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

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Abstract

Financial markets expose individuals to the broader economy. Does participation in financial markets also lead citizens to re-evaluate the costs of conflict, their views on politics and even their voting decisions? Prior to the 2015 Israeli elections, we randomly assigned financial assets from Israeli and Palestinian companies to likely voters and gave them incentives to actively trade for up to seven weeks. Exposure to financial markets systematically shifted vote choices and increased support for peace initiatives. We delineate the mechanisms for this change and show that financial market exposure led to learning and reevaluation of the economic costs of conflict.

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1 Introduction

Public attention in societies facing violent conflict is often focused 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*—leads 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.[1] Indeed, the hypothesis that market exposure affects political attitudes, particularly with respect to conflict, is very old, dating back at least to Montesquieu (1748). As Condorcet put it in 1795, "the spirit of industry and commerce... is inimical to unrest and violence as the natural enemies of wealth" (Condorcet, 2012, p. 91). Theoretically, financial markets may change political attitudes as they can demonstrate the shared returns to the economy from peace and the risks from conflict. 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. We do this in the context of a geopolitically important and highly persistent ethnic conflict—that between Israelis and Palestinians. This is a challenging setting: conflicting interests and distrust reinforced by more than eighty years of recurrent violence have produced seemingly entrenched ethnic animosities, to the point that many consider the conflict

[1] See Blattman and Miguel (2010) and World Bank (2011). In the Israeli-Palestinian context, the Rand Corporation estimates that a two-state solution will yield Israelis an economic dividend of $123 billion over ten years, and Palestinians $50 billion (Anthony et al., 2015). A return to widespread conflict would lower Israeli per capita GDP by 10% and Palestinian by 46% over the same period. Similarly, Eckstein and Tsiddon (2004) estimate that reduced investment and reallocation of resources due to conflict reduced the level of Israeli GDP per capita by 10% during the Second Intifada (2001- 2004) alone.

intractable.

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 endowments of assets that tracked the value of specific indices or company stocks from both Israel and the Palestinian Authority, or an endowment of cash to invest in stocks. 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. We cross-randomized the dates at which individuals would be divested of their portfolio to be either before or after the elections, and randomly assigned the initial value of the portfolio (either NIS 200 (∼\$50) or NIS 400 (∼\$100)).

Individuals also participated in a parallel series of surveys that allowed us to track not 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. Section 3 details how this was achieved and verified.

Our main result (Section 5) is that exposure to financial markets causes large and systematic shifts in individuals' actual vote choices in the 2015 elections.[2] Exposure to the stock market increases the probability of voting for parties that support restarting the peace process—known in Israel as the *left*—by 4 to 6 percentage points (relative to their vote share of 25% in the control group). It similarly reduces the probability of voting for parties skeptical of peace negotiations—the *right*—by about 4 to 5 pp (relative to 36% in the control). Consistent with random assignment, these estimates are unaffected by controlling for individuals' vote choices in the recently held 2013 elections, education, income levels, region, religiosity, risk and time preferences, initial financial literacy and

[2] A desirable feature of the Israeli setting from an academic perspective is that the entire country comprises a single constituency of 5.9 million eligible voters. Thus our study had no effect on the election outcomes themselves.

other characteristics. In terms of magnitude, these effects are comparable to recently estimated effects of changes in security risks on Israeli voters (Berrebi and Klor, 2008, Getmansky and Zeitzoff, 2014).

In Section 6 we exploit the sub-treatments and detailed survey questions to shed light on the underlying mechanisms. We start with the two main alternatives: that the exposure to financial markets gave participants a direct material incentive to change their vote, or that it changed their policy preferences. 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 an incentive to vote for parties that favor the peace process. Inconsistent with the material incentive 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 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. Further, these effects on attitudes are specific to the peace process: there is little evidence that individuals' preferences over other policies shift leftward.

We next consider two key sets of reasons why policy preferences could change. First, we find no evidence for either a wealth effect or an effect on individuals' mood or subjective well-being. Second, however, we do find evidence consistent with two forms of learning. Treated individuals become more financially literate. Financial literacy has been associated with a number of beneficial welfare outcomes (Lusardi and Mitchell, 2014), but, surprisingly, experimentally assigned education programs have had rather limited effects on financial literacy on average (see Hastings, Madrian and Skimmyhorn, 2013). We find that allowing individuals to trade in the stock market in a simplified way significantly raises their financial literacy scores. They also report being more familiar with the stock market. In addition, when evaluating the effects of a peace agreement with the Palestinians, individuals exposed to financial markets predict better outcomes for Israel's economy. This effect is greater for the *risk-averse*, suggesting that treated individuals perceive greater risks associated with status quo policies relative to the risks of negotiating for peace. Consistent with learning, we also find that treated individuals become more likely to follow financial media.

A further question is whether the treatment effects are transitory, perhaps reflecting short-term attention to economics, or whether they persist and even cumulate over time. We find suggestive evidence that the effects of financial market exposure on voting intentions and support for peace concessions persist a year after the intervention. In fact, differences between treatment and control show up even after controlling for individuals' 2015 (post-treatment) positions. This is consistent with individuals continuing to learn.

Finally, we examine the differences between holding in-group (Israeli) vs out-group (Palestinian) assets. On the one hand, out-group assets could have larger effects as they expose individuals to new sets of considerations and shared risks, and generate more of an opportunity for learning. 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, individuals exposed to 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 ended up being quite similar.

An important feature of our intervention is that, unlike campaigns that distribute potentially contentious information that might be perceived as propaganda, our intervention is unobtrusive and non-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 quite intensive, it does not require prohibitively high stakes or long durations: assigning $50 worth of assets appears 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 other party to the conflict, seem to raise the potential for implementing the intervention at scale and in a wide range of settings.

Beyond the substantive contribution, our paper makes two methodological contributions. First, we innovate relative to the existing finance literature by implementing random assignment to empirically identify the causal effects, not only of exposure to financial assets but also of opportunities to trade those assets, on individual political behavior, knowledge and attitudes. We develop a simplified trading platform that allows inexperienced individuals to trade in 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).[3]

Second, we use double-blinded samples in parallel surveys in order to measure treatment effects. This mitigates problems that arise when subjects modify their self-reports in response to the treatment (see Podsakoff et al., 2003 for a discussion of common biases in this class). Our approach provides a useful addition to existing methods of addressing this problem which include the use of filler questions to distract individuals from the purpose of the study, list experiments, or proxy outcome measures like the Implicit Association Test that are considered less susceptible to conscious processes. Our use of online panels can be scaled easily, particularly as internet penetration expands, reach broad representative samples, and be applied to questions quite removed from the political economy of conflict.

[3]The existing literature on the effects of financial market exposure on political attitudes exclusively uses observational data. The closest paper to our's, substantively, is Jha (2015), who exploits the coincidence of individual politicians' abilities to sign legally binding share contracts with novel share offerings by overseas companies to identify 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, though not exposure to trading in the stock market. Methodologically, the most closely related paper is Bursztyn et al. (2014), who assign a financial asset randomly among those that chose to purchase it through a brokerage firm, and find that this has effects on take up by peers.

This paper naturally links to the literature on economics and ethnic conflict. Places that are more ethnically polarized tend to face more conflict (Montalvo and Reynal-Querol, 2005), and conflict intensifies ethnic biases (Shayo and Zussman, 2011, 2017). This can generate vicious cycles in which ethnic biases and conflict reinforce each other and persist (Sambanis and Shayo, 2013). Yet, at least as early as Montesquieu (1748), economic interests from capitalist activity have been seen as means to offset *passions* that can lead to violence (Hirschman, 1977). Indeed, securing peace was a major motivation for European economic integration, and bilateral trade between countries is negatively associated with conflict (Martin, Mayer and Thoenig, 2008). Even within countries, complementarities that stimulate exchange between ethnic groups appear to mitigate violence and to foster cultures of tolerance (Jha, 2013, 2014).[4] However, to the best of our knowledge, the causal effects of market interaction on choices and attitudes towards conflict have not been studied at the individual level. Moreover, unlike the previous literature which tended to emphasize direct economic incentives, we show that exposure to financial markets can affect policy preferences even without creating a significant new personal financial stake in peace.

Before proceeding, a word of caution. This is but the first paper to measure the effects of exposure to financial markets on voting behavior and attitudes towards peace initiatives. Replication and extensions of this work are vital – both to examine under what conditions the results replicate, to evaluate the potential for scaling up, and to shed further light on the mechanisms at play.

[4]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, Jha, Mitchener and Takashima, in progress). For a useful comparative analysis of financial development in these settings, see also Rousseau and Sylla (2008).

2 Context

Our study focuses on the March 2015 Israeli general elections. Israel is a parliamentary democracy with proportional representation. Elections must be 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 (Appendix Figure A1). This recent history is also valuable because the 2013 elections provide a recent measure of participants' (pre-treatment) vote choices. We focus on Jewish voters, who comprise close to 80% of the voting population.

It is important to note that, rather than economic policies, the main dividing line between the right and the left political parties in Israel focuses on the Israeli-Palestinian conflict. This also shows up very clearly in our sample.[5] Parties on the right (led by the *Likud*) largely favor the status quo, viewing concessions for peace as highly risky and likely to lead to a major deterioration of the security situation.[6] In contrast, parties on the left (led by the Zionist Union) see 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 they favor restarting the peace process with the goal of finding a permanent solution to the conflict.[7] While many parties can be clearly classified as left or right, other parties—which we will refer to as *center*—could in principle join a

[5] In an OLS regression of ordered vote choice in 2015 on pre-treatment indices of individual attitudes towards peace concessions and towards economic policies (these measures are explained below), both indices are highly significant, with an R^2 of 0.296. However, of this R^2, the peace index is responsible for 0.279 (or 94.1%), while the economic policy index only accounts for 0.016 (or 5.4%).

