Fixed Effects for each week. Notice that the reference stocks are largely unaffected by the polls. However, Israeli stocks lose value with increases in predicted shares for the right. Looking at the two main parties which were the focus of the election (and for whom an increase in seat share would reduce reliance on coalition partners) in Columns 4 and 5 reveals that an increase in seat share for Likud was associated with a fall in the value of both Israeli and Palestinian stocks in our study.

Table B16: Engagement and Perceived Determinants of Asset Value among Compliers

Panel A: N= 840		Mean	SD	Palestinian Stock	Voucher Treatment	High Allocation	Late Divest	% Price change
Engagement Index (Z-Score)		0.000	[0.739]	-0.333 (0.082)	0.136 (0.065)	0.134 (0.051)	-0.007 (0.056)	-0.036 (0.013)
Deciles of Time Spent upto Mar 4		7.192	[1.881]	-0.282 (0.234)	-0.347 (0.168)	0.321 (0.131)	-0.024 (0.144)	-0.065 (0.037)
Facts Correct on Mar 4 [0-4]		2.201	[1.280]	-1.438 (0.144)	-0.034 (0.118)	0.199 (0.083)	0.040 (0.092)	-0.111 (0.023)
# Decisions Registered [0-3]		2.646	[0.752]	-0.271 (0.075)	0.054 (0.069)	0.086 (0.054)	-0.027 (0.058)	-0.037 (0.012)
# Non-Zero Trades to Mar 4 [0-3]		1.869	[1.200]	0.361 (0.145)	0.821 (0.100)	0.116 (0.083)	-0.011 (0.088)	0.031 (0.023)
# Buy Decisions [0-3]		0.942	[1.078]	-0.067 (0.082)	1.817 (0.079)	0.004 (0.054)	0.009 (0.058)	0.010 (0.014)
# Sell Decisions [0-3]		1.200	[1.124]	0.428 (0.130)	-1.024 (0.083)	0.088 (0.074)	0.010 (0.079)	0.036 (0.020)
Panel B: N= 840								
# Facts Correct on Mar 4		2.201	[1.280]	-1.438 (0.144)	-0.034 (0.118)	0.199 (0.083)	0.040 (0.092)	-0.111 (0.023)
Sector of Stock?		0.689	[0.463]	-0.175 (0.047)	-0.278 (0.043)	0.081 (0.031)	-0.038 (0.034)	-0.009 (0.008)
Movement in Price Last Week?		0.481	[0.500]	-0.302 (0.056)	0.004 (0.049)	0.078 (0.035)	0.034 (0.038)	-0.051 (0.009)
Movement in Price Last 3 Years?		0.630	[0.483]	-0.410 (0.052)	0.039 (0.037)	0.049 (0.031)	0.005 (0.035)	0.000 (0.008)
Movement in Price Next Week?		0.401	[0.490]	-0.551 (0.056)	0.201 (0.047)	-0.008 (0.032)	0.039 (0.034)	-0.051 (0.009)
Panel C: Perceived Most Important Determinant of an Asset's Value Mar 4 [N=746]								
Companies' Management		0.131	[0.338]	-0.193 (0.073)	0.012 (0.042)	-0.025 (0.026)	-0.027 (0.029)	-0.010 (0.010)
Companies' Employees		0.035	[0.184]	0.029 (0.045)	-0.015 (0.025)	0.006 (0.014)	-0.002 (0.014)	0.006 (0.006)
National Econ. Policies & Conditions		0.607	[0.489]	-0.431 (0.092)	0.036 (0.055)	-0.014 (0.037)	0.008 (0.040)	-0.029 (0.013)
Domestic Political Conditions		0.063	[0.243]	0.193 (0.046)	-0.007 (0.026)	0.020 (0.019)	-0.007 (0.019)	0.012 (0.006)
Peaceful Relations w/ Neighbors		0.164	[0.370]	0.401 (0.062)	-0.025 (0.036)	0.013 (0.026)	0.028 (0.027)	0.021 (0.009)

