

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

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Abstract

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

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1 Introduction

Public attention in societies facing violent conflict 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*—can lead individuals to reevaluate the costs of conflict and to change their political choices to support peace initiatives.

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

This paper presents results from the first study to experimentally assign individuals financial assets, allow them to trade in those assets, and trace the effects on their political views and behavior. 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 intractable.

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

Why then has a peace agreement proven so elusive? Among the many possible reasons, one is particularly relevant for our study. As we describe below, the central debate in Israeli politics lies in weighing the risks and returns of maintaining status quo policies against the risks and returns of making concessions for peace. However, in their day-to-day interactions, individuals may have very different exposure and familiarity with the

risks faced by the economy as a whole. Can exposure to financial markets that provide broader exposure to these risks also change attitudes towards peace initiatives, and even voting decisions?

A month and a half prior to the highly contested 2015 Israeli elections, we randomly assigned 1345 Jewish Israeli voters to either a financial asset treatment or a control group. Individuals in the treatment group received either vouchers that could be used to invest in specific stocks, or endowments of assets that tracked the value of specific indices or company stocks from both Israel and the Palestinian Authority. Participants were given incentives to learn about the performance of their asset and to make weekly decisions to buy or sell part of their portfolio. 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 (\sim \$50) or NIS 400 (\sim \$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 novel approach helps rule out potential social desirability biases or experimenter demand effects that often plague studies on peacemaking. Section 3 details how this was achieved and verified.

Our main result (Section 5) is that exposure to financial markets causes large and systematic shifts in individuals' vote choices in the 2015 elections.¹ Exposure to the stock market reduces the probability of voting for parties skeptical of peace negotiations—known in Israel as the *right*—by about 4 to 5 percentage points (relative to their vote share of 36% in the control). In particular, it reduces support for the incumbent *Likud* party, headed by Benjamin Netanyahu, by 4 to 5 percentage points (relative to 20% in the control). At the same time, it increases the probability of voting for parties that support restarting the peace process—the *left*—by 4 to 6 percentage points (relative to 25% in the control). This mainly reflects a 3 to 5 percentage points increase in the probability of voting for the chief opposition party, *The Zionist Union*, which includes the Labour party.

Consistent with random assignment, these estimates are unaffected by controlling for individuals' vote choices in the recently held 2013 elections, as well as education, income levels, region, religiosity, risk and time preferences, initial financial literacy and other characteristics. In terms of magnitude, these effects are comparable to recently

¹A 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.

estimated effects of changes in security risks—e.g., living in a town within the rocket range of Hamas-dominated Gaza—on Israeli voters (Berrebi and Klor, 2008, Getmansky and Zeitzoff, 2014).

Section 6 exploits the sub-treatments and detailed survey questions to shed light on the underlying mechanisms. The analysis here is more exploratory in nature, as we move away from the one-treatment one-outcome framework to multiple (potentially under-powered) sub-treatments; and multiple outcomes, some of which are attitudes rather than chosen behavior. Nonetheless, we believe the results are illuminating.

We start with two key alternative explanations: that the exposure to financial markets gave participants a direct material incentive to change their vote, or that it induced a change in 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 a direct material 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. In fact those that had realized losses in the stock market prior to the election were more likely to change their voting decision than those still invested but with paper losses, a pattern consistent with an increased sensitivity to risks among those with realized losses (Imas, 2016).

Instead of responding to some direct material incentive, 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. These effects on attitudes are specific to the peace process: if anything, individuals' preferences over *economic* policies shift slightly to the right.

We next use survey responses to better understand why these policy preferences change. We find that treated individuals appear to reevaluate the risks and benefits of the status quo versus restarting the peace process. Specifically, when evaluating the effects of a peace agreement with the Palestinians, individuals exposed to financial markets tend to 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. Relatedly, treated individuals appear (somewhat) more likely to see economic issues—rather than security—as central to the election. They also report being more familiar with the stock market, are more likely to know how the stock market performed recently, and become more likely to follow financial media. In Jha and Shayo (2018) we also report findings that treated individuals

become more financially literate and that women become more confident in their own financial knowledge. In contrast, we find little evidence for either a wealth effect or an effect on individuals' mood or subjective well-being.

A further question is whether the treatment effects are transitory, perhaps reflecting short-term attention to economics, or that even a relatively short intensive exposure to financial markets can lead to lasting re-evaluations of political positions. Strikingly, we find evidence that effects on voting intentions persist even a year after the intervention. Indeed, we cannot rule out the possibility that the treatment changes *more* minds in the long run than in the immediate run-up to the election.

Importantly, we also study the differences between holding in-group (Israeli) vs out-group (Palestinian) assets. On the one hand, the out-group assets could have larger effects as they expose individuals to new sets of considerations and shared risks, and are more likely to demonstrate the connection between financial markets and the peace process. On the other hand, out-group assets are less familiar, and there may also be stigma and psychological costs associated with “trading with the enemy”. Indeed, we find that individuals assigned domestic stocks are more likely to take up assets and are more engaged. Our prior was that the former factors would dominate. Ultimately, however, domestic assets turned out to have greater returns, strengthening their effects, and the overall effects 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 non-paternalistic and arguably empowering. It encourages individuals to learn about stock markets on their own and leaves them to draw their *own* conclusions about the economic costs of different policies. Further, while the treatment is rather intensive, it does not require prohibitively high stakes or long durations: assigning \$50 worth of assets 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, raise the potential for implementing the intervention at scale and in a wide range of settings.

This paper naturally links to a large literature on conflict and underdevelopment. An important body of work shows that places that experienced violence historically tend to be more prone to future violence, often due to changes in culture or a polarization of social attitudes (eg Voigtländer and Voth, 2012, Besley and Reynal-Querol, 2014, Shayo and Zussman, 2011, 2017, Sambanis and Shayo, 2013). A smaller, parallel, literature examines how economic interests may offset these *passions* and mitigate violence (Hirschman, 1977, Martin, Mayer and Thoenig, 2008, Rohner, Thoenig and Zilibotti, 2013, Jha, 2013, 2014,

Grosfeld, Sakalli and Zhuravskaya, 2017, Becker and Pascali, 2016). To the best of our knowledge, however, the causal effects of market interaction on *individual-level* choices and attitudes towards conflict remain to be studied.

More generally, failures of the Political Coase Theorem have been blamed not only for conflict but also for underdevelopment around the world (eg Acemoglu and Robinson, 2000, Rajan, 2006, Fearon, 1996). Yet, by allowing individuals to share in the future gains from the economy and exposing them to novel risks and broader economic considerations, financial assets can also align interests against conflict and in favor of beneficial reforms. In fact, 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, 2017).

Motivated by these historical cases, our contribution lies in examining, experimentally, whether properly designed financial exposure can have meaningful effects in a contemporary environment. Further, unlike this literature, we are able to show that exposure to financial markets can affect policy preferences even without directly creating a significant personal financial stake, by helping individuals to re-evaluate the risks and returns of conflict and peace.

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.² More broadly, the micro-finance and financial inclusion literature in development has made extensive use of random assignment of different financial services, such as savings accounts (Karlan and Morduch, 2010, provides a useful overview). Methodologically, 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 holding this asset has effects on take up by peers. However, no study, to our knowledge, has thus-far randomly assigned opportunities to trade in financial markets. We develop our own simplified trading platform that allows inexperienced individuals to both hold and trade in assets that track real stocks at their actual market prices. Notably, participants do not need to go through the process of purchasing

²The 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.

the assets themselves, as everything is done through our platform. This offers a method of conducting experiments with an important set of factors that have thus far proven very hard to randomize, certainly at scale.

Second, we use double-blinded samples in parallel surveys 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 can potentially be applied to questions quite removed from the political economy of conflict, both to other failures of the Political Coase Theorem, and beyond.

2 Institutional and Political 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 particularly valuable because the 2013 elections provide a recent measure of participants' (pre-treatment) vote choices. We focus on Jewish voters, who comprise around 80% of the population.