[6] This *holdup* problem is arguably a common feature in a number of other conflicts (Fearon, 1996).

[7] On the eve of the elections, on March 16 2015, the leader of the *Likud* party, 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). By contrast, the platform of the Zionist Union stated that "reaching a diplomatic settlement [of the conflict] is a foremost Israeli interest and a necessary condition for securing the future of the state of Israel as a Jewish and democratic country" and called for restarting negotiations "with the aim of reaching a permanent settlement with the Palestinians, based on the principle of two states for two peoples."

coalition led by either the *Likud* or the Zionist Union.

3 Experimental Design

We recruited 1681 anonymous individual participants from among Jewish Israeli citizens who had previously voted and who participate in a large Israeli internet panel. This panel of about 60,000 participants is nationally representative in terms of age and sex, and is commonly used for commercial market research, political polling and academic studies. The panel also has a particularly useful feature: anonymity in the identity of the respondents from our perspective, and anonymity of the originators of different surveys from the respondents' perspective. This feature allows us to avoid social desirability biases that often plague research on peace-building initiatives.

Individuals were invited to a study on investor behavior, and told that they would be participating in several surveys and would be asked questions on various issues. They were informed that they would be entered into a lottery to win either stocks or cash to invest, and that the stocks participating in the study would be from the entire region.[8] Among those that consented, we conducted two parallel sets of surveys. Everyone received a set of surveys gauging their social and political attitudes, and separately, their financial knowledge and economic preferences. In addition, those that won the lottery received a survey each week in which to make their financial investment decisions.

Importantly, the surveys were designed so that participants did not associate the social surveys with the financial surveys. This was achieved by three features. First, as mentioned above, our surveys were anonymous: they were among 110 sent to panelists by anonymous sources during February and March. 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 we

[8] To avoid 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.

selected to participate in the study were broad indices or the stocks of bricks and mortar banks and telecoms companies rather than holding companies, companies with extensive business in the West Bank or companies with overt ties to national defense.[9]

To verify whether these measures were effective, we asked our participants an open-ended question on what they believed "the researchers can learn from the study" in the concluding investment survey. The results are in Figure 1. 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.

[**Figure 1**]

As our main interest is in political behavior, we limited survey invitations to those that had voted in the past. We further over-sampled non-orthodox center voters[10] at twice their vote share (see also Figures 3 and 4). These swing voters are arguably the most politically relevant since they often determine the electoral outcome. All respondents were asked to fill out an initial financial survey on investment behavior and financial literacy. These included their prior investment history (including whether they had traded stocks in the last six months), and a battery of questions measuring financial literacy, adapted from Van Rooij, Lusardi and Alessie (2011), risk aversion and time preference (from Dohmen et al. (2011) and Benjamin, Choi and Strickland (2010)). A few days later they were invited to answer an initial social survey which included questions on political behavior, social and political attitudes, and well-being (from Benjamin et al. (2014)). Of the 1681 who completed the initial financial survey, 1418 completed

[9]The only defense company in the Tel Aviv 25 (TA-25), Elbit Systems, has a weight of only 3.26%.
[10]That is, individuals who voted for the secular parties: *Yesh Atid, Hatnu'ah* or *Kadimah* in 2013.

the initial social survey as well. Based upon the initial surveys, we screened out those who provided incomplete answers, had been grossly inconsistent when asked the same factual questions at different times, or had completed the survey extremely quickly (see Figure A2 for details). This left 1345 participants to randomly assign to the various treatments. The combined outcome of this sampling strategy is that the sample used for random assignment approximates the broader Jewish population of Israel in terms of geographical region and sex, but tends to be more educated and more secular, with fewer individuals over 55 and in the top-most income deciles (Table A1).

Among these 1345 respondents, we employed a stratified block randomization procedure designed to increase balance across treatment groups in political and demographic variables.[11] A sample of 309 were assigned to the control group, and 1036 were assigned to the asset treatment. Further, to help understand the mechanisms involved, participants within the asset treatment were initially endowed with either cash to invest, stocks from Israel or stocks from the Palestinian Authority, each of high or low initial value, and each with redemption date either before or after the elections. The following table summarizes the basic design and initial allocation.

	Total	Redeem pre-elections			Redeem post-elections		
		All	NIS 200	NIS 400	All	NIS 200	NIS 400
Asset Treatment	1036						
Cash to invest	206	64	32	32	142	71	71
Israeli Stocks	414	141	70	71	273	136	137
Palestinian Stocks	416	141	71	70	275	137	138
Control	309						

Every week, participants in the asset treatment could reallocate up to 10% of their holdings by buying or selling a particular financial asset, commission-free. This limit was chosen to encourage individuals to learn by doing rather than simply choosing their entire portfolios immediately. To further incentivize engagement with the stock market,

[11]Specifically, we created 104 blocks of 13 (less for one block), with the blocks created to stratify 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, discrepancies in their reported voting in the 2013 elections and a measure of their willingness to take risks. This creates relatively homogeneous blocks. Within each block we then randomize individuals into the subtreatments.

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.

The 830 individuals who were initially assigned stock endowments could sell (and later buy back) a specific stock or index fund. Of these, 414 were assigned assets from Israel, evenly and randomly distributed between the Tel Aviv 25 Index as well as stocks from a commercial bank—Bank Leumi—and a telecoms company, Bezeq. The remaining 416 were assigned assets from the Palestinian Authority, distributed evenly between the Palestine Stock Exchange General Index as well as stocks from a commercial bank—the Bank of Palestine—and a telecoms company, PALTEL.[12] 202 of the individuals assigned an initial endowment of cash could buy (and later sell) an asset that tracked the Tel-Aviv 25 Index, while the remaining four traded for indices from Cyprus, Egypt, Jordan and Turkey.[13]

About 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. The 10% trading limit also ensured that just before the elections, each subject's portfolio included at least 66% of the experimentally assigned asset. Finally, about half of the participants in the treatment group were given assets

[12]The specific companies were selected along two criteria: lack of overt connection to the Israeli-Palestinian peace process and comparability. PALTEL is the largest private employer in the Palestinian Authority, while Bezeq was the former Israeli state telecoms monopoly. The Bank of Palestine is the Palestinian Authority's largest commercial bank, while Bank Leumi literally means "National Bank", and is one of the two largest banks in Israel.

The assets were in fact a derivative claim on the authors' research funds rather than an actual purchase of the underlying asset. This also meant that the study could not affect the asset prices directly even for those that are thinly traded. Since the Palestinian and other 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 the Palestinian or other cross-national stocks. We disallowed short sales.

[13]We considered assigning more individuals to *neutral* stocks, such as the Cyprus, Jordanian and Turkish market indices and even the S&P 500. However, as our main motivation was to study the effects of holding financial assets that allowed individuals to learn about the economic costs of conflict, our first priority was to study the effects of exposure to the Israeli and Palestinian asset markets. Since assignment to neutral stocks would have been at the expense of these treatments, we ultimately had to limit this exposure to 4 individuals. We maintained these 4 assets to be consistent with the information provided to participants, that the stocks participating in the study are from the entire region (see footnote 8).

initially valued at NIS 200 (around \$50), with the rest valued at NIS 400 (around \$100). While these sums are not large—they are comparable to the average Israeli *daily* wage of around NIS 312 in December 2014—they are quite significant compared to the standard pay of NIS 0.1 per question these participants receive for our and other surveys, as well as relative to typical stakes in experimental economics.

All members of the treatment group were invited to an instructions survey in which they were informed of their asset allocation (Figure A3), given detailed explanations about the rules of the game, and quizzed to make sure they understood how the value of their assets would be determined. 840 participants completed the instructions survey and agreed to continue. The incomplete takeup probably reflects some self-selection as well as differential willingness to hold different assets. Not surprisingly, the lowest takeup was for the low (NIS 200) assets (77.2%, 78.4% and 78.6% for Israeli, Palestinian and cash endowments respectively). For the NIS 400 assets, cash had the highest takeup (91.3%), followed by Israeli (86.1%) and Palestinian (78.8%). Anticipating this, we took special care to survey the outcomes of non-takers so we can estimate both Treatment on the Treated (TOT) and more conservative Intent to Treat (ITT) effects. The latter measure the effect of being assigned to treatment whether or not an individual actually took up the assets. For TOT we use the random assignment to treatment as an instrument for actual treatment.

The 840 participants who completed the instructions survey received weekly updates about the price of their assigned asset and a statement of the composition and current value of their financial portfolio. This was sent out after markets closed on the last business day of the week (usually on Thursdays). 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. Participants were then asked to make their investment decisions and had until the opening of the stock market the following week to do so. All trades were implemented via a trading platform incorporated into our surveys (Figure A4).[14] 69% of the 840 participants entered a trading decision at every opportunity they had and 80% did so in all but one week. Figure 2 provides a timeline of the surveys and shows the performance of the assigned stocks over the course of the experiment. As it turned out, the returns on the Israeli assets were consistently higher than the returns on Palestinian assets during our intervention.