Notes: Each row represents a separate OLS regression of measures of engagement on the subtrements as of March 4, the last date at which both early and late divesters took the same survey, with coefficients for Palestinian Stock, Voucher, High, Late Divestment and the % Price change by March 4. The omitted category for Palestinian Stock and Voucher is the Israeli Stock Treatment. All regressions include strata FE and controls from Table 2, Col 2. Panel B provides the components of the Facts Questions. Panel C estimates the effect of each subtrement on the probability an individual will ascribe the most important determinant of an asset value to a particular cause as of March 4. Robust standard errors in parentheses.

Table B17: Perceived Determinants of Asset Value and Political Attitudes among Compliers

	(1) OLS Ordered Vote	(2) OLS Peace Index	(3) OLS Econ. Policy Index
The Main Determinant of My Asset's Value is:			
1 if Companies' Employees	0.012 (0.067)	-0.008 (0.141)	0.454 (0.132)
1 if National Econ. Policies & Conditions	0.044 (0.034)	0.148 (0.081)	-0.002 (0.065)
1 if Domestic Political Conditions	0.076 (0.052)	0.049 (0.125)	0.144 (0.099)
1 if Peaceful Relations w/ Neighbors	0.038 (0.042)	0.279 (0.102)	0.041 (0.081)
Strata FE	YES	YES	YES
Demographic Controls	YES	YES	YES
Observations	741	732	721
R-squared	0.609	0.526	0.322

An observation is a complier who answered the March 4 survey. Each column is a regression on a set of indicator variables for the main factor that an individual believed drives the value of their asset on March 4. The excluded category is that the asset's value is determined by companies' management. In Column 1, the individual's voting decision in 2015 is ranked (0) Right (0.5) Center/ Other (1) Left. All regressions include strata fixed effects and full set of controls from Table 3, Col 2. Robust standard errors in parentheses.

Table B18: Social and Business Attitudes towards Israeli Arabs

	N	Mean	SD	Treatment Effect	SE	(Pseudo) R ²
<i>The following refer to relations between Jewish and Arab citizens of Israel [1- disapprove, 2- tend to disapprove, 3- tend to approve, 4- approve]</i>						
Arab parties will be part of the governing coalition [O.Probit]	1,279	2.088	1.050	0.128	(0.078)	0.174
Social Relations Index [OLS]	1,279	0.005	0.987	0.021	(0.055)	0.391
Arabs will live in Jewish neighborhoods [O.Probit]	1,279	2.177	1.039	0.016	(0.075)	0.166
Arabs will attend Jewish high schools [O.Probit]	1,279	2.245	1.086	0.034	(0.077)	0.195
Business Index [OLS]	1,279	0.009	0.983	0.013	(0.056)	0.354
Arabs and Jews will form joint businesses [O.Probit]	1,279	2.767	1.026	-0.010	(0.075)	0.161
Arabs will manage Jewish-owned companies [O.Probit]	1,279	2.548	1.081	0.078	(0.074)	0.138