It is important to stress that, rather than economic policies, the main dividing line between the right and the left in Israeli politics focuses on the Israeli-Palestinian conflict.³ The Israeli right (led by the *Likud* party) largely favors the status quo, viewing concessions for peace as highly risky and likely to lead to a major deterioration of the security situation. In contrast, the left (led by *The Zionist Union* party) sees status quo policies, including permitting settlements in the West Bank, as already costly and likely

³This also shows up very clearly in the voting patterns in our sample. In an OLS regression of ordered vote choice in 2015 on pre-treatment indices of individual attitudes towards peace concessions and towards economic policies (all 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%).

to put Israel’s security and democracy at further risk. Instead it favors restarting the peace process with the goal of finding a permanent solution to the conflict. Finally, while many Israeli parties can be clearly classified as left or right based on this dimension, other parties—which we will refer to as *center*—tend to focus on different issues and are widely seen as potential members of a coalition led by either the *Likud* or by *The Zionist Union*. These include the religious ultra-orthodox parties *Shas* and *Yahadut HaTorah*, as well as parties focused on civic and economic issues, *Yesh Atid* and *Kulanu*.

A brief description of the position of the three largest parties in our dataset would be useful. The first is the ruling party, the *Likud*, which won 23% of the vote in 2015. The *Likud* did not publish a formal platform in the 2015 elections, but being in power since 2009 it is strongly associated with the status quo and skepticism towards the peace process. On the eve of the elections, on March 16, 2015, the leader of the *Likud*, Prime Minister Benjamin Netanyahu, argued that “Whoever moves to establish a Palestinian state or intends to withdraw from territory is simply yielding territory for radical Islamic terrorist attacks against Israel”, and stated that he would not allow a Palestinian state if elected (Reuters, 2015).

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

The third party is *Yesh Atid*, headed by ex-journalist Yair Lapid. In 2015 *Yesh Atid* focused primarily on economic and civic issues that appeal to the secular middle class, with much less emphasis on the conflict and no clear position on that issue.⁴ As described in Section 3, at baseline we over-sampled Jewish individuals who had voted for non-orthodox center parties in 2013, as these are considered Israel’s swing voters. Since *Yesh Atid* is the main center party in Israel, its unweighted 2015 vote share in our sample

⁴Whereas the opening chapter of the *Zionist Union*’s 2015 platform detailed specific national security and peace initiatives, *Yesh Atid*’s 2015 platform opened with a chapter on corruption, followed by chapters on the cost of living, housing, education, health and welfare. National security and diplomacy were brought up in the eighth chapter (just before small businesses). The chapter listed the many threats Israel faced but did not commit to a clear policy, concluding that “Israel needs to develop a comprehensive national security conception, based on the development and reinforcement of military, political and economic power resources – and a proactive and active foreign policy – and act accordingly” (Yesh Atid, 2015, p. 110).

is 18% even though it won 9% of the general vote in 2015.⁵

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 (the invitation and survey instruments are available on our websites, *linked here*, and in the Supplemental Appendix). They were informed that they would be entered into a lottery to win either a financial asset or an initial voucher of cash to invest in a financial asset, and that these assets would track the value of specific stocks from the entire region.⁶ 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

⁵As we shall discuss, undoing these weights does not change the overall pattern of results. Regarding the remaining parties in our data: *Meretz* and the *Arab Joint List* are both clearly to the left of the *Zionist Union* in terms of support for peace negotiations and willingness to withdraw from territories occupied in the 1967 war. At the other extreme, *Haam Itanu* and *Habayit Hayehudi* are highly supportive of Jewish settlements in the occupied territories and oppose any withdrawals, and as such are to the right of the *Likud*. Finally, *Israel Beitenu*, headed by Avigdor Lieberman, is potentially the hardest to place as, from time to time, Lieberman positions himself more in the center on some issues. Nonetheless he has always been either part of a right wing coalition or opposing it from the right. Classifying *Israel Beitenu* as center does not affect our results.

⁶To 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.⁷

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.⁸ 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 (see also our companion paper, Jha and Shayo (2018)).

As our main interest was in political behavior, however, we limited survey invitations to those that had voted in the past. As mentioned above, we further over-sampled non-orthodox center voters at twice their vote share.⁹ 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, risk aversion and time preference (see survey instruments). A couple of days later they were invited to answer an initial social survey which included questions on political behavior, social and political attitudes, and well-being. Of the 1681 who completed the initial financial survey, 1418 completed 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 somewhat more educated and secular, with fewer individuals over 55 and in the top-most income deciles (Appendix Table A1).

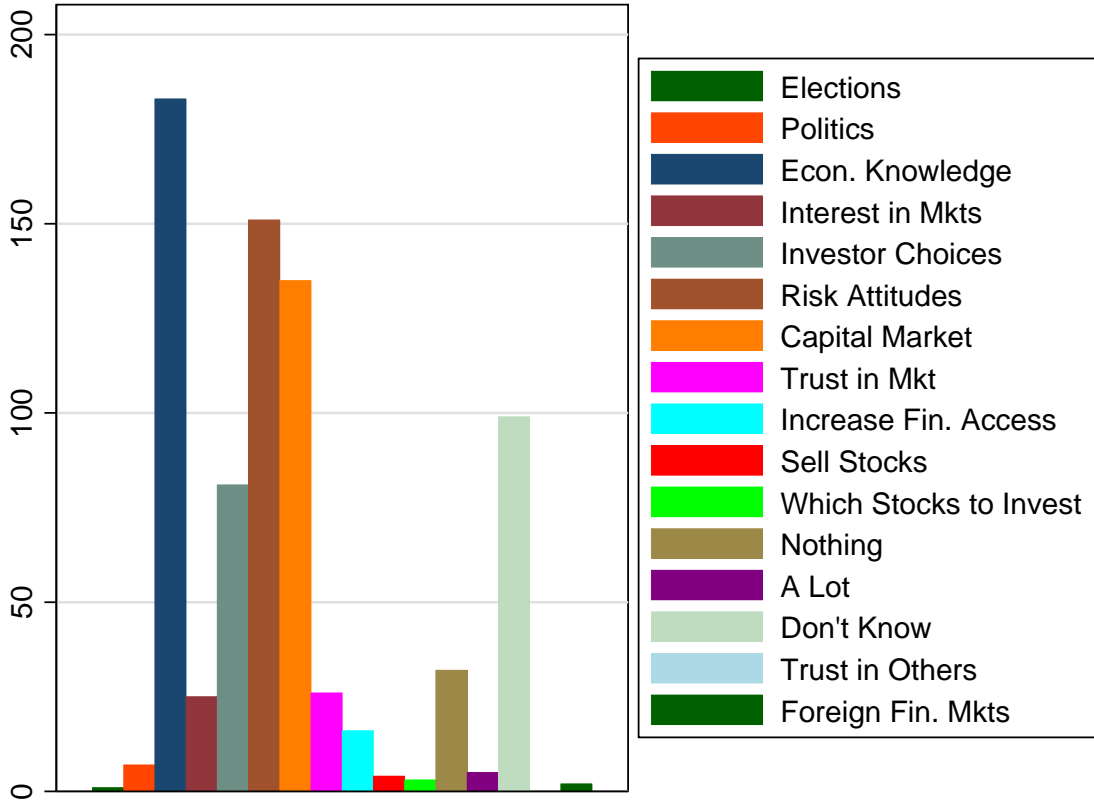
Among these 1345 respondents, we employed a stratified block randomization proce-

⁷The only defense company in the Tel Aviv 25 (TA-25), Elbit Systems, had a weight of only 3.26%.

⁸We are grateful to Kate Casey and Ori Heffetz for suggesting this test during the design phase of our experiment.

⁹That is, individuals who voted for the secular parties: *Yesh Atid*, *Hatnuah* or *Kadimah* in 2013. See also Figure 3.

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



Responses to an open-response question at the end of the trading period (March 12 or April 2) asking: “What do you think the researchers can learn from the study?” Respondents only include the 840 participants who actually received treatment.

ture designed to increase balance across treatment groups in political and demographic variables.¹⁰ A sample of 309 were assigned to the control group, and 1036 were assigned to the treatment group. Further, to help understand the mechanisms involved, participants within the treatment group were initially endowed with either vouchers to invest in stocks, 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. Table 1 summarizes the basic design and initial allocation.

Every week, participants in the treatment group could reallocate up to 10% of their

¹⁰Specifically, 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.