[**Figure 2**]

Two days after the elections we surveyed all individuals on their vote choice as well as attitudes towards the peace process. This provided data on the vote choice of 1291 participants. For the voting data, we were further able to augment and compare these responses to the participants' routine updates to the survey company on their demographic and voting data, as well as to our own (anonymous) information survey in April 2015. There were very few discrepancies among the three, again consistent with an absence of social desirability bias in responses.[15] As a result, we benefit from very little attrition in our main outcome variable: we observe the vote choice of 1311 out of the 1345 initially assigned to treatment (97.4% of asset treatment group and 97.7% of the control, Table A3).[16]

4 Data

Table 1 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

[14]Specifically, 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.

[15]Of 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 coefficient on asset treatment from a regression of the probability of reporting a matching vote in the two surveys is -0.008 (SE=0.0144).

[16]There was slightly higher attrition on the questions measuring attitudes towards the peace process, with a response rate of 95% (1277/1345).

vote outcome. Column 3 reports the raw mean difference while Column 5 reports mean differences within the 104 stratification bins. As expected from stratified random assignment with low attrition rates, 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.[17]

[Table 1]

Attitudes towards making concessions for peace at baseline, and attitudes towards left or right economic policies—measures that we will 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 literacy scores (based upon questions adapted from Van Rooij et al. (2011)). Two variables show small but statistically significant differences. Individuals in the asset treatment are somewhat younger on average (39.3 vs 41.5 years old) and consider themselves to be slightly more willing to take risks (an average of 4.7 on a 1-10 scale, compared to 4.3 in the control). We control for these and other demographic variables in our regressions (including a quadratic for age).[18]

[17] Right wing parties in 2013 include *Likud Beyteynu* and *Habayit Hayehudi*. Left wing parties include *HaAvoda* (Labor) and *Meretz*.

[18] These 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. 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 1 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 and count the number of significant differences. We repeat this procedure 500 times. Appendix Figure A5 shows the

5 Main Results

We begin with our central question: does exposure to financial markets change political choices? Figure 3 shows the raw vote shares across the asset treatment and control groups. The left panel shows vote shares in the 2013 elections (prior to our intervention). Consistent with Table 1, the treatment and control groups have similar distributions of votes across left, right and center parties in 2013. Voting decisions in 2015 (right panel), however, reveal substantial differences. While 24.8% of the control voted for the left (a proportion similar to the 25.3% overall vote share for Jewish left parties in the 2015 elections), the left won 30.9% of votes among the treatment group. At the same time, right parties won 31.2% of the votes of the treated group, compared to 35.8% in the control.[19]

[**Figure 3**]

Figure 4 shows the detailed party vote. Again, the treatment and control groups have very similar distributions in the 2013 elections (note the oversampling of the centrist *Yesh Atid* party), but show notable differences in the 2015 elections. These mainly reflect a lower vote share in the treatment group for the major right-wing party, the *Likud*; a lower vote share for the centrist *Yesh Atid*; and a higher vote share for the left parties, the *Zionist Union* and *Meretz*.

[**Figure 4**]

Table 2 presents estimates of the treatment effect on the probability of voting for the left (Cols 1-4) and the right (Cols 5-8) in the 2015 elections. For the most part we

distribution of the number of significant differences at the 10% level across simulations. Less than 6% of the simulations have zero significant differences and less than 28% have less than two (the number we obtain). The number of differences significant at the 10% level ranges from 0 to 9, with an average of 2.64 across simulations. The number of differences significant at the 5% level ranges from 0 to 7, with an average of 1.28.

[19]The Left parties in 2015 are the Zionist Union, *Meretz* and the Arab Joint List. The Right parties are *Likud, Habayit Hayehudi, Israel Beytenu & Yachad-Ha'am Itanu*. Center parties are *Yesh Atid, Kulanu, Shas* and *Yahadut HaTorah*. There can be some disagreement about the designation of Ultra-Orthodox parties—*Shas* and *Yahadut HaTorah*—as center parties. Therefore our analysis largely focuses on voting for unambiguously left and right parties.

report Intent to Treat (ITT) estimates, not only because they are more conservative, but as they are important when one is interested in the treatment effect taking into account that some individuals may not participate.[20] Consistent with random assignment, the raw mean treatment effects (Cols 1 and 5) are essentially unaffected when we add controls for other factors that may shape vote choices (Cols 2 and 6), although the estimates become more precise. 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 controls include vote choices in the recently held 2013 elections, prior experience in trading stocks, sex, age (and age squared), categorical variables for levels of education, income, religiosity, geographical region and marital status, pre-treatment measures of willingness to take risks, patience and financial literacy, as well as 104 strata fixed effects. That these controls are meaningful determinants of vote choice is reflected in the increase in R^2 from 0.003 to 0.45 for the decision to vote left, and from 0.002 to 0.52 for the right.

[Table 2]

As explained above, we oversampled center (swing) voters at twice their vote share in 2013. Columns 3 and 6 re-weight the sample to reflect the actual vote share of Jewish parties in 2013. The point estimate is smaller (a 4.3 pp increase) for the probability of voting left, but larger (a 5.1 pp decrease) for voting right. This reflects 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 A4). Thus, by reducing the relative weight on *ex ante* center voters, we also put less weight on those that move from the center to the left and more on those that move from the right to the center.

[20] Unless explicitly stated, we use heteroskedasticity-robust standard errors. The clustering problem does not arise in our benchmark specifications since we randomize at the individual level. Thus the Moulton factor is 1. Bootstrapping the standard errors for the main specifications in Columns 2 and 6 (in Stata with seed 111111 and 500 replications) yields an estimate on the left of 0.059 [0.0235], and on the right of -0.044 [0.0238].

Finally, it is useful to also measure the treatment effect on those individuals that not only were assigned to the asset treatment but actually participated. 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.[21]

Henceforth, we summarize the voting decision in a single ordered vote choice variable, with values ranging from 0 for Right, 0.5 for Center or Other, and 1 for Left, paralleling Figure 3. Table 3, Panel A, presents the estimated treatment effect for the entire population. Cols 1-2 report the proportional odds ratios from an ordered logit regression on the unweighted and re-weighted sample. The odds of voting for a more left-wing block vs. a more right-wing block (e.g. left vs either center or right) are 1.47-1.49 times greater in the treatment than in the control. Columns 3-5 report OLS and 2SLS estimates. The linear effect on the ordered vote choice ranges from 0.052 leftward shift in the unweighted and 0.047 in the reweighted ITT, to 0.064 in the TOT (*p-values* equal 0.006, 0.013, 0.004, respectively). Panel B restricts attention to those who lacked experience trading in stocks in the six months prior to the experiment. Perhaps not surprisingly, the effects of exposure to financial markets tend to be higher for this group. We will return to this result below.[22]

[21] Appendix Table A5 repeats these exercises separately for the probability of voting for each party (as well as for not voting). The table also reports multinomial logit estimates of the vote choice. The overall patterns are consistent with Figure 4. The treatment significantly increases the likelihood of voting for the main left-wing party, the *Zionist Union*, by 4 to 5 pp in the ITT and TOT specifications, respectively. And it significantly reduces the likelihood of voting for the centrist *Yesh Atid* (by 3-4 pp) and for the main right-wing party, the *Likud* (by 4-5 pp). Again, reweighing the sample accentuates the negative effect on the *Likud* and attenuates the positive effect on the *Zionist Union*. There is no appreciable effect on turnout.

[22] Because we stratified on past experience, the strata fixed effects absorb much of the relationship between past financial experience and vote choice in Table 2. Table A6 removes the strata fixed effects. Two patterns emerge. First, even without the treatment, those that had past experience in the financial markets were 9-10pp more likely to vote for a left party in 2015, with this increased probability coming at the expense of the center, rather than the right. Second, the point estimates of the effects of financial market exposure on inexperienced traders tend to be larger, and mimic these patterns (an increase in 7-9pp on the left, with no effect on the right). Thus, it appears that the treatment leads those inexperienced in financial markets to become more like experienced traders in their political choices.

[**Table 3**]

As a useful robustness check, we can use 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. However, such a difference-in-difference analysis must be interpreted with caution. Between 2013 and 2015, there have been changes in the composition of parties and how they fit into the right-left spectrum.[23] Thus, voting "left" or "right" could mean different things in 2013 and 2015. While in the preferred specification in Table 2, we simply controlled for vote in 2013, a difference-in-difference analysis imposes the additional assumption that a left vote is the same regardless of year. With this caveat, Table 4 reports the results of this exercise. Our main interest 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. The effect on the ordered vote choice is unaffected by the inclusion of either individual controls or individual fixed effects (Cols 1-3). 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 4**]

6 Mechanisms

So far we have seen that exposure to financial assets moves individuals' votes in the 2015 elections left, towards parties that are more supportive of the peace process. We now exploit a rich set of sub-treatments and attitudinal measures to delineate the mechanisms

[23] For example, one of the main center parties in 2013, *Hatnuah*, created a joint list with the Labor 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 ethnic *Shas* party split, with offshoot *Ha'am Itanu* adopting an extreme right position.

through which this occurs. Table A2 reports balancing tests across sub-treatments. As before, sub-treatments are balanced relative to the control across almost all dimensions. We continue to control for pre-treatment characteristics, as in Table 2 (Col 2).

6.1 Economic incentives or changes in policy preferences?

We first evaluate two main alternatives: that the exposure to financial markets gave participants a direct material incentive to change their vote, or that it changed their policy preferences.