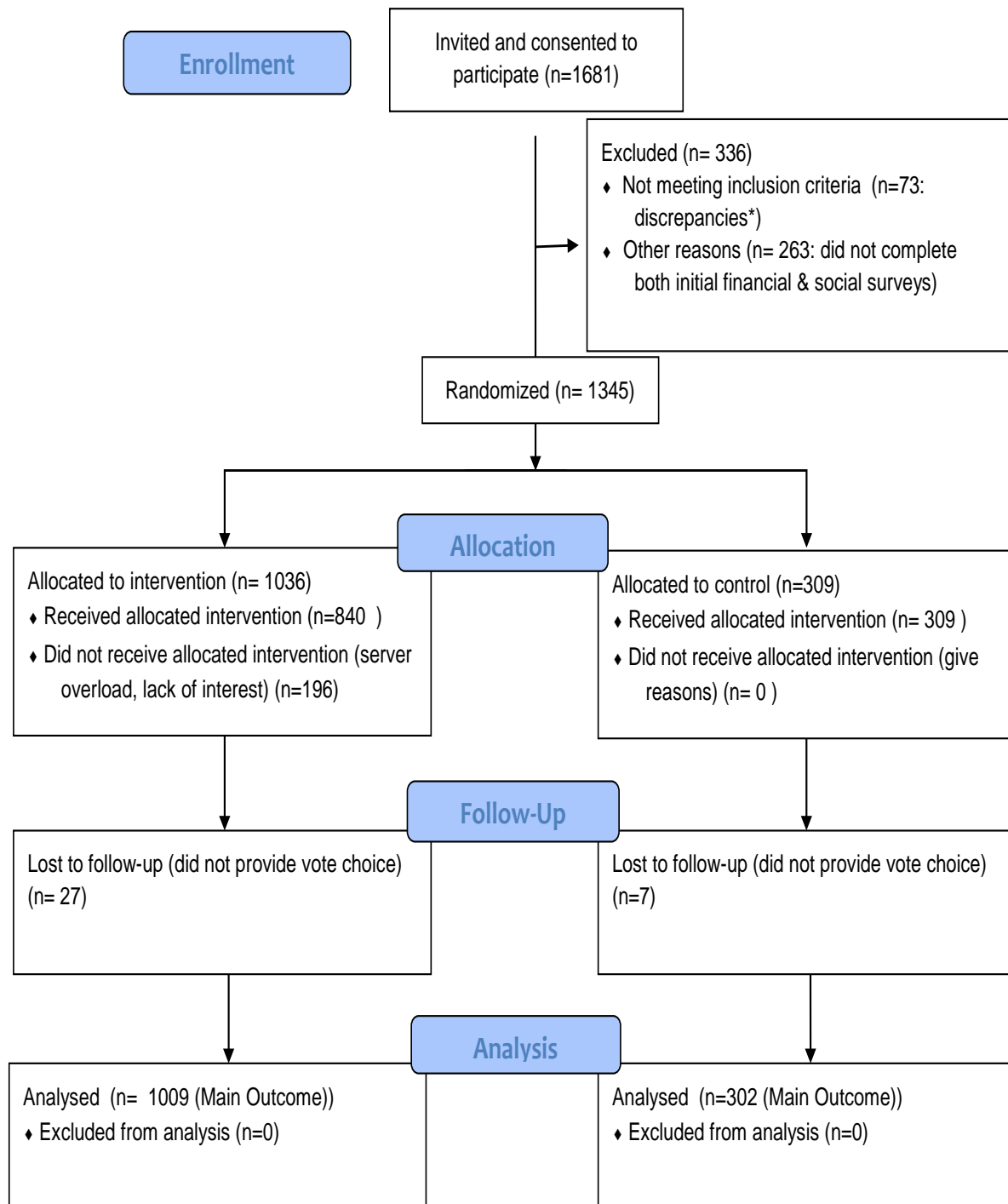
Notes: The table reports the treatment effects on a series of questions on social and business attitudes towards Israeli Arabs. Each row reports either an OLS regression on a Z Score Index, following Kling et al 2007, or an ordered-probit regression on the component dependent variables indicated in the first column. The social relations questions are taken from Smootha (2013, 2015). Among the Jewish population in 2012, he finds that the proportions approving mixed neighbourhoods were 55% and on mixed schools 46%. The business questions are our own. All regressions control for the full set of strata FE and controls from Table 3, Column 2. Robust standard errors in parentheses.