Table 1: Experimental Design

	<u>Total</u>	<u>Redeem pre-elections</u>			<u>Redeem post-elections</u>		
		All	NIS 200	NIS 400	All	NIS 200	NIS 400
Treatment	1036						
Voucher 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						

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 (or selling all their stocks) immediately.¹¹ To further incentivize engagement with the stock market, 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 The 206 individuals who were initially assigned vouchers could buy (and later sell) a specific index fund. Of these, 202 could trade an asset that tracked the Tel-Aviv 25 Index. In addition, four traded for indices from Cyprus, Egypt, Jordan and Turkey.¹³ Both the voucher and the stock

¹¹These limits on divestment and incentives to learn and engage were informed by the lessons of voucher privatization in Russia, where allocations of shares in valuable companies were rapidly sold by novice investors at kopecks on the ruble. See Boycko, Shleifer and Vishny (1997).

¹²The 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.

¹³We included these four indices to be consistent with the information provided to participants, that the stocks participating in the study are from the entire region (see footnote 6). Initially, we considered assigning more individuals to such *neutral* stocks, including potentially the S&P 500. However, as our main motivation was to study the effects of holding financial assets that allowed individuals to learn

groups traded on the same platform and received the same information, but naturally, the main trading possibility faced by the voucher group was to buy while that of the stock groups was to sell their asset.

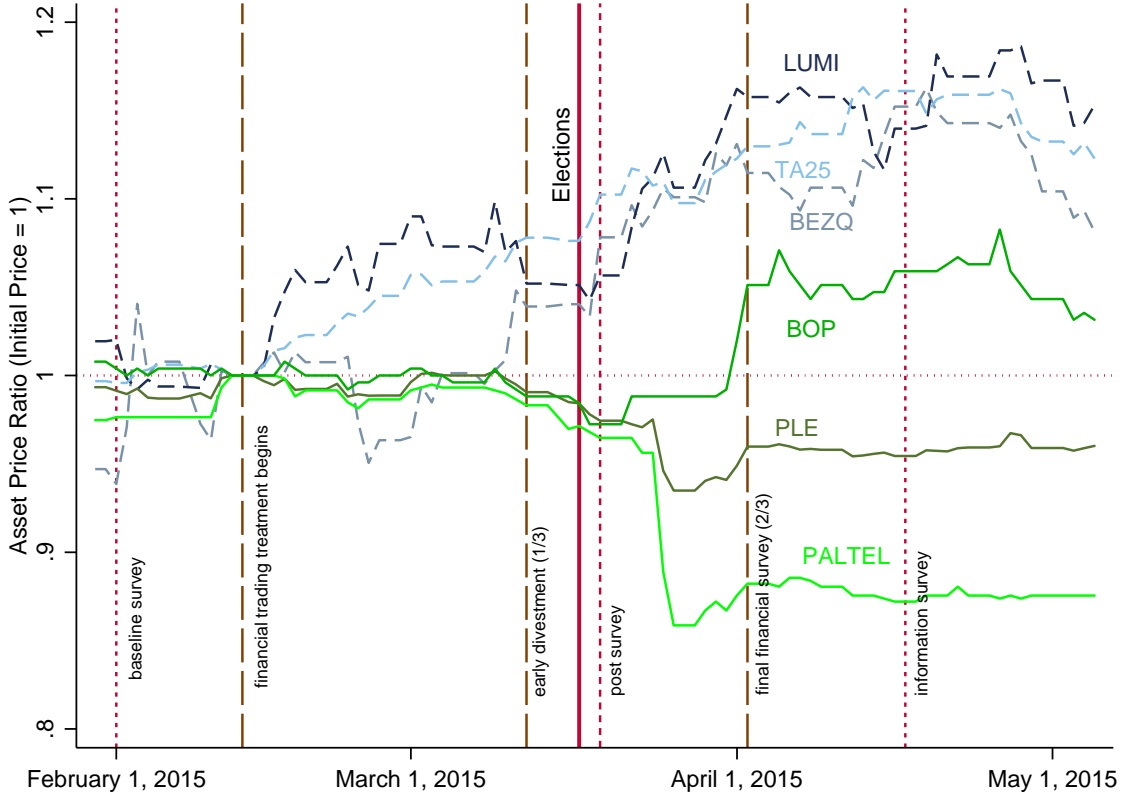
About a third of the treatment group were fully divested of their assets the weekend prior to the March 17 elections. The others could continue to trade in their assets until two weeks after the elections. Finally, about half of the participants in the treatment group were given assets initially valued 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 typical stakes in experimental economics, as well as relative to the standard pay of NIS 0.1 per question these participants receive for our and other surveys.

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 voucher endowments respectively). For the NIS 400 assets, vouchers had the highest takeup (91.3%), followed by Israeli (86.1%) and Palestinian stocks (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

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 the power of the main treatments, we ultimately chose to limit this exposure to four individuals.

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.

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 each of the Israeli assets was ultimately higher than those from the Palestinian economy.

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 re-

¹⁴Specifically, 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.

sponses 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 the treatment group and 97.7% of the control, Table A3).¹⁶

4 Data

Table 2 compares the treatment and control groups across a broad range of pre-treatment characteristics. We restrict attention to the 1311 individuals for whom we have the 2015 vote outcome. 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. Figure 3A shows balance party-by-party.

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 test scores. Two variables show small but statistically significant differences. Individuals in the treatment group are somewhat younger on average (39.3 vs 41.5 years old) and 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

¹⁵Of 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 the treatment indicator from a regression of the probability of reporting a matching vote in the two surveys is -0.008 (SE=0.0144), suggesting that discrepancies in reporting are also unrelated to treatment.

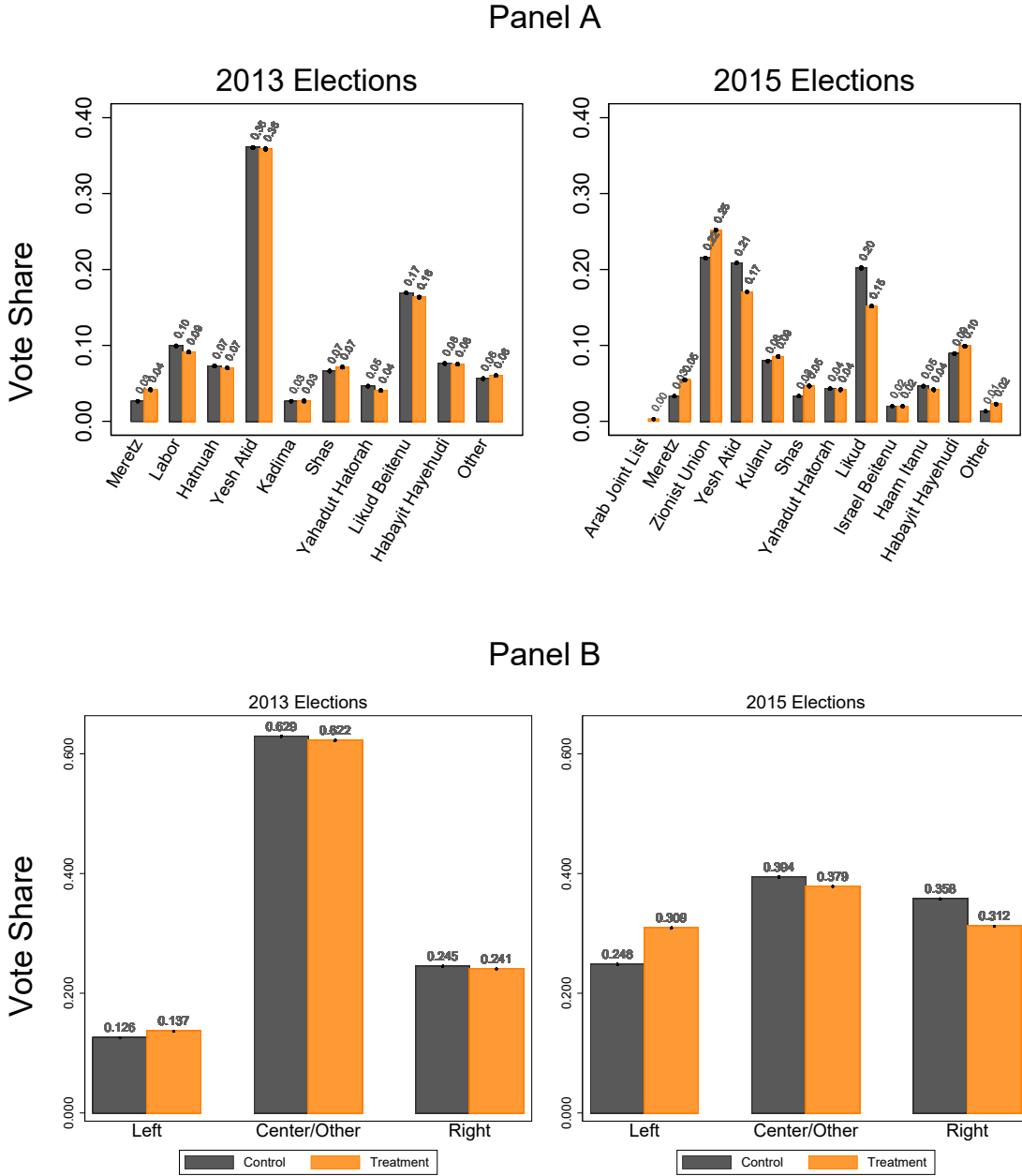
¹⁶We have slightly higher attrition on the questions measuring attitudes towards the peace process, with a response rate of 95% (1277/1345).