Peace overtures tend to raise both Israeli and Palestinian asset prices (Zussman et al., 2008). This may lead individuals holding stocks on Election Day to have a direct incentive to vote for parties that favor the peace process.[24] To test whether this is the case in our context, we employ four strategies that give us exogenous variation in the degree of asset exposure on election day (Table 5). First, we compare individuals who were exogenously divested of their assets the week *prior* to the elections to those who retained the direct material incentive by being divested after. Compared to the average effect of asset treatment on the ordered vote choice (Col 1), and to those divested pre-election, the effect on those divested post-election is *lower*, not higher (Col 2). This appears consistent with a stronger treatment effect of realized rather than paper gains and losses (see Imas, 2016), but in any case suggests the effect does not crucially hinge on holding assets on election day.

[Table 5]

Second, we compare individuals initially assigned a portfolio purely of stock to those given cash to buy stock. Given our trading restrictions, those endowed with stock still held at least 66% of their assets in stock on election day, compared to 35%, at most, for

[24] 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 A9).

those endowed with cash. However, the coefficient on the cash endowment treatment is not significant, and, if anything, suggests the effect is *higher*, not lower. Third, as we show in the next section (Table 7), receiving a high allocation, $100 of assets also does not have a significant effect beyond receiving $50.

Finally, in Table 5, Col 4, we examine the effects of the actual asset holdings of each participant on election day. As election day asset holdings are naturally endogenous to individual investment decisions, we generate an instrument for election day asset holdings based upon the portfolio of a passive investor who registered a decision every week to simply hold their initial asset allocation. This instrument combines all the exogenous features of the experiment that drive the value of stocks, including timing of divestment, high vs low allocation, stock vs cash endowment, and the price change of the underlying asset. Again, there is no evidence for an additional effect of actual stock holdings beyond the average treatment effect. Thus, a direct material incentive alone does not explain the results. It should be stressed, however, that this does not rule out potential incentive effects on political attitudes. It is possible, even likely, that our intervention simply provided too small a direct incentive. Our results thus accentuate the desirability of further studies with increased stakes, while pointing to an additional channel.

Rather than the direct material incentives provided by stockholdings, individuals might also change their vote choices if exposure to financial markets affects their policy preferences over the peace process or in other domains. We therefore asked our participants two sets of questions: on attitudes toward the peace process, and on their views on conservative vs liberal economic policies (see Table 6). The questions on the peace process are drawn from a national survey conducted since 2003 (Smooha, 2015). These include both a broad question on support for a two-state solution, as well as agreement with 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. Participants were asked whether they agree, tend to agree, tend to disagree or disagree on each question.[25] For economic policy attitudes, 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 gauging support for reductions in capital gains tax on investment in the Israeli stock market. We combine the two sets of questions into a *Peace Index* and an *Economic Policy Index*, following Kling, Liebman and Katz (2007), where higher values indicate more of a *left* position.

[**Table 6**]

Table 6 presents the overall effect of exposure to asset treatment on the two indices, as well as the effect component-by-component. Each regression includes the full set of controls from Table 2 (Col 2). 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 are stronger for the more specific and less widely accepted concessions, and, once again, the point estimates are more pronounced among those less experienced in financial markets prior to the experiment (Col 5). In contrast, the overall effect on the index of preferences over economic policies is insignificant, and if anything slightly negative, indicating that financial market exposure may have induced a slight move rightwards on these issues. This comes mainly from a change in policy preferences towards increased individual—rather than governmental—responsibility for addressing poverty.

To summarize, the effect of financial market exposure on voting decisions appears to reflect a change in policy preferences rather than any direct economic incentives, and the

[25] The proportions agreeing to 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 2003-4 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 Smooha (2015) for details.

change in policy preferences stems from attitudes towards the peace process, rather than economic policies.[26]

6.2 Why did policy preferences change? Wealth and affect versus learning

We now consider two sets of potential reasons why policy preferences may have changed: a change in wealth, mood or subjective well-being on one hand, and learning on the other.

Receiving a financial portfolio worth \$50 or \$100 could conceivably 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 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 7 (Cols 1,3,5) estimates the interaction of the asset treatment with an indicator for below average pre-treatment income on the vote choice, peace index, and economic policy index. While poorer individuals do support more left-leaning economic policies in our sample (Col 5), the interaction term shows no significant difference in the treatment effect for this group for any of these measures.

[**Table 7**]

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

[26] In the post-election survey, we also asked questions to assess individuals' acceptance of cooperating with and interacting with Israeli Arabs in political, social and business domains (Table A12). The point estimates of the average treatment effect are positive on all three domains, although the effect is statistically significant only for the acceptance of Jewish-Arab political coalitions.

$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. Immediately after the elections, we asked individuals not only about their overall life satisfaction but also a battery comprising the top predictors of well-being based on Benjamin et al. (2014, Table 2).[27] As Table 8 suggests, however, the asset treatment did not significantly change *any* individual indicators of subjective well-being or a combined index of all the outcomes. Taken together, our treatment effects do not appear to be due to a wealth effect or a change in mood or affective state.

[**Table 8**]

Instead, as the stronger point estimates of the effect on inexperienced investors suggest, exposure to financial markets in a simplified way may have overcome fixed barriers to learning that could explain the change in policy preferences. Figure 5 presents a histogram of responses to an open-ended question "What did you learn from the study?" among the treated. While some treated participants, particularly those with pre-existing experience in the stock markets, said that they learned nothing, by far the modal responses were that individuals felt more familiar and confident in interacting with the stock market, and that they became more cognizant of market risks and risk-return tradeoffs.

[**Figure 5**]

Beyond these subjective measures, we also administered a battery of standard questions to gauge individuals' financial literacy, following Van Rooij et al. (2011). Financial

[27] We included the top ten items, except the mental health question, which might have been considered intrusive in the cultural context.

literacy has been associated with improvements in financial decisionmaking, planning and thus wealth (Lusardi and Mitchell, 2014). However, the evidence on the effectiveness of financial education programs is mixed (see Hastings et al., 2013 for a review), and experimentally assigned educational programs have tended to have positive but small effects on financial literacy on average. Kaiser and Menkhoff (2016) provide a meta-analysis. Among various interventions, simplification and rules of thumb seem among the most promising (e.g. Drexler, Fischer and Schoar, 2014, Carpena, Cole, Shapiro and Zia, 2015). Could providing an opportunity to learn about the financial markets by doing, have lasting effects even on this hard-to-move dimension?

Table 9 provides the text of each literacy question as well as the treatment effect on whether a specific question was answered correctly, and the overall percent correct. These questions were asked both around the time of the elections (March 12 or April 2, depending on treatment condition), and three months after the experiment (July 15). Unfortunately, we were unable to survey non-compliers in March-April. Thus we present three sets of results. First we compare compliers to the control (Col 1). Next we assess whether the differences are robust to assigning the missing to either their pre-treatment February (Col 2) or their July (Col 3) literacy scores, given that the March-April scores are likely to fall in between these. Regardless of the specification, the effects are quite similar: the treatment significantly raises the proportion of financial literacy questions answered correctly by 3-4 pp, compared to a baseline of 70.2% correct. Differences are still visible even in July, three months after the experiment (Col 4). These overall effects reflect increases across a range of questions, particularly in basic numeracy, compound interest, avoiding money illusion and understanding the riskiness of stocks versus mutual funds.

[**Table 9**]

A separate question is whether treated individuals become more aware of the economic costs of conflict, and the commensurate economic benefits from peace. To assess whether this was the case, immediately after the elections we 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 Israel's economy or security—and two questions on the effects on their *personal* safety and economic situation (Table 10 provides the exact wording). Interestingly, while 58% of individuals provide the same answer to the two sociotropic questions, 33% say an agreement will have a more beneficial (or less harmful) effect on the 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. This pattern shows up for both right and left voters.[28] Thus, the notion that a peace agreement could be beneficial to the economy (or at least less harmful than it might be to security) is not foreign to voters. We now examine whether exposure to financial markets enhances such assesments.

Table 10 (Panel A) shows the OLS treatment effect on the sociotropic and personal indices, as well as ordered probit estimates on responses to each individual question. Individuals in the treatment group—especially the financially inexperienced—predict greater benefits from a peace agreement for Israel, and *Israel's economy* in particular. In contrast, the treated are as likely as the control group to predict that they will personally benefit from a two state solution. That financial market exposure leads individuals to re-evaluate the costs and benefits of a peace deal to the national economy plausibly helps explain the change in policy preferences and vote choices documented in the previous sections.

[**Table 10**]

We can also rule out other informational effects. One possibility is that the financial

[28]Among participants that had voted for the right in 2013, 57% provide the same answer to the two questions and 35% provide a more positive answer on the economy than on security. For the personal questions, 65% provide the same answer to the two questions, 23% provide a more positive assessment on the effect on personal economic situation and 12% provide a more positive assessment on the effect on personal security. The difference between the last two proportions is more pronounced among right voters in 2013 (32% vs. 6%).

treatment distracted individuals, leading to lower exposure to political news or propaganda relative to the control, which could affect political engagement (Gentzkow, Shapiro and Sinkinson, 2011, Falck, Gold and Heblich, 2014, although we do not observe an effect on turnout, see Table A5). Alternatively, the treatment might have changed the *slant* of the news sources they followed, which could affect the choice of party (DellaVigna and Kaplan, 2007, Enikolopov, Petrova and Zhuravskaya, 2011). A month after the elections, we fielded an information survey assessing participants' political knowledge on factual issues, on the political platforms of the leaders of the *Likud* and Zionist Union, and on events that took place during the election.[29] As Panel B in Table 10 shows, we find no evidence that the asset treatment affected individuals' political knowledge. Similarly, we 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.