Table B19: Additional Questions from the post-Election Survey

	N	Mean	SD	Treatment Effect	SE
<i>To which of the following groups do you most belong [1-most, 2- second most, 3- other]</i>					
Israelis	1,286	1.753	0.844	-0.065	(0.081)
Jews	1,286	1.968	0.877	-0.012	(0.080)
Arabs	1,286	2.939	0.264	-0.226	(0.162)
Secular	1,286	2.558	0.713	0.021	(0.092)
Traditional	1,286	2.870	0.437	-0.348	(0.136)
Religious	1,286	2.856	0.434	-0.313	(0.129)
Ultra Orthodox	1,286	2.838	0.472	-0.241	(0.152)
Rich	1,286	2.940	0.262	-0.241	(0.152)
Middle Class	1,286	2.637	0.675	-0.033	(0.090)
Poor	1,286	2.905	0.375	-0.265	(0.146)
Sephardi	1,286	2.876	0.433	-0.160	(0.130)
Ashkenazi	1,286	2.867	0.447	-0.183	(0.126)
New Immigrants	1,286	2.929	0.304	-0.276	(0.151)
<i>And how proud are you of the following groups? [1- Not Proud at all, 4- Very Proud]</i>					
Israelis	1,282	2.975	0.834	-0.025	(0.077)
Jews	1,282	3.293	0.797	-0.072	(0.079)
Arabs	1,282	1.696	0.706	0.112	(0.077)
Secular	1,282	2.916	0.775	0.070	(0.074)
Traditional	1,282	2.832	0.719	-0.055	(0.076)
Religious	1,282	2.562	0.834	0.015	(0.074)
Ultra Orthodox	1,282	1.925	0.949	-0.054	(0.079)
Rich	1,282	2.196	0.807	0.035	(0.074)
Middle Class	1,282	2.905	0.759	0.019	(0.075)
Poor	1,282	2.405	0.930	-0.059	(0.075)
Sephardi	1,282	2.676	0.873	-0.014	(0.074)
Ashkenazi	1,282	2.772	0.779	-0.026	(0.074)
New Immigrants	1,282	2.849	0.828	-0.041	(0.073)
<i>To what extent do you agree or disagree with the following sentences? [1- do not agree, 4- agree]*</i>					
I would rather live in the state of Israel than in any other country in the world.	1,281	3.297	0.889	-0.060	(0.084)
When Israel wins some big achievements in fields e.g. sports, science and economics, I feel proud	1,281	3.411	0.790	-0.032	(0.084)
Should the new government increase budgetary support of isolated settlements? [1- reduce a lot, 3- keep the same, 5- increase a lot]	1,276	2.283	1.265	0.044	(0.077)
<i>Here are some more questions about the conflict between Israel and the Palestinians and Israel's positions in the region. To what extent do you agree or disagree with the following statements: [1- do not agree, 4- agree]*</i>					
The Palestinians are the main culprits in the long conflict between them and the Jews.	1,276	2.994	0.941	-0.106	(0.076)
Israel should integrate with the West and maintain only necessary contacts with Arab States.	1,276	2.708	0.850	-0.039	(0.076)

The table reports the treatment effects on all remaining questions from the post-election survey (Final Social Survey, March 19). Each row reports the treatment effect from an ordered-probit regression with the dependent variable indicated in the first column. All regressions control for the full set of strata FE and controls from Table 3, Col 2. Robust standard errors in parentheses. Due to a glitch in the administration of the survey, some participants entered inadmissible numbers as responses to these questions, making these responses hard to interpret. *: These two questions taken from Smootha (2012).

Figure B1: CONSORT Diagram



*=The main reason for screening out was extremely quick completion of the survey, which could raise a concern regarding the reliability of the responses. Specifically, the initial financial survey included 33 questions and we screened out 53 subjects who completed the entire survey in less than 180 seconds (the median completion time was 461 and the mean was 600 seconds). The remaining 20 individuals were screened out due to incomplete or inconsistent answers. In particular, we screened out 14 respondents whose answer to our question about voting in the 2013 elections was different enough from the answer in the survey company's database to move them from right to left blocks or vice versa.

Figure B2: Asset Prices in Context, 2012-2016.