Table 2: Descriptive Statistics and Balance Tests

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

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

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



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

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

other demographic variables in our regressions (including a quadratic for age).¹⁷

5 Main Results

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

Voting decisions in the 2015 elections that followed the treatment, however, reveal substantial differences (Panel A, right). The right-wing ruling Likud party won 20.2% of the votes in the control group but only 15.2% in the treatment group. In contrast, the main left-wing party, The Zionist Union, won 21.5% of the votes in the control but 25.2% of the votes among the treated. In the center, the Yesh Atid party won 21% of the votes in the control but 17% in the treatment group.

To help clarify, we show the same result consolidating parties into left and right blocks (Panel B). Within the control group, 24.8% voted for the left (a proportion similar to the 25.3% overall vote share for Jewish left parties in the 2015 elections), but this share increases to 30.9% in the treatment group. At the same time, right parties won 35.8% of the votes in the control group, but only 31.2% in the treatment group.

Table 3 presents estimates of the treatment effect on the probability of voting for left (Cols 1-4) and right parties (Cols 5-8) in the 2015 elections. Table 4 shows the effect separately for the probability of voting for each party, as well as of not voting. For the most part we report Intent to Treat (ITT) estimates, not only because they are

¹⁷These slight age differences actually work *against* the main effect, as, unlike in the US, younger voters in Israel are *less* likely to vote for the left (see also Table A8). Similarly, as we show below, the effects are *stronger* for the *risk-averse*. To further check whether the number of significant differences might indicate a potential problem with the realization of our randomization procedure, we do the following. We randomly assign the sample of 1311 individuals in Table 2 to fictitious treatment and control groups, with the same proportions as those of the actual groups. We then perform the tests reported in columns 3-4 and count the number of significant differences. We repeat this procedure 500 times. Appendix Figure A5 shows the 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.

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

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

Notes: OLS (ITT) and 2SLS (TOT) estimates of the treatment effect on the probability that an individual voted for a left or right party in 2015. Robust standard errors in parentheses. 2SLS estimates use assignment to treatment as instrument. Data in Cols 3,7 are reweighted to represent the vote share of Jewish parties in 2013. Cols 2-4, 6-8 include fixed effects for 104 blocks constructed to stratify sequentially on: 2013 vote, sex, traded stocks, would recommend Arab stocks, geographical region, discrepancies in 2013 vote across surveys, and subjective willingness to take risks. 'Demographic controls' include sex, age, age squared, four education categories, marital status, six regional dummies, four religiosity categories, five income categories (and a dummy for missing), time preference above the median, financial literacy score and subjective willingness to take risks.

more conservative, but as they are important when one is interested in the treatment effect taking into account that some individuals may choose not to participate. We use heteroskedasticity-robust standard errors: the clustering problem does not arise in our benchmark specifications since we randomize at the individual level (the Moulton factor

is 1).¹⁸

Columns 1 and 5 in Table 3 replicate the raw mean differences in Figure 3, Panel B. In Columns 2 and 6 we control for other factors that may shape vote choices. The controls include (pre-treatment) 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. Note that these controls are meaningful determinants of vote choice, as reflected in the increase in R^2 from 0.003 to 0.447 for the decision to vote left, and from 0.002 to 0.518 for the right. Consistent with random assignment, however, the mean treatment effects are essentially unaffected. They again indicate a 6pp increased probability of voting for the left and a 4.4pp reduction in the probability of voting for the right (*p-values* 0.011 and 0.066, respectively).

The magnitude of these effects may be accentuated by the fact that we over-sampled centrist voters that are more likely to change their voting decisions. To assess the influence of our sampling strategy on the effect size, Columns 3 and 6 re-weigh the sample in favor of non-centrist voters so as to match the actual vote share of Jewish parties in 2013. The point estimate is smaller for the probability of voting left (a 4.3 pp increase), but larger (a 5.1 pp decrease) for voting right. This 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* centrist 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.

Finally, it is useful to also measure the treatment effect on those individuals that not only were assigned to the treatment but actually took up the assets (i.e. completed the instructions survey and agreed to continue). 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.

Table 4 estimates the treatment effect party by party. For ease of interpretation, we present a separate regression for each party (Table A5 reports multinomial logit estimates of the vote choice, showing similar patterns). Consistent with the raw data in Figure 3, the treatment significantly increases the likelihood of voting for the main left-wing party,

¹⁸See also Abadie, Athey, Imbens and Wooldridge (2017). Bootstrapping the standard errors for the main specifications in Columns 2 and 6 of Table 3 (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].

The Zionist Union, by 3.7 to 5.3 pp in the ITT and TOT specifications, respectively. It significantly reduces the likelihood of voting both for the main right-wing party, the *Likud* (by 4-5 pp) and the centrist *Yesh Atid* (by 3-4 pp). Again, reweighing the sample strengthens the negative effect on the *Likud* and attenuates the positive effect on the *Zionist Union*. There is no appreciable effect on turnout.

Henceforth, we summarize the voting decision in a single ordered vote choice variable, paralleling the blocks in Figure 3-B. This will be particularly useful for studying mechanisms. We normalize the values to range from 0 for Right, 0.5 for Center or Other, and 1 for Left, for comparability with the binary outcomes. Table 5, 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 effects on the ordered vote choice reveal a 0.052 leftward shift in the unweighted ITT, 0.047 in the reweighted ITT and 0.064 in the TOT (*p-values* equal 0.006, 0.013, 0.004, respectively).

Panel B of Table 5 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. It is important to note that because we stratified on past experience, the strata fixed effects absorb much of the relationship between past financial experience and vote choice in Table 3. When we remove the strata fixed effects (Table A6), 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 relative to the right. Second, the point estimates of the effects of financial market exposure on inexperienced traders tend to be larger, and mimic these patterns. Thus, it appears that the treatment leads those inexperienced in financial markets to become more like experienced traders in their political choices. This indicates that if, over time, the treatment makes individuals more like experienced traders, then the effect on their political attitudes is likely to be stable even in the long run. We examine this empirically below.

As a useful robustness check, we can exploit the fact that we observe voting before the experiment, in 2013, and after, in 2015, to examine within-individual changes in voting behavior over time. However, such a difference-in-difference analysis must be interpreted with caution. Between 2013 and 2015, there have been changes in the composition of