Four months after the elections, in July 2015, we also asked individuals which news outlets they read regularly. As Panel C shows, while treated individuals do not change their consumption of non-financial news, they significantly increase the number of financial outlets that they follow. In contrast, we find no change in the media *slant* between treatment and control: they are as likely to read left-leaning news sources (*Haaretz*) and right-leaning outlets (Sheldon Adelson's *Israel Hayom*). These findings suggest that treated individuals are making their inferences about the economic costs of conflict from increased engagement with the financial markets and financial news in particular, rather than from broader media influences. The findings are thus consistent with the treatment encouraging individuals to take an interest in economics and follow financial news,

[29] These 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 settlement?*), 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)?*).

leading to learning. Given that it is not easy to nudge people to follow economics or gain financial literacy, our treatment suggests a new and apparently effective method of achieving this.

6.3 Short-term attention versus persistent learning

We now examine the persistence of the treatment effect on policy preferences. Beyond the direct importance of this question, it can also help shed further light on the mechanism involved. Specifically, the effect we find may be due to short-term attention to economics or temporary behavioral responses (Jayaraman, Ray and Vericourt, 2016). In this case, the effect should not persist. Alternatively, there are at least three reasons why there could be a lasting effect. The first is habit formation: having decided to support a particular position, and given that there are 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 (see Mullainathan and Washington (2009)). A third possibility is that, having overcome fixed costs to learning, treated individuals continue to follow the broader economy over time, and this continues to influence their policy preferences. Note that, unlike the first two reasons for persistence, the third implies that the treatment might even have additional effects, beyond its effects 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. We were able to re-sample 943 participants, a sub-sample that is not statistically distinguishable across treatment and control on pre-treatment vote choice, policy preferences and other characteristics (Table A7). Yet, as Table 11 (Cols 1-2) suggests, when asked in April 2016 which political party they would vote for if the elections were held that day, those exposed to the financial asset treatment continue to show a 0.040 (ITT) to 0.047 (TOT) increase in their ordered vote choice in favor of left parties (p-values both 0.047). 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, as well as a higher Peace Index (Table A8). These results are inconsistent with a limited short-term attention effect. Remarkably, the treatment effect appears to be positive even controlling for individuals' vote choice in 2015 (Table 11, Cols 3-4). This adds credence to the continued learning interpretation rather than habit formation or cognitive dissonance.[30]

[**Table 11**]

6.4 Re-evaluating the risks of status quo policies vs a peace settlement

As discussed above, exposure to the stock market appears to lead individuals to reevaluate the economic risks and benefits from a peace agreement. This could reflect changes in the perceived riskiness of concessions for peace (emphasized by the right) or the riskiness of status quo policies (emphasized by the left). We can exploit the data we collected on individuals' pre-treatment risk aversion to distinguish which is most relevant in our setting. If the treatment primarily reduces an individual's perceived risk of pursuing a peace initiative, either by lowering her perception of the probability of bad outcomes or by increasing her evaluation of the returns in 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 (see Appendix for the theoretical intuition). If, on the other hand, the asset 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.

[30] We also find that one year out, those exposed to the asset treatment 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. This is a substantial increase relative to the sample average of 40.1% who follow financial news. One year out, there is again little change (2.26pp [0.0246]) in the probability of following non-financial news outlets (mean= 88.8%).

Table 12 estimates the effect of the asset treatment, interacted with individuals' self-assessed pre-treatment risk aversion, on the main outcomes as well as on predictions about the effects of a peace settlement. Notice that 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.

[Table 12]

6.5 In-group vs. out-group assets, price effects and engagement

One might expect that exposure to the assets of the other party to the conflict—Palestinian assets in our case—has a greater effect than exposure to the assets of one's own group. This was, in fact, our prior. Out-group assets expose individuals to new sets of considerations and shared risks, they are more novel, and generate more of an opportunity for learning. 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" that can affect participation on both the extensive margin, in the takeup of the assets, and the intensive margin, in the levels of engagement and learning. Simultaneously, the relative price performance of the different assets may also influence willingness to participate (Malmendier and Nagel, 2011, Greenwood and Nagel, 2009).

Table 13 separates the overall asset treatment effect into the effect of being assigned

Palestinian versus other assets. We examine both the Vote Choice and the Peace Index. The effects of being assigned to Palestinian stocks appear to be similar in magnitude to non-Palestinian assets (Cols 1-2). Palestinian and non-Palestinian asset exposure have almost identical effects on the Peace Index (Panel B). For the vote choice, exposure to non-Palestinian assets may even have a somewhat stronger effect, though the difference is not significant (Panel A). These broad similarities in the overall effects, however, may mask differences due to the price performance of Palestinian and Israeli assets during the time of our study, differences in the extent to which individuals were engaged, and differences in the inferences they make from their asset exposure. We consider each in turn.

[**Table 13**]

In Cols 3-4, we estimate the effect of the price change (in basis points) of each individual's 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 significantly higher for assets that performed well prior to election day, though improved price performance does not appear to increase willingness to support concessions for peace. Because participating Israeli assets all out-performed the Palestinian assets (see Figure 2), the price changes also correlate with assignment to in-group vs. out-group assets, making it hard to disentangle the two effects. Including both price change and the assets' nationality (Cols 5-6), the Palestinian asset effects become somewhat stronger relative to Cols 1-2, and the effects of the non-Palestinian assets are attenuated. This is particularly the case for the willingness to make peace concessions. However, the point estimate differences between exposure to Palestinian and non-Palestinian assets remain statistically insignificant.

Take-up and engagement also show interesting differences. Those assigned Palestinian stocks are less likely to take up the asset treatment (78.6% relative to 82.7% for the non-Palestinian). Further, even among those that took up assets, those 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 A10, Panels A,B). Though those with 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.

Among compliers, those that are more engaged in the experiment appear to have also learned more (Figure A6). This may further help explain the absence of a stronger overall effect of exposure to Palestinian assets relative to other assets. However, there is some suggestive evidence that those assigned Palestinian assets make different inferences. 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 (Table A10, 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 A11).

Thus, there appear to be two parallel channels at play. Individuals exposed to domestic assets are more likely to take up assets, are more engaged, and learn more about financial markets, making a re-evaluation of the costs of conflict more likely. In addition, domestic assets performed better during the time of our study. Individuals exposed to out-group assets, however, are more likely to make the direct link between their financial asset and the peace process, and those that do are more likely to alter their attitudes towards peace. The overall effects end up being quite similar.

7 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 driven by direct monetary incentives but rather by changes in policy preferences. Furthermore, the change in policy preferences appears to reflect learning: exposure to financial markets raises overall financial literacy and the propensity to consume financial news, and leads to a re-evaluation of the economic gains from a peace settlement.

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. The last feature is less likely to elicit a backlash by either politicians or participants. Our intervention is also unobtrusive and non-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. 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 governments 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. If our results replicate, then beyond the direct aid provided, such policies may lead recipients to internalize and take more account of the gains and risks of conflict and peacemaking to society more generally. In so doing, financial exposure may provide a useful channel for fostering peace.

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Table 1: **Descriptive Statistics and Balance Tests**

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

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

Table 2: **Treatment Effects on Left and Right Vote in 2015**

	Vote for Left Party in 2015				Vote for Right Party in 2015			
	ITT	ITT	ITT reweighted	TOT	ITT	ITT	ITT reweighted	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Asset Treatment	0.061	0.059	0.043	0.073	-0.045	-0.044	-0.051	-0.054
	(0.029)	(0.023)	(0.020)	(0.029)	(0.031)	(0.024)	(0.027)	(0.029)
Voted Right '13		-0.254	-0.201	-0.272		0.492	0.473	0.505
		(0.091)	(0.083)	(0.094)		(0.122)	(0.127)	(0.120)
Voted Left '13		0.596	0.614	0.608		-0.222	-0.249	-0.231
		(0.091)	(0.090)	(0.090)		(0.088)	(0.088)	(0.092)
Bought/Sold Shares in Last 6 Mths [0/1]		0.018	0.015	0.015		0.030	0.024	0.032
		(0.040)	(0.035)	(0.041)		(0.040)	(0.043)	(0.041)
Traditional		-0.138	-0.155	-0.133		0.102	0.128	0.099
		(0.032)	(0.029)	(0.033)		(0.032)	(0.036)	(0.032)
Religious		-0.166	-0.162	-0.165		0.241	0.232	0.240
		(0.032)	(0.031)	(0.032)		(0.049)	(0.049)	(0.049)
Ultra-Orthodox		-0.221	-0.208	-0.222		0.056	0.033	0.057
		(0.039)	(0.037)	(0.040)		(0.086)	(0.088)	(0.086)
Post Secondary		0.068	0.063	0.066		-0.060	-0.046	-0.059
		(0.033)	(0.027)	(0.033)		(0.034)	(0.037)	(0.034)
BA Student		0.088	0.072	0.088		-0.041	-0.025	-0.041
		(0.038)	(0.032)	(0.039)		(0.039)	(0.042)	(0.039)
BA Graduate & Above		0.062	0.038	0.062		-0.044	-0.021	-0.045
		(0.030)	(0.026)	(0.030)		(0.032)	(0.035)	(0.032)
Willing to Take Risks [1-10]		-0.001	0.002	-0.001		0.007	0.008	0.007
		(0.005)	(0.004)	(0.005)		(0.005)	(0.005)	(0.005)
Time preference above median		0.012	0.009	0.010		0.004	0.004	0.005
		(0.022)	(0.018)	(0.022)		(0.021)	(0.024)	(0.021)
Financial Literacy, %Correct		0.000	0.000	0.000		-0.001	-0.001	-0.001
		(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)
Strata FE	NO	YES	YES	YES	NO	YES	YES	YES
Demographic Controls	NO	YES	YES	YES	NO	YES	YES	YES
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 asset 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.