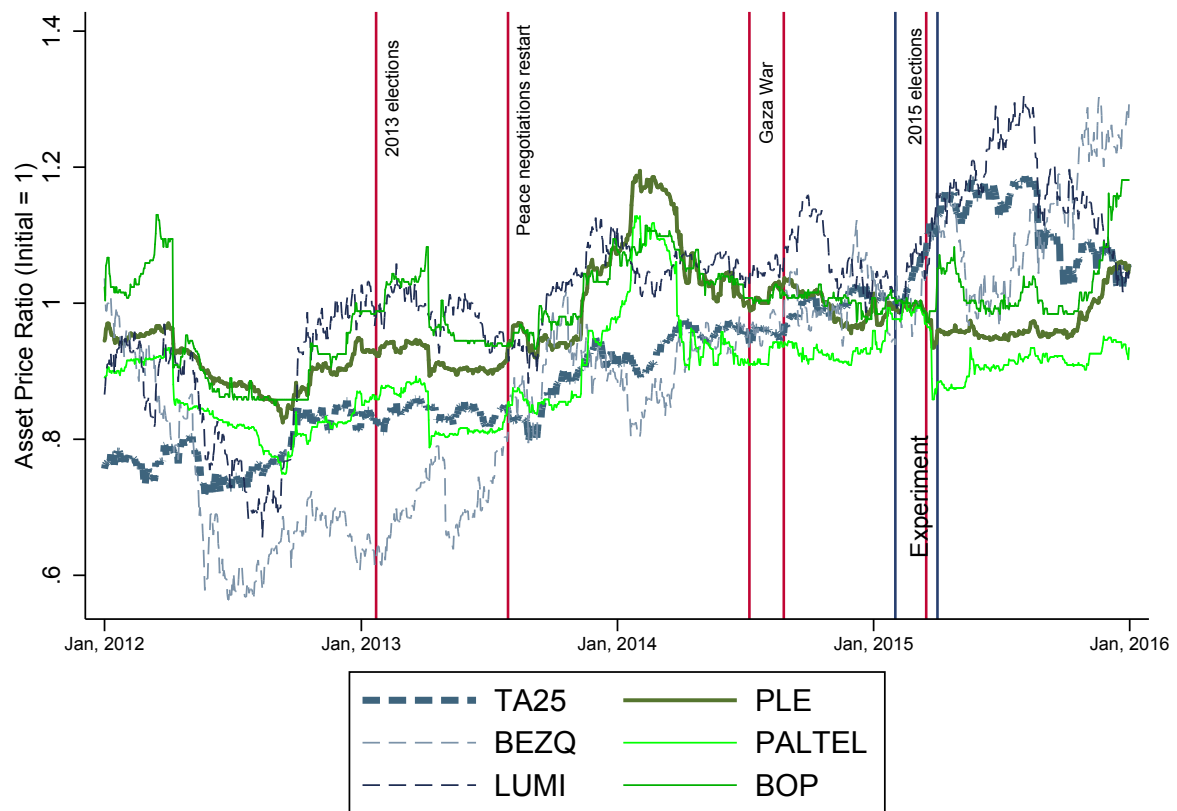


Figure B3: Initial Allocation Screen: Example.

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

• המניות כוללות בנקים וחברות תקשורת.
• המדדים עוקבים אחר הערך של כמה מהחברות הגדולות בכל מדינה (בדרך כלל מדד מסוים 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 ירד - ערך הנכסים שלך ירד בהתאם.

הרשימה מסודרת בסדר אלפביתי לפי סימול המניה או המדד באנגלית.

שם	שם באנגלית	סימול	מטבע	מחיר הנכס היום (במטבע מקומי)	מספר המניות שברשותי	ערך הנכסים שלי / כנ"ב (בש"ח)	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			
בזק (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 ליאסנא	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 בשגם	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	XU030	TRY	106,359.21			
כסף מזומן	CASH	CASH	ILS	1.00			

total value in NIS total value in JOD # shares current price in JOD

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

Figure B4: Weekly Trading Screen: Example.