Table 4: Treatment Effects on Party Vote in 2015

Vote in 2015 elections [0/1]	ITT-No Controls (2)			ITT- Full Controls (3)			ITT- Reweighted (4)			IV-TOT (5)			
	Sample Mean	Treatment Effect	SE	R ²	Treatment Effect	SE	R ²	Treatment Effect	SE	R ²	Treatment Effect	SE	R ²
Arab Joint List	0.002	0.003	(0.002)	0.001	0.002	(0.002)	0.148	0.003	(0.002)	0.152	0.002	(0.002)	0.147
Meretz	0.050	0.021	(0.013)	0.002	0.014	(0.009)	0.408	0.012	(0.011)	0.444	0.017	(0.011)	0.408
Zionist Union	0.243	0.037	(0.027)	0.001	0.043	(0.023)	0.353	0.028	(0.020)	0.437	0.053	(0.027)	0.350
Yesh Atid	0.179	-0.038	(0.026)	0.002	-0.032	(0.024)	0.262	-0.018	(0.018)	0.252	-0.039	(0.028)	0.261
Kulanu	0.084	0.006	(0.018)	0.000	0.005	(0.018)	0.125	0.011	(0.016)	0.133	0.006	(0.021)	0.125
Shas	0.043	0.013	(0.012)	0.001	0.008	(0.010)	0.572	0.010	(0.014)	0.581	0.010	(0.012)	0.571
Yahadut HaTorah	0.042	-0.001	(0.013)	0.000	-0.000	(0.008)	0.748	-0.002	(0.010)	0.767	-0.000	(0.009)	0.748
Likud	0.163	-0.050	(0.026)	0.003	-0.043	(0.021)	0.391	-0.055	(0.026)	0.434	-0.054	(0.025)	0.387
Israel Beitemu	0.020	-0.000	(0.009)	0.000	0.000	(0.009)	0.099	0.001	(0.010)	0.123	0.000	(0.011)	0.099
Haam Itanu	0.043	-0.005	(0.014)	0.000	-0.007	(0.013)	0.280	-0.009	(0.017)	0.272	-0.009	(0.015)	0.282
Habayit Hayehudi	0.097	0.010	(0.019)	0.000	0.006	(0.015)	0.380	0.013	(0.019)	0.393	0.008	(0.018)	0.380
Other	0.013	-0.005	(0.008)	0.000	-0.003	(0.008)	0.102	-0.001	(0.009)	0.100	-0.003	(0.009)	0.102
Did Not Vote	0.021	0.010	(0.008)	0.001	0.008	(0.008)	0.102	0.009	(0.009)	0.107	0.009	(0.010)	0.102

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

Table 5: Treatment Effects on Ordered Vote Choice in 2015

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

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

Table 6: Difference-in-Difference Effects on Ordered Vote Choice in 2015

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

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

parties and how they fit into the right-left spectrum.¹⁹ Thus, voting “left” or “right” could mean different things in 2013 and 2015. While in Tables 3-5, 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 6 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.

As a further robustness check, in Table A7, we take out voters of each of the 2013 parties, one party at a time. The treatment effect is not driven by the voters of any one particular party. Looking at heterogeneous effects (Table A8), we find that the effect on the vote is similar across gender and education groups and appears stronger for older participants.

Further, looking across regions (Table A9 and Figure A6) reveals that the point estimates of the effect on the vote are positive throughout the country, with two notable exceptions: the point estimates are 0 in the West Bank (i.e., among Jewish settlers) and in the Jerusalem region, which both includes territories occupied in 1967, and where religiosity is very high.²⁰ Indeed, as we show in Table A8, the treatment mainly affects secular and traditional voters, and has a weaker and statistically insignificant effect on the religious and ultra orthodox. This is not surprising, since the latter overwhelmingly vote for ethnic and religious parties. As we will show, however, though the treatment has limited effect on the vote in these groups, financial asset exposure does appear to have a similar positive effect on attitudes towards the peace process across levels of religiosity.

6 Mechanisms

So far we have demonstrated that exposure to financial assets moves individuals’ votes in the 2015 elections leftward, towards parties that are more supportive of the peace

¹⁹Specifically, one of the main center parties in 2013, *Hatnuah*, created a joint list with the Labour Party, thereby moving to the left. The centrist *Kadimah* party disappeared. On the other side, Moshe Kahlon, a former member of the Likud, created a new centrist party called *Kulanu*. The ultra orthodox *Shas* party split, with offshoot *Haam Itanu* adopting an extreme right position. Lieberman’s *Israel Beitenu*, split from the joint list it had formed with the Likud in 2013.

²⁰The share of religious and ultra orthodox in Jerusalem is 52%, compared to 21% in the broader sample.

process. We believe this result is important in and of itself, and appears to confirm Montesquieu’s conjecture discussed in the introduction. Nonetheless, in this section we exploit the rich set of sub-treatments and attitudinal measures to try to shed more light on the *mechanisms* through which this occurs. The analysis here is exploratory in nature, but we believe it offers several potentially important insights.

Table A2 reports balancing tests across sub-treatments. As before, sub-treatments are balanced relative to the control across almost all dimensions. Even so, we continue to control for pre-treatment characteristics, as in Table 3 (Col 2).

6.1 Economic incentives or changes in policy preferences?

We first evaluate two major alternatives: that the exposure to financial markets gave participants a direct material incentive to change their vote, or that it induced a change in 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.²¹ To test whether this is the case, in Table 7 we employ three complementary strategies that give us exogenous variation in the degree of asset exposure on election day. First, compared to the overall treatment effect (Col 1), we separately examine the effect on individuals who were exogenously divested of their assets the week *prior* to the elections and those who retained the direct material incentive by being divested after (Col 2). Compared to those divested post-election, the effect on those already divested on election day is actually 0.039 *higher*, not lower. We return to this result below.

Second, we compare individuals initially assigned a portfolio purely of stock which they could then sell to those initially given vouchers with which 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 those endowed with vouchers. However, as seen in Col 3, the coefficient on the voucher treatment is statistically insignificant (and, if anything, suggests the effect is *higher*, not lower).

Finally, we examine the effects of the actual asset holdings of each participant on election day (Col 4). As 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 registers a decision every week to simply hold

²¹Within 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 A14).

Table 7: Effects of Election Day Stockholdings on Ordered Vote Choice in 2015

	Full Sample						Risk Averse	
	OLS	OLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.052	0.038	0.045	0.059				
	(0.019)	(0.020)	(0.019)	(0.020)				
Divest Before Election		0.039						
		(0.019)						
Voucher Treatment			0.033		0.020	0.019	0.028	0.022
			(0.022)		(0.024)	(0.023)	(0.031)	(0.030)
Stock value- actual on election day (100s NIS)				-0.006				
				(0.007)				
Divest Before x 1(Price Gain by Mar. 12)					0.067		0.088	
					(0.027)		(0.033)	
Divest Before x 1(Price Loss by Mar. 12)					0.084		0.126	
					(0.029)		(0.039)	
Divest After x 1(Price Gain by Mar. 12)					0.055		0.073	
					(0.023)		(0.030)	
Divest After x 1(Price Loss by Mar. 12)					0.005		0.006	
					(0.024)		(0.032)	
1(Realized Gain before Election)						0.063		0.082
						(0.031)		(0.035)
1(Realized Loss before Election)						0.088		0.135
						(0.034)		(0.043)
1(Paper Gain before Election)						0.068		0.097
						(0.026)		(0.034)
1(Paper Loss before Election)						-0.015		-0.028
						(0.028)		(0.040)
F(excluded instruments)				1100		*		*
Strata FE	YES	YES	YES	YES	YES	YES	YES	YES
Demographic Controls	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.549	0.550	0.550	0.549	0.553	0.553	0.574	0.572
Observations	1,311	1,311	1,311	1,311	1,311	1,311	817	817

Notes: Dependent variable is vote choice, ordered from Right (0), Center/Other (0.5) to Left (1). Col 4 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. Col 5 estimates separate effects according to whether early or late divesters experienced price gains or losses. Col 6 uses the price variables in Col 5 as instruments for whether an agent experienced realized or paper portfolio gains or losses. Cols 7-8 repeat the estimates in Col 5-6 for the sub-sample reporting ex ante median or below willingness to take risks. All regressions include the full set of controls from Table 3, Col 2. Robust standard errors in parentheses. *:Note that the four price gain/loss instruments are nearly perfectly collinear with the portfolio gain/loss so we do not report the F-statistics on each first stage.

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 voucher endowment, and the price change of the underlying asset. As seen from the F-statistic, the instrument works well. However, there is no evidence

for an additional effect of actual stock holdings beyond the average treatment effect.

We conclude that the voting results cannot be attributed to a *direct* material incentive generated by the stocks we provided. There are two points worth stressing, however. First, even if our specific treatment did not create an incentive to vote in a certain way, this does not rule out the possibility of incentive effects on political attitudes more generally. It is likely that a larger scale intervention could induce direct incentive effects.

Second, the incentives generated by the stock positions we provide could be either undone by participants trading outside the experiment or they may even be augmented as individuals become more familiar and engaged with stock markets. Anticipating these possibilities, we explicitly stratified on those that had traded stocks within six months prior to the experiment as they would be better positioned to undo the treatment.²² Every week, we also asked participants that took up our financial assets directly whether or not they had traded outside the experiment. In Figure A8 we plot the share of participants (among the compliers) who bought or sold stocks outside the experiment (see also Jha and Shayo (2018)). Notice that, as expected, the experienced are considerably more likely to report trading outside the experiment. Further, for the inexperienced that took up assets, there is indeed an increase in the propensity to both buy and sell outside the experiment as the study proceeds, consistent with an increase in familiarity with the stock market. By the time of the elections 14% of inexperienced compliers reported trading outside at least once. However, this variation can explain little of the treatment effect (see Figure A9).