Table 3: **Treatment Effects on Ordered Vote Choice in 2015**

	Ordered Logit		OLS		IV-2SLS
	ITT	ITT re-weighted	ITT	ITT re-weighted	TOT
	(1)	(2)	(3)	(4)	(5)
A. Full sample (N=1311)					
Asset 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
B. Inexperienced (did not buy/sell assets six months before the experiment (N=842))					
Asset 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
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. Cols 5-6 are 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 2 (Col 2). Cols 2,4 re-weight the data to match the parties' share of 2013 Jewish vote.

Table 4: **Difference-in-Difference Effects on Ordered Vote Choice in 2015**

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

Notes : This table provides OLS (ITT) (Cols 1-4) and 2SLS (TOT) (Col 5) estimates of the difference in the difference in ordered vote choice between individuals in the asset treatment group and control group over two waves: 2013 and 2015. Standard errors are clustered at the individual level (in parentheses). *2015* is a dummy for 2015. Col 2 includes the full set of controls from Table 2, 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.

Table 5: **Effects of Election Day Stockholdings on Ordered Vote Choice in 2015**

N=1311	OLS	OLS	OLS	2SLS
	(1)	(2)	(3)	(4)
Asset Treatment	0.052	0.077	0.045	0.059
	(0.019)	(0.022)	(0.019)	(0.020)
Divest After Election		-0.039		
		(0.019)		
Cash Endowment			0.033	
			(0.022)	
Stock Value on Election Day (100s NIS)				-0.006
				(0.007)
Strata FE	YES	YES	YES	YES
Demographic Controls	YES	YES	YES	YES
R-squared	0.549	0.550	0.550	0.549

Notes : This table provides estimates of the effect of determinants of stockholdings on election day on an individual's vote choice ordered from Right (0), Center/Other (0.5) to Left (1). These determinants include whether an agent was divested after the elections (Col 2) and was initially assigned stocks vs cash (Col 3). Col 4 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. Robust standard errors in parentheses.

Table 6: **Treatment Effects on Attitudes**

Sample	Full Sample				Inexperienced		
	Mean [SD]	Treatment Effect	Obs.	R^2 / Pseudo R^2	Treatment Effect	Obs.	R^2 / Pseudo R^2
	(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 effect of the asset treatment 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 2 (Col 2).

Table 7: **Wealth Effects**

	Ordered Vote Choice		Peace Index		Econ. Policy Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Asset Treatment	0.053	0.044	0.104	0.083	-0.017	-0.003
	(0.025)	(0.021)	(0.058)	(0.049)	(0.052)	(0.047)
Below Avg Income	0.001		-0.052		0.175	
	(0.035)		(0.089)		(0.081)	
Asset Treatment x Below Avg Income	-0.004		0.014		-0.028	
	(0.039)		(0.094)		(0.089)	
High Allocation		0.016		0.055		-0.045
		(0.018)		(0.042)		(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 asset treatment, a dummy for whether an individual had household income below the Israeli average, the interaction with the asset 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 2, Col 2.

Table 8: **Well-Being**

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 coefficient of asset treatment from a separate regression with the dependent variable mentioned in the first column. All regressions include strata fixed effects and the full set of controls from Table 2, 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.

Table 9: **Treatment Effects on Financial Literacy**

	Baseline Mean	(1) Mar-Apr (C)	(2) Mar-Apr (I)	(3) Mar-Apr (J)	(4) July
Financial Literacy Test [% Correct Overall]	70.186	4.961	3.083	3.888	2.909
		(1.304)	(1.127)	(1.231)	(1.224)
Individual Questions Correct? [0/1]					
Numeracy: Suppose you had NIS 100 in a savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money in the account for the entire period? (i) > NIS 102; (ii) = NIS 102; (iii) < NIS 102; (iv) DK.	0.871	0.059	0.051	0.048	0.020
		(0.023)	(0.021)	(0.022)	(0.021)
Compounding: Suppose you had NIS 100 in a savings account and the interest rate is 20% per year and you never withdraw money or interest payments. After 5 years, how much would you have in this account in total? (i) >NIS 200; (ii) = NIS 200; (iii) < NIS 200; (iv) DK	0.693	0.058	0.033	0.057	0.034
		(0.031)	(0.028)	(0.030)	(0.032)
Inflation: Imagine an average household in Israel that has a savings account with an interest rate equal to 1% per year. Suppose the inflation is 2% per year. After 1 year, how much would the household be able to buy with the money in this account? (i) > today; (ii) = today; (iii) < today; (iv) DK	0.703	0.006	-0.004	-0.002	-0.019
		(0.026)	(0.024)	(0.025)	(0.027)
Money Illusion: Suppose that in the year 2020, your income has doubled compared to today and prices of all goods have also doubled. In 2020, how much will you be able to buy with your income? (i) > today; (ii) =; (iii) < today; (iv) DK; .	0.768	0.09	0.074	0.071	0.039
		(0.031)	(0.028)	(0.030)	(0.031)
Stock Meaning: Which of the following statements is correct? If somebody buys the stock of firm X in the stock market: (i) He owns a part of firm X; (ii) He has lent money to firm X; (iii) He is liable for firm X's debts; (iv) None of the above; (v) DK.	0.674	0.058	0.024	0.053	0.042
		(0.031)	(0.027)	(0.029)	(0.030)
Highest Return: Considering a long time period (for example 10 or 20 years), which asset normally gives the highest return? (i) Savings accounts; (ii) Bonds; (iii) Stocks; (iv) DK.	0.411	0.050	0.013	0.020	0.023
		(0.032)	(0.028)	(0.030)	(0.032)
Diversification: When an investor spreads his investments among more assets, does the risk of losing money: (i) go up; (ii) go down; (iii) =; (iv) DK.	0.793	0.028	0.025	0.025	0.022
		(0.026)	(0.024)	(0.025)	(0.026)
Risk: Stock vs Fund: True or False: Buying a single company's stock usually provides a safer return than a stock mutual fund. (i) T, (ii) F, (iii) DK	0.514+				0.071
					(0.033)
Observations		1065	1345	1244	1114
Strata FE		Yes	Yes	Yes	Yes
Demographic Controls		Yes	Yes	Yes	Yes
Initial Financial Literacy Score FE		Yes	Yes	Yes	Yes

Each coefficient represents a separate OLS regression on a measure of financial literacy on the asset treatment, with robust standard errors in parentheses. The outcome in the first row is the percent correct overall on the financial literacy tests, while the rows below provide the proportion getting each question correct. All regressions include strata and the full set of demographic controls, and in addition, we include a set of indicators controlling for each value of the initial financial literacy score. Col 1 (C) provides the post-experiment effect on the literacy score, but only includes compliers and control. Col 2 (I) substitutes pre-treatment values for those missing a literacy score in March-April. Col 3 (J) substitutes the July value for those missing a literacy score in March-April. Col 4 is the financial literacy score in the July survey, with an additional question on the Risk: Stock vs Fund. +: We did not measure this question at baseline, so this the mean for the July survey.

Table 10: **Forms of Learning and Re-Evaluation**

Sample	All				Inexperienced	
	Mean	**SD**	**Treatment Effect**	**SE**	**Treatment Effect**	**SE**
A. Consequences of a Two-State Agreement (OLS/Ordered Probits) [March 2015]						
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... [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 economic situation? (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 own economic situation? (O. Probit)	3.048	[1.047]	-0.013	(0.077)	0.005	(0.101)
your own personal security? (O. Probit)	2.888	[1.237]	-0.002	(0.075)	0.059	(0.094)
Observations			1281 / 1282		823	
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)
Number of non-financial outlets [0-5]	1.393	[1.032]	-0.080	(0.075)	-0.135	(0.097)
Haaretz [0/1]	0.151	[0.358]	0.005	(0.023)	-0.028	(0.029)
Israel Hayom [0/1]	0.431	[0.495]	-0.052	(0.035)	-0.066	(0.045)
Observations			1,120		705	

Notes: The table reports the coefficient of asset treatment from a separate regression with the dependent variable mentioned in the first column. All regressions include the full set of controls and strata FE in Table 2, Col 2. Robust standard errors in parentheses. On March 19, 2015 , we asked individuals to predict the effects of a two state solution at two levels--personal and national--and on two dimensions: security and the economy (Panel A). On April 17, we asked individuals 13 political knowledge questions, of which 2 were questions on salient events in the run-up to elections, 6 were questions on the positions taken prior to the elections by the two leading candidates for the right and left-- Netanyahu and 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, Kalkalist* and *Maariv* . Of these, *Globes, Marker* and *Kalkalist* are financial outlets (Panel C).

Table 11: **Voting Intentions, One Year Post-Intervention**

	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)
Asset Treatment	0.040	0.047	0.025	0.029
	(0.020)	(0.024)	(0.016)	(0.019)
Voted Right '15			-0.266	-0.266
			(0.027)	(0.027)
Voted Left '15			0.202	0.203
			(0.024)	(0.024)
Demographic Controls	YES	YES	YES	YES
Strata FE	YES	YES	YES	YES
Observations	943	943	939	939
R-squared	0.530	0.529	0.657	0.657

Notes : This table presents OLS (ITT) and IV (TOT) estimates. 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 2, 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.