The screenshot displays a trading interface with the following sections and callouts:

- Link to website with info on assigned stock:** Points to a URL: <http://il.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 "קניה" (Buy) section.
- Selling decision (if current portfolio includes stocks):** Points to the "מכירה" (Sell) section.

Text on the screen:

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.
 לדוגמא, באתר הבא: <http://il.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 נכסיך הוא 201.5 ש"ח.

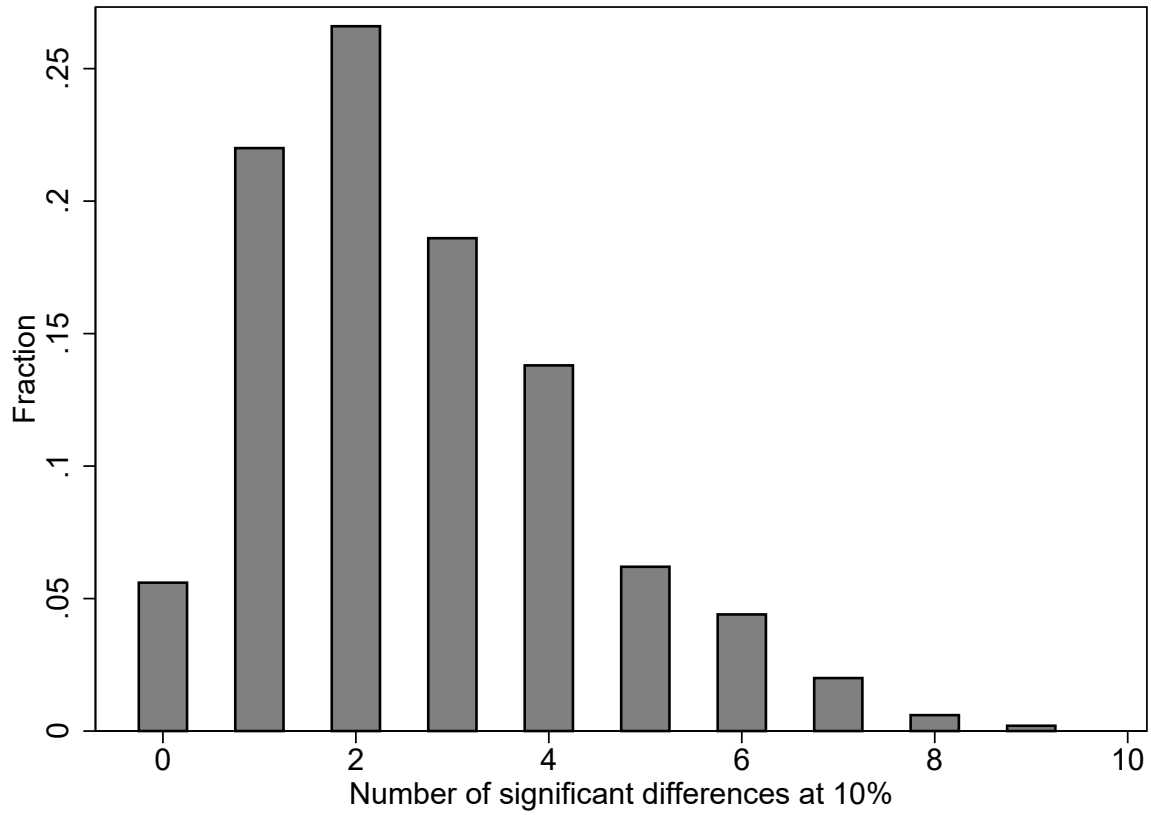
החלטות ההשקעה שלך
 אנא חזן להלן את החלטות הקניה והמכירה שלך. קניה ומכירה של נכסים אינן כרוכות בעמלה.

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 (נא להזין מספרים שלמים בלבד)
 אם אינך מעוניין למכור את המניות או חלקן, הקלד אפס.