As discussed above, the effect on those who were divested before the election is not smaller than the effect on those who had experimentally-assigned *skin in the game* on election day. This is inconsistent with direct material incentives explaining the effect. However, it remains an intriguing question why individuals who were divested before the elections actually appear to respond *more* in their voting decisions (Col 2). One possibility is that knowing that they were committing to a shorter duration, made early divesters more likely to take up the treatment to begin with. It may have also made them more engaged in trading and in other parts of the study during the period prior to elections, increasing the treatment intensity. However, early divesters are only 0.011 (se=0.026) more likely to take up the treatment, and do not appear to engage in more trades, have more accurate knowledge of their stock's performance, spend more time on the survey or be otherwise more engaged prior to the elections (Table A15).

Instead, we unpack the results in light of a distinction highlighted by Imas (2016): that differences in risk-related behavior across settings can be reconciled by the differ-

²²We are grateful to Ken Singleton for pointing this out during the design phase of our experiment.

ential effects of realized losses versus paper losses. In particular, Imas shows that individuals experiencing realized losses tend to become more averse to risks, whereas those experiencing paper losses become more risk-seeking. If this is true, and if the treatment operates in part through exposing individuals to broader economic risks, then the effects should be greater for those with realized losses relative to paper losses. Table 7, Col 5 examines whether the treatment effect differs for early and late divesters according to whether the price of their assigned asset rose or fell prior to the early group's divestment. The results appear to confirm Imas's interpretation: while those whose assets did well show similar effects among both early and late divesters, among those whose prices fell, the effect is 0.084 (se=0.029) for those who divested before the elections while it is 0.005 (se=0.024) for those who did not realize these falls in price. Column 6 uses the price change to instrument for realized versus paper portfolio gains and losses, showing a consistent picture: those with realized losses by election change their vote while those with paper losses are less sensitive.

Finally, Columns 7 and 8 repeat this exercise for the subset of individuals who reported (pre-treatment) a willingness to take risks that is at or below the sample median. Consistent with the risk sensitivity interpretation, the difference between those with realized and paper losses is further amplified for the risk-averse. Below we show that the risk-averse respond more to the treatment in their attitudes towards the peace process as well.

Rather than the direct material incentives provided by stockholdings, individuals might also change their vote choices if exposure to financial markets induces them to change their policy preferences over the peace process or in other domains. We therefore asked our participants two sets of questions: on attitudes toward the peace process, and on their views on conservative vs liberal economic policies (see Table 8). The questions on the peace process that we use are drawn from a standard battery included in 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 approve, tend to approve, tend to disapprove or disapprove of the statement in each question.²³ For economic policy attitudes, we include questions from

²³The proportions approving of these principles in our sample closely resemble the numbers in the representative sample of the Jewish population in the most recent survey, conducted in 2013. The overall trends in the population reveal either stable or falling support for these principles between 2003-4 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

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 8 presents the overall effect of exposure to financial assets on the two indices, as well as the effect component-by-component. Each regression includes the full set of controls from Table 3 (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 appear 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). Remarkably, as we show in Appendix Table A8 (cols 2,5), attitudes towards peace appear to change as much for the religious and ultra-orthodox as for secular and traditional voters.

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. In our case, this comes mainly from a change in policy preferences towards increased individual—rather than governmental—responsibility for addressing poverty.

Finally, in the post-election survey, we also assessed whether the exposure to financial markets affected what could be considered inclusive preferences. Specifically, we asked questions that assess individuals' acceptance of cooperating and interacting with Israeli Arabs in political, social and business domains (Table A17). While the point estimates of the average treatment effect are positive on all three domains, the effect is statistically significant only for the political domain (the acceptance of Arab parties in the governing coalition).

To summarize, the effect of financial market exposure on voting decisions appears to reflect a change in policy preferences rather than any direct economic incentives, and the change in policy preferences stems chiefly from attitudes towards the peace process (and potentially very slightly, inclusiveness), rather than economic policies. As Figure A9 suggests, the change in attitude towards the peace process seems to be the chief factor explaining the treatment effect on the vote.

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 Smoocha (2015) for details.

Table 8: **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 treatment effect on attitude indices. The peace questions were asked in the March 19 survey. The economic questions were asked in the July 19 survey [The effect on the economic policy index for compliers vs control, asked March 12 (early divesters)/ April 5 (late divesters) is also negative and insignificant (-.0274 [0.039])]. The bottom panel reports ordered probit estimates of the treatment effect on the specific questions composing the indices. Col 1 provides means and standard deviations [in brackets]. Each summary index is the average of z-scores of its components, with the sign of each measure oriented so that attitudes commonly associated with the left have higher scores. The z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation (Kling et al. 2007). Robust standard errors in parentheses. All regressions include the full set of controls from Table 3 (Col 2).

Table 9: **Wealth Effects**

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

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

6.2 Wealth and affect versus awareness and re-evaluation of risks

Why, then, did policy preferences change? One possibility is that receiving a financial portfolio worth \$50 or \$100 might have some form of wealth effect that could change policy preferences directly. It could also affect well-being or increase stress. It is worth observing, however, that the initial amounts we provide are unlikely to change an individual's overall wealth meaningfully enough to influence voting a month later. Further, as we just saw, economic policy preferences move, if at all, slightly to the *right*, rather than to the left. However, we can test whether the effects of asset exposure are larger for the poor, as one might expect with a direct wealth effect. Table 9 (Cols 1,3,5) estimates the interaction of the treatment with an indicator for below average pre-treatment income on the vote choice, peace index, and economic policy index. As expected, poorer individuals do support more left-leaning economic policies in our sample (Col 5). However, the interaction term shows no significant difference in the treatment effect for this group for any of these outcomes.

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

\$100 is only 0.016 larger (a statistically insignificant difference).

Another possibility is that the provision of financial assets causes meaningful changes in individuals' well-being, mood or affective states of mind, potentially associated with winning a lottery or with having to make financial decisions. In other settings, the positive effect of such chance events has tended to favor incumbent parties, which should, if anything, attenuate our results (e.g. Healy, Malhotra and Mo, 2010). To examine this directly, we asked individuals immediately after the elections not only about their overall life satisfaction but also a battery comprising the top predictors of well-being based on Benjamin et al. (2014, Table 2). As we show in Table A10, however, the treatment did not significantly change *any* individual indicator of subjective well-being or a combined index of all indicators. Taken together, our treatment effects do not appear to be due to a wealth effect nor to a change in mood or affective state.

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 and led to a re-evaluation of risks in a way that could help explain the change in policy preferences. In Jha and Shayo (2018), we present 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.

Consequently, treated individuals may become more aware of the economic risks and returns of the status-quo relative to those of restarting the peace process. To assess this possibility, 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 and security—and two questions on the effects on their *personal* safety and economic situation (Table 10 provides the exact wording).

Before turning to the results, it is important to note that the idea that a peace agreement could benefit the economy—or at least be less harmful to the economy than it might be to security—is not entirely novel to voters. While 58% of individuals provide the exact same answer to the two sociotropic questions, 33% of them 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.²⁴

²⁴Among participants that had voted for the right in 2013, 57% provide the same answer to the two

We now examine whether exposure to financial markets enhances such assessments.

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 question. Individuals in the treatment group—especially among the financially inexperienced—predict greater benefits from a peace agreement for Israel, and *Israel's economy* in particular. In contrast, the treated are as likely as the control group to predict that they will personally benefit from a two state solution. As Figure A9 suggests, the effect on assessments of the potential gains to the Israeli economy from a peace agreement can explain around 16% of the treatment effect on voting behavior.²⁵

6.3 Re-evaluating 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 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.

Table 11 estimates the treatment effect, interacted with individuals' self-assessed pre-treatment risk aversion, on voting, policy preferences and predictions about the effects

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%).

²⁵One possibility is that exposure to the stock market increases individuals' perception of the correlation between their personal financial situation and the performance of the stock market, or of Israel's economy more generally. However we do not find that treated individuals increase their assessment of these correlations. This again suggests that it is individuals' reassessment of the gains and risks to the national economy, more than how this affects their personal economic incentives, that is specifically relevant.