Table 12: **Differential Effects by Risk Aversion**

	(1)	(2)	(3)	(4)	(5)
	Ordered Vote Choice	Peace Index	Econ Pol. Index	Effects of a Peace Settlement	
				Sociotropic Index	Personal Index
Asset Treatment	0.016	-0.079	-0.099	-0.098	-0.129
	(0.032)	(0.075)	(0.073)	(0.093)	(0.095)
Risk Averse	-0.027	-0.176	-0.043	-0.140	-0.126
	(0.037)	(0.086)	(0.083)	(0.104)	(0.108)
Asset Treatment * Risk Averse	0.055	0.291	0.115	0.218	0.205
	(0.041)	(0.095)	(0.089)	(0.116)	(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

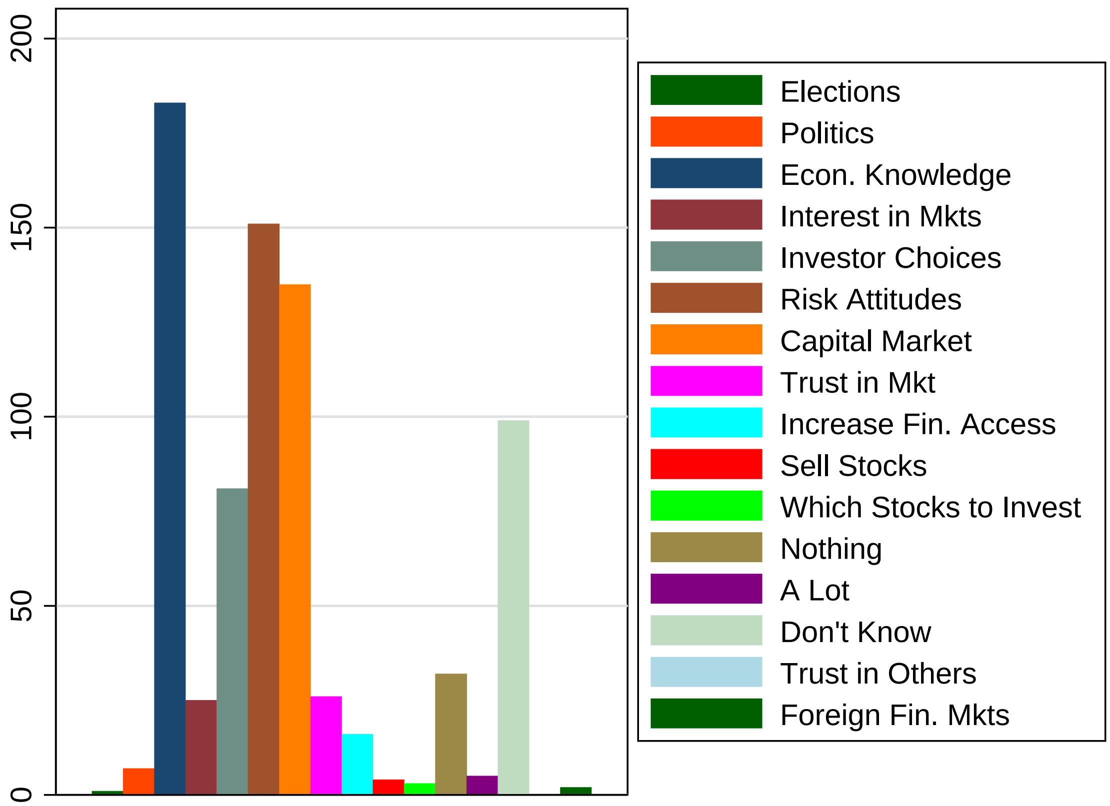
This table shows the differential effects of asset treatment on risk averse individuals, defined as those with ex ante subjective willingness to take risks at the median or below. The outcomes are the 2015 vote choice, ordered Right (0) Center/Other (0.5) Left (2), the Peace Index and the Economic Policy 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, Col 2, except that we replace the willingness to take risk measure with a dummy for being risk averse. Robust standard errors in parentheses.

Table 13: **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	0.042			0.042	0.055
	(0.022)	(0.028)			(0.024)	(0.031)
Non-Palestinian Assets	0.065	0.078			0.038	0.043
	(0.020)	(0.024)			(0.036)	(0.042)
Asset Treatment			0.041	0.051		
			(0.020)	(0.025)		
Price change of asset by elections (basis points)			0.454	0.517	0.507	0.660
			(0.222)	(0.273)	(0.557)	(0.651)
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	0.142			0.120	0.155
	(0.051)	(0.065)			(0.058)	(0.072)
Non-Palestinian Assets	0.110	0.131			0.086	0.098
	(0.047)	(0.057)			(0.086)	(0.099)
Asset Treatment			0.109	0.136		
			(0.046)	(0.058)		
Price change of asset by elections (basis points)			0.044	-0.023	0.442	0.632
			(0.520)	(0.631)	(1.297)	(1.510)
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

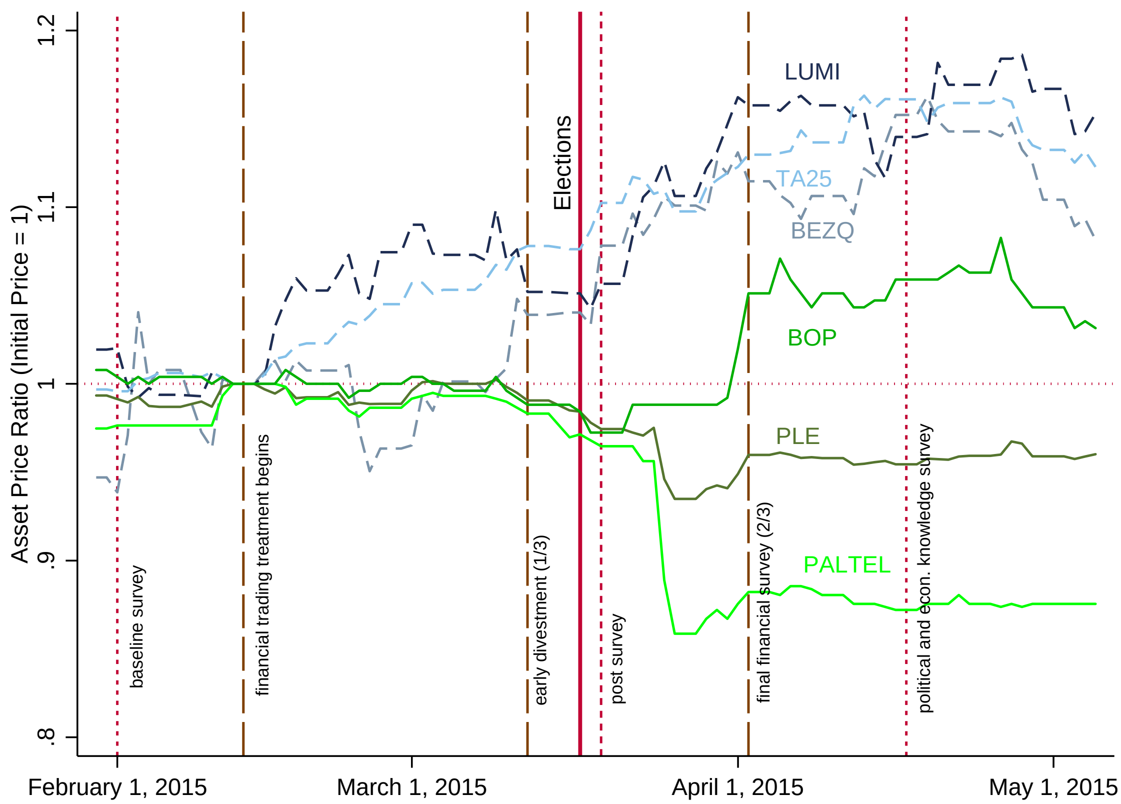
Notes: This table presents OLS (ITT) and 2SLS (TOT) estimates of the treatment effect on an individual's vote choice, ordered Right (0) Center/Other (0.5) Left (1) (Panel A) and the Peace Index (Panel B). The price change is the change in basis points measured from the day of assignment to the trading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

Figure 1: What can the researchers learn from this study?