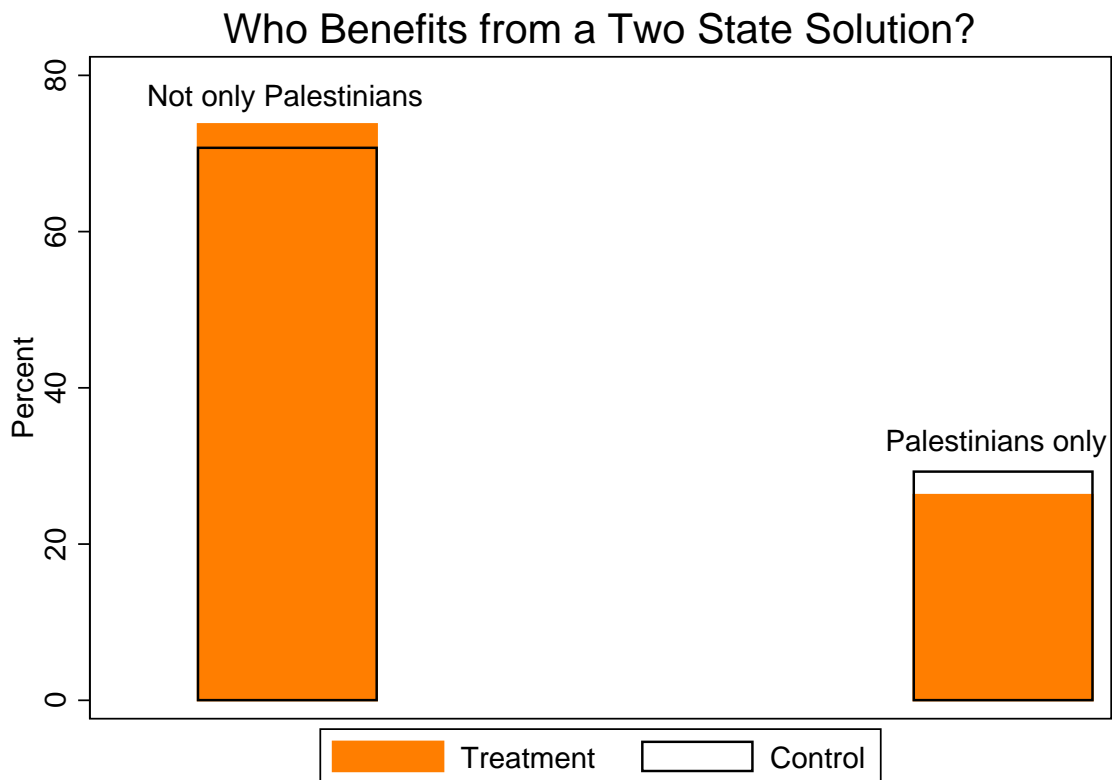
המשך

Figure B5: Balancing Tests Simulations



The figure reports the results from 500 simulations. In each, we randomly assign the sample of 1311 individuals in Tables 2 and 3 to fictitious treatment and control groups, with the same proportions as those of the actual groups. We then perform the tests reported in columns 3-4 in Table 2 and count the number of significant differences. The figure shows the distribution of the number of differences significant at the 10% level.

Figure B6: Is a Peace Settlement Zero Sum? Long-Term Differences in 2016



In the 2016 follow-up survey we asked who would benefit from a permanent settlement based around a two state solution. As the Figure reveals, 29.27% of the control believed that a settlement would benefit only the Palestinians– this falls to 26.27% in the treatment group.

Figure B7: Trading Activity Outside the Experiment



The figure shows, for each weekly survey, the share of compliers who say they have either bought or sold domestic or foreign stocks in the preceding week, apart from any trading done as part of the study. The top two graphs show inexperienced participants, namely those who have not traded in financial assets in the six month preceding the experiment. The Bottom two graphs show experienced participants.