Table 10: Additional Mechanisms

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 based on the principle of two states for two peoples. How do you think this will affect... [1 (worsen a lot), 2 (worsen somewhat), 3 (no change), 4 (improve somewhat), 5(improve a lot)]				
Sociotropic Index (OLS)	0.011	[0.948]	0.041 (0.054)	0.130 (0.068)
Israel's economy? (O. Probit)	3.294	[1.329]	0.126 (0.073)	0.223 (0.094)
Israel's security? (O. Probit)	2.956	[1.392]	-0.010 (0.076)	0.097 (0.097)
Personal Index (OLS)	-0.013	[0.929]	0.003 (0.056)	0.030 (0.070)
your personal economic situation? (O. Probit)	3.048	[1.047]	-0.013 (0.077)	0.005 (0.101)
your personal security? (O. Probit)	2.888	[1.237]	-0.002 (0.075)	0.059 (0.094)
Observations			1281 / 1282	823
B. What is the Most important Issue in Israel today? (OLS)[March 2015]				
Solely or Mainly Socio-Economic [0/1]	0.4074	[0.492]	0.039 (0.030)	0.023 (0.039)
Solely or Mainly Security and the Political Process [0/1]	0.1387	[0.346]	0.007 (0.022)	0.005 (0.027)
			1291	828
C. 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
D. 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)
<i>Haaretz</i> [0/1]	0.151	[0.358]	0.005 (0.023)	-0.028 (0.029)
<i>Israel Hayom</i> [0/1]	0.431	[0.495]	-0.052 (0.035)	-0.066 (0.045)
Observations			1,120	705

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

of a peace settlement. Notice that ex-ante risk averse individuals—in both treatment and control—are not significantly different from their more risk-tolerant counterparts in either their ordered vote choice or in their economic policy preferences (Cols 1 and 3, respectively). However, while risk averse individuals in the control group are significantly *less* supportive of peace concessions, risk averse individuals that were exposed to financial

Table 11: **Differential Effects by Risk Aversion**

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

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

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.

6.4 Salience and Information

Along with financial asset exposure leading individuals to re-evaluate the benefits of different policies to Israel's economy, it may also make economic issues more salient in their political decision-making. In the post-election survey, we asked individuals: "What do you think is the most important issue in Israel today?" Table 10 (Panel B) provides some (imprecisely estimated) evidence on this: treated individuals are on average 3.9pp more likely to respond that the main issue is only or mainly the socio-economic situation rather than only or mainly security and the political [diplomatic] process, or "both are equally important".

We next look more closely at possible channels that may have led to a shift in attention. One possibility is that the financial treatment distracted individuals, leading to lower exposure to political news or propaganda relative to the control, which could affect political engagement (see Gentzkow, Shapiro and Sinkinson, 2011, Falck, Gold and

Heblich, 2014, although as mentioned above, in our case there is no effect on turnout, see Table 4). 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 therefore 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 campaign.²⁶ As Panel C in Table 10 shows, however, we find no evidence that the 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 D reveals, while treated individuals do not significantly change their consumption of non-financial news, they do 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 not being subject to broader media influences. Instead the treatment appears to encourage individuals to take a specific interest in economics and follow financial news. 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. We examine this process of “learning by trading” in more detail in our companion paper (Jha and Shayo, 2018).

6.5 Short versus long-term attention and awareness

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

²⁶These included 13 questions on the positions of the candidates (eg *what is Herzog’s position concerning the establishment of a Palestinian state as part of a political agreement?*), events during the run-up to the elections (eg *what was the main subject of Netanyahu’s Congress speech?*), and simple factual questions (eg *who was Minister of Defense in the previous government (until December 2014)?*). In the same survey we also included questions on perceptions of Netanyahu’s positions on 5 issues in which he has *not* expressed a consistent position. The treatment did not have a systematic effect on these items either.

Table 12: **Voting Intentions, One Year Post-Intervention**

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

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

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 treated individuals continue to follow the broader economy, and this continues to influence their policy preferences. Note that, unlike the first two reasons for persistence, the third implies that the treatment might potentially have additional effects, beyond its immediate effect on vote choices during the 2015 elections.

A year after the experimental intervention, in April 2016, we surveyed the original participants about their current political positions. 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 A11). Yet, as Table 12 (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* 0.047 and 0.032, respectively). This reflects an in-

creased 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 A12).²⁷

Taken together, the results from the 2016 survey are inconsistent with a limited short-term attention effect. Remarkably, Cols 3-4 in Table 12 suggest that the treatment effect might be positive even controlling for individuals' vote choice in 2015. This supports a continued awareness and information-gathering interpretation rather than habit formation or cognitive dissonance alone.²⁸

6.6 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—would have 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 more novel sets of considerations and shared risks. However, out-group assets are less familiar, and there may also be stigma and psychological costs associated with “trading with the enemy” that can affect participation on both the extensive margin, in the takeup of the financial assets (Huberman, 2001), 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 and risk sensitivity (Imas, 2016, Malmendier and Nagel, 2011, Greenwood and Nagel, 2009).

Table 13 separates the overall treatment effect into the effect of being assigned Pales-

²⁷In Appendix Table A13 we report treatment effects on the individual components of the peace index as well as on other outcomes. Most are imprecisely estimated, but it might be worth noting that the point estimates are positive on all components of the peace index, as well as those of the social and business indices, and negative on blaming the Palestinians for the conflict and integrating solely with the West. The differences between treatment and control on the two state solution's effects on the economy are attenuated relative to 2015, but as the increase in means relative to Table 10 suggest, this is not because the treated group fell in their assessments of the benefits of a peace settlement but rather that the perceived benefits among the control rose substantially. Attention in both groups shifted somewhat to security. It is worth noting that the period between the elections in March 2015 and our follow-up survey in April 2016 witnessed an upsurge in violence known as the ‘Stabbing Intifada’, in which 140 people were killed and 12,040 injured. See <https://www.hrw.org/world-report/2016/country-chapters/israel/palestine>. The control mimic the treatment in another dimension as well: when asked a new question on what would be the effects of a continued lack of negotiations, both treatment and control were close to three times more likely to expect the economic and political situation to worsen than improve. Finally, we asked one more novel question in the 2016 follow-up, to gauge whether financial market exposure changes the extent to which a peace settlement is viewed as zero sum. The results are suggestive: as Figure A7 shows, while 29.27% of the control believed that a peace settlement would benefit “only the Palestinians”, this falls to 26.27% in the treatment group.

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

tinian versus other assets. We examine both the Vote Choice and the Peace Index. The effects of being assigned to Palestinian stocks appear rather 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.

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, but improved price performance does not appear to significantly increase willingness to support concessions for peace. It is important to note that the participating Israeli assets all out-performed the Palestinian assets (see Figure 2) and those exposed to Palestinian assets all faced either realized or paper losses by election day. Thus 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. 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 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 A15, Panels A,B). Though individuals assigned 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.

Despite the lower level of engagement (and thus the weaker intensity of treatment), there is also suggestive evidence that those assigned Palestinian assets make different inferences that may offset this. In particular, they are 40pp more likely than those that received Israeli assets to credit peaceful relations with neighbors as the most important driver of their assets' value rather than company management, workers, national eco-

Table 13: **Effects of In-Group vs Out-Group Financial Assets**

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

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

conomic policies and conditions and domestic political factors (Table A15, 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 A16).

Thus, there appear to be two parallel channels at play. Individuals exposed to domestic assets are more likely to take up assets and are more engaged, increasing the intensity of treatment. In addition, domestic assets performed better during the time of our study. Individuals exposed to out-group assets, however, appear more likely to make the direct link between their financial asset and the peace process, and those that do are more likely to 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 primarily driven by direct monetary incentives but rather by changes in policy preferences. Furthermore, the change in policy preferences appears to reflect an increased awareness of the broader risks to the economy of status-quo policies relative to those that stem from initiatives for peace. We note that financial exposure may also affect voting decisions through additional channels that were not captured by the survey measures and subtreatments we included.

The novel design we use to harness financial exposure to increase awareness of shared economic risks can be useful for a range of settings and applies well beyond violent conflict. For example, in follow-up research, Jha, Margalit and Shayo (in progress) we apply a parallel methodology, exposing UK voters to the opportunity to trade financial assets in UK companies that complement the EU economy and EU companies that complement that of the United Kingdom. Undecided voters exposed to these conditions were much more likely than the control to vote to ‘Remain’ in the EU during the 2016 Brexit referendum.