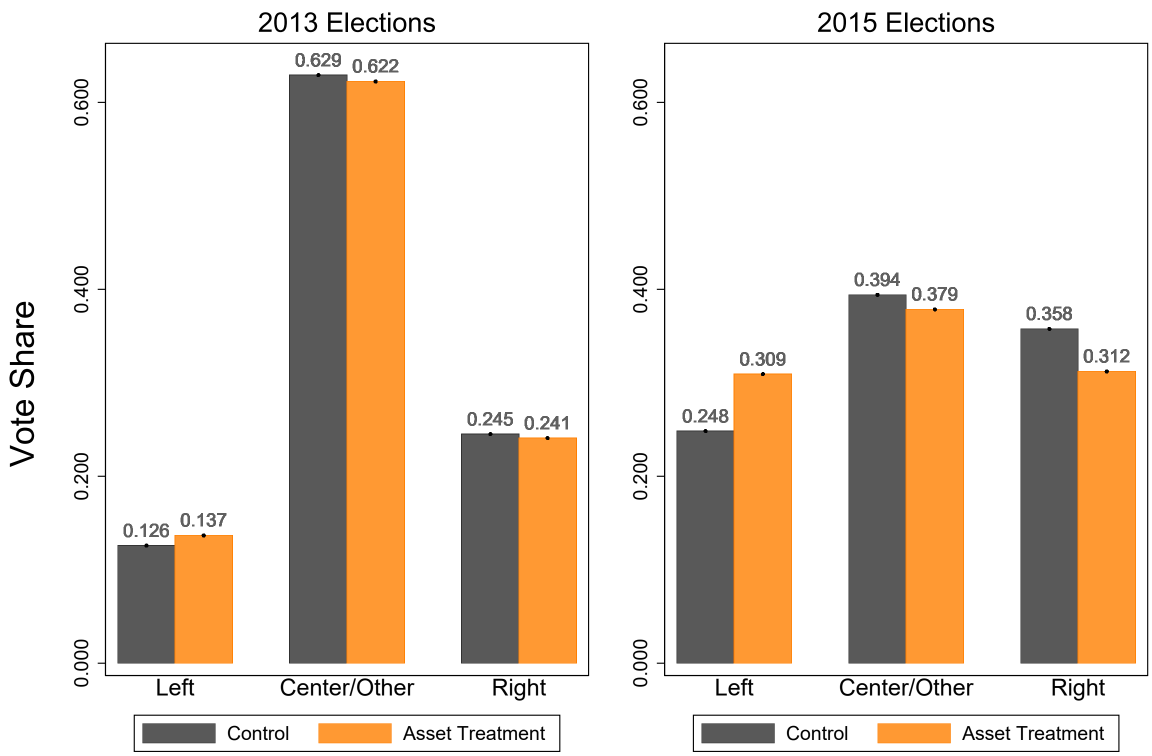
These are the results of an open-response question at the end of the trading period (March 12 or April 2) to the question "What do you think the researchers can learn from the study?". Respondents only include the 840 participants who actually received treatment. Notice that, despite the study being conducted around the time of the elections, only eight mentioned politics or elections in their responses. The modal responses (other than 'don't know') were that the researchers learned about the subjects' economic knowledge, and attitudes towards risk and the capital market.

Figure 2: Asset Prices during the Experiment and 2015 Elections.



Israeli stocks (Bezeq Telecoms (BEZQ), Bank Leumi (LUMI) and the Tel Aviv 25 (TA25)) are dashed and blue, Palestinian stocks (Palestine Telecoms (PALTEL), Bank of Palestine (BOP) and the Palestinian General Market Index (PLE)) are solid and green. Asset prices fluctuated over the course of the experiment, with greater volatility for Israeli stocks. Israeli stocks increased, while Palestinian stocks remained relatively stable until the eve of the elections. The elections, that resulted in gains for the *Likud* party, were followed by further gains for Israeli stocks and losses for Palestinian stocks.

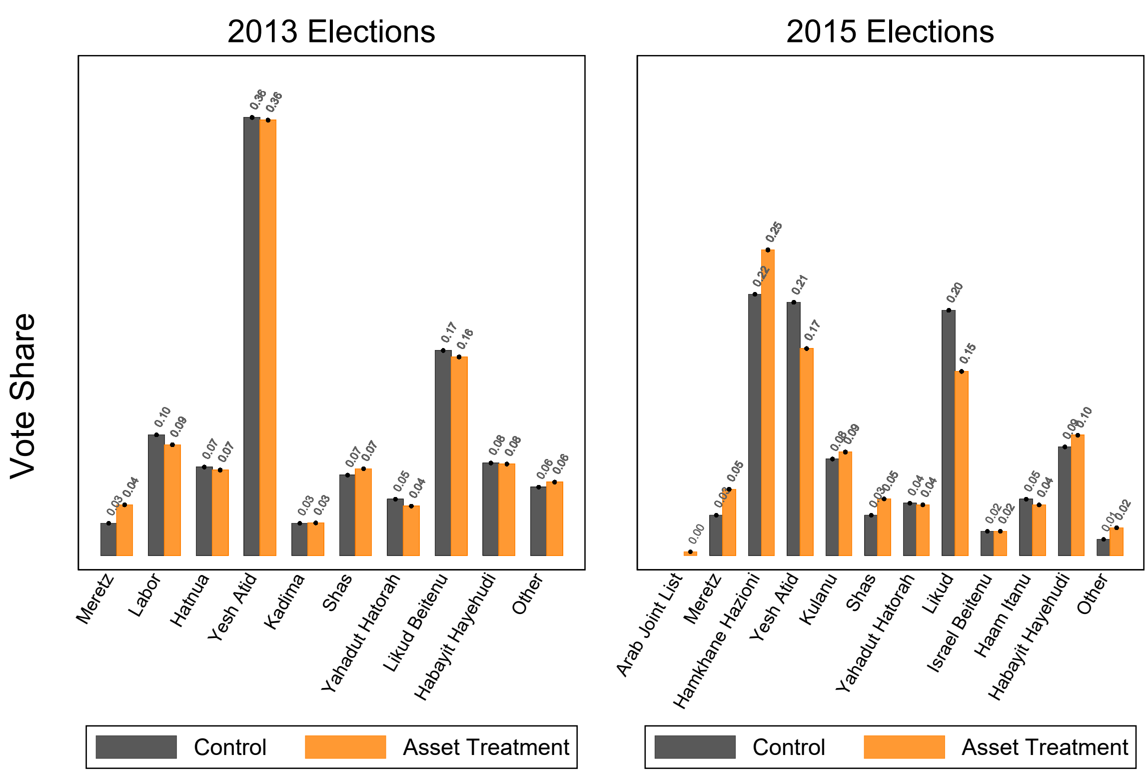
Figure 3: Vote in Treatment and Control Groups in 2013 and 2015



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

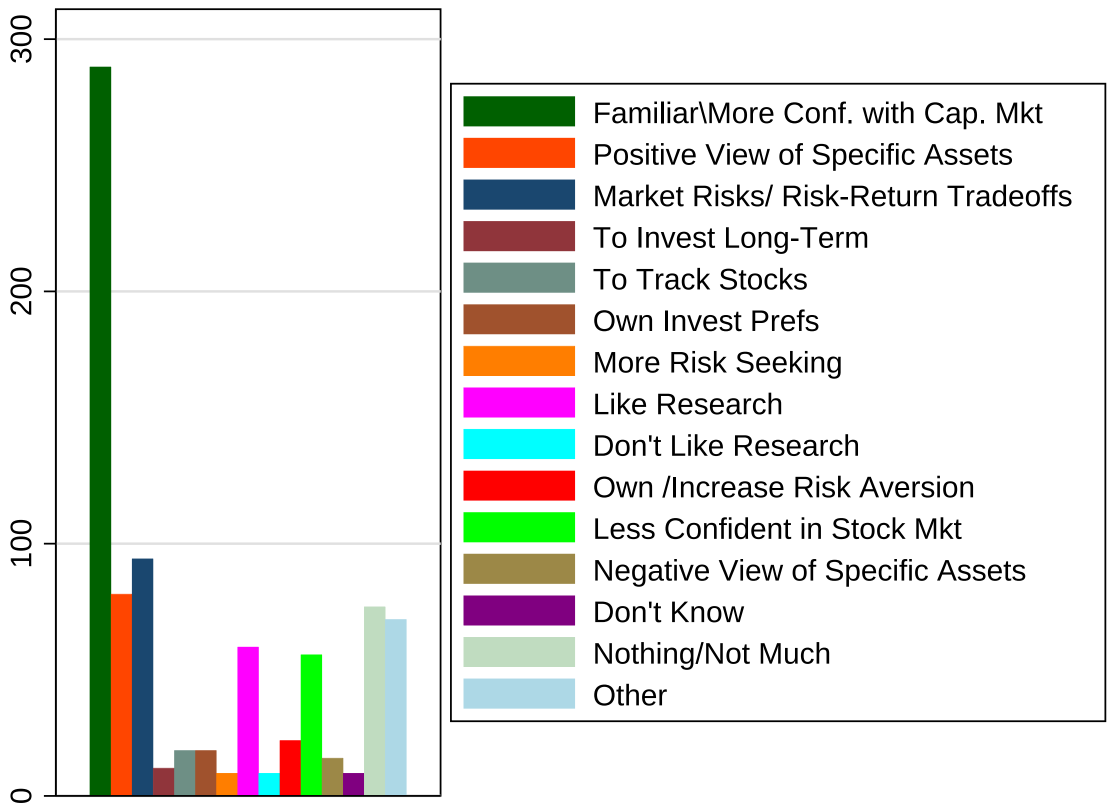
Note: 2013 Left parties include Labor & *Meretz*. Center parties: *Hatnu'a, Kadima, Shas, Yahadut HaTorah* & *Yesh Atid*. Right parties: *Likud Beytenu* and *Habayit Hayehudi*. 2015 Left parties include the Zionist Union, *Meretz* & the Arab Joint List. Center parties: *Yesh Atid, Kulanu, Shas* and *Yahadut HaTorah*; Right parties: *Likud, Habayit Hayehudi, Israel Beytenu & Yachad-Ha'am Itanu*. We over-sampled center voters (based upon their choice in 2013) at twice their vote share. Notice that the treatment and control groups are well-balanced on vote choice in the 2013 elections. However, during the 2015 elections that followed the treatment, there is a shift to the left and away from the right in the asset treatment group relative to the control.

Figure 4: Detailed Vote in Treatment and Control Groups in 2013 and 2015



N=1311. 'Other' includes 1 and 27 individuals who did not vote in 2013 and 2015, respectively.

Figure 5: What did you learn from this study?



These are the results of an open-response question at the end of the trading period (eg March 12 or April 2) to the question "What did you learn from the study?". Respondents only include the compliers. Notice that the modal responses reflect how individuals felt more familiar with and confident engaging with the stock market and financial assets and more aware of the volatility and the risks involved.