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

One intriguing possibility is that rather than focusing on providing aid to 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. Beyond the direct aid provided,

such policies might potentially lead recipients to internalize and take more account of the gains and risks of conflict and peacemaking to society more generally. In so doing, carefully designed financial exposure may provide a useful channel for fostering peace.

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SUPPLEMENTAL APPENDIX (FOR ONLINE PUBLICATION)
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Differential Effects by Risk Aversion: Theoretical Intuition 2
 How much of the treatment effect can be explained by different mechanisms? 3

List of Tables

A1 Comparison of the Sample and the Israeli Population 5
 A2 Balance by Sub-Treatment 6
 A3 Attrition 7
 A4 Vote Transition Matrices in Treatment and Control, 2013-2015 8
 A5 Treatment effect on Party Vote in 2015: Multinomial Logit 9
 A6 Financial Experience and Vote Choice, 2015 10
 A7 Are Treatment Effects Driven by the Voters of a Specific Party? 11
 A8 Treatment Effects by Religiosity, Gender, Age & Education 12
 A9 Treatment Effects by Region 13
 A10 Subjective Well-Being and Affect 14
 A11 Descriptive Statistics and Balance, 2016 Follow-Up Sample 15
 A12 Long-Term Effects on Intended Vote and Support for Peace Concessions, 2016
 Follow-Up Sample 16
 A13 Long-Term Effects on Other Outcomes, 2016 Follow-Up Sample 17
 A14 Election Polls and Asset Price Performance 18
 A15 Engagement and Perceived Determinants of Asset Value among Compliers 19
 A16 Perceived Determinants of Asset Value and Political Attitudes among Compliers 20
 A17 Social and Business Attitudes towards Israeli Arabs 21
 A18 Additional Questions from the post-Election Survey 22

List of Figures

A1 Asset Prices in Context, 2012-2016. 23
 A2 CONSORT Diagram 24
 A3 Initial Allocation Screen: Example. 25
 A4 Weekly Trading Screen: Example. 26
 A5 Balancing Tests Simulations 27
 A6 Treatment Effects on the Ordered Vote Choice by Region, 2015 Elections 28
 A7 Is a Peace Settlement Zero Sum? Long-Term Differences in 2016 29
 A8 Trading Activity Outside the Experiment 30
 A9 How Much of the Treatment Effect Can Be Explained by Different Mechanisms? 31

Differential Effects by Risk Aversion: Theoretical Intuition

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

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

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

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

As a heuristic exercise, this appendix examines how much of the estimated treatment effect is explained when we control for each of the candidate channels discussed in Section 6 in the paper. We do not claim to engage in a full-fledged mediation exercise, which requires strong orthogonality conditions (see discussion in Imai, Keele, Tingley and Yamamoto, 2011). Nevertheless this exercise can help illuminate patterns in the data.

Figure A9 shows the estimated treatment effect on the ordered vote choice, after controlling for different outcome variables. The change in coefficients suggests a consistent pattern that highlights the relationship between asset exposure, attitudes towards peace and a focus on the gains to the broader economy. In the post-election social survey (top-left panel), individuals' attitudes towards peace stand out as a major factor that is both influenced by the treatment and is correlated with the vote choice: holding individuals' post-treatment peace attitudes constant attenuates the treatment effect by 28.6%. Two other factors also stand out: the fact that, as we have seen, treated individuals are (somewhat) more likely to view socio-economics as the main issue in the election and that they also increase their assessment of the potential gains to the Israeli economy from a peace agreement. Both these factors also correlate with a vote for parties supportive of the peace process, and controlling for them attenuates the treatment effect by 9.6% and 17.3% respectively.

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

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

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

As one step in this direction, the bottom right panel of Figure A9 compares the extent to which controlling for different responses among the compliers augments or attenuates the treatment effect. First observe that controlling for those that traded outside the experiment actually strengthens the treatment effect. This suggests that these outside trades might indeed have played a small role in undoing the treatment. Further, we

find some suggestive evidence for the parallel channels we discussed in subsection 6.6 (on the Israeli and Palestinian sub-treatments). The more engaged and active in the study (higher for the Israeli asset treatment) are more likely to change their voting decision, thus controlling for engagement attenuates the treatment effect. In parallel, however, as we have seen there is a correlation between compliers that emphasized the role of inter-state peace in driving their asset's value and support for peace (higher for the Palestinian treatment). Controlling for individuals' evaluations of the drivers of their asset also attenuates the treatment substantially. This attenuation is consistent with both engagement in financial activity and the making of a link between financial assets and peace potentially acting as parallel intermediating mechanisms.

Table A1: Comparison of the Sample and the Israeli Population

	Sample (N = 1345)	Israeli Population
1. Region: Jewish Population in District (%)		
Jerusalem District	9.4	11.1
Northern District	9.5	9.5
Haifa District	13.7	10.7
Central District	29.2	28.5
Tel Aviv District	19.8	20.2
Southern District	10.6	14.2
West Bank	7.8	5.8
2. % Female in Jewish Pop., 18+	48.3	51.4
3. Age (Jewish Population above age 18 (%))		
Male		
18-24	10.1	14.6
25-34	29.6	20.4
35-44	28.1	18.7
45-54	15.0	14.7
55-64	9.6	15.1
65+	7.6	16.5
Female		
18-24	14.2	13.3
25-34	29.7	19.2
35-44	26.3	17.9
45-54	14.0	14.6
55-64	10.5	15.5
65+	5.4	19.5
4. Religiosity (Jewish Population aged 20 and over (%))		
Not religious/Secular	63.1	43.4
Traditional	16.8	36.6
Religious	11.9	10.6
Ultra-orthodox	8.2	9.1
5. Education (Jewish Population level of schooling (%))		
Less than high school grad (0 to 10 yrs.)	5.8	13.7
High school graduate (11 to 12 yrs.)	13.7	33.3
Post-secondary/BA Student (13 to 15 yrs.)	38.2	24.1
College grad and above (16+ yrs.)	42.3	28.9
6. Net Monthly Income per Household (NIS)		
Mean	10,978	14,622
Median	12,000	13,122

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

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

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

4. Statistical Abstract of Israel 2015, Table 7.6, 2013 Totals. Survey data for (4) includes all observations age 20 or over (8 excluded from total sample)

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

6. Statistical Abstract of Israel 2015, Table 5.27, 2013 Total (mean). Median is midpoint between 5th and 6th deciles. Data are for entire population, not just Jewish. Survey data represents midpoint of SES categories.

Table A2: Balance by Sub-Treatment

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

Notes: Standard deviations in brackets in Col 1. Standard errors in parentheses in Cols 2-11. Each entry in Cols 2-11 is derived from a separate OLS regression where the explanatory variable is an indicator for treatment. +: mid-point of SES income categories.

Table A3: Attrition

	Treatment	Control
Initial assignment	1036	309
Observed vote in March 2015 elections	1009	302
Proportion observed	0.974	0.977
Observed peace deal attitudes, March 2015	985	292
Proportion observed	0.951	0.945
Observed economic attitudes, July 2015	854	257
Proportion observed	0.824	0.832
Observed vote intention, April 2016	731	207
Proportion observed	0.706	0.670

Table A4: Vote Transition Matrices in Treatment and Control, 2013-2015

	Treatment				Control				
	Vote in 2015				Vote in 2015				
	Right	Center	Left	Total	Right	Center	Left	Total	
Right	83.13	13.99	2.88	100	Right	86.49	10.81	2.7	100
Center	17.04	52.87	30.1	100	Center	21.58	56.32	22.11	100
Left	4.35	11.59	84.06	100	Left	7.89	10.53	81.58	100
Total	31.22	37.86	30.92	100	Total	35.76	39.4	24.83	100

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

Table A5: Treatment effect on Party Vote in 2015: Multinomial Logit

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

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

Table A6: Financial Experience and Vote Choice, 2015

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

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

Table A7: Are Treatment Effects Driven by the Voters of a Specific Party?

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

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

Table A8: Treatment Effects by Religiosity, Gender, Age & Education

	(1)	(2)	(3)	(4)	(5)	(6)
	Ordered Vote	Peace Index	Econ Index	Ordered Vote	Peace Index	Econ Index
A: Religiosity	Religious and Ultra-Orthodox			Secular and Traditional		
Treatment Effect	0.028 (0.030)	0.088 (0.095)	-0.038 (0.087)	0.053 (0.022)	0.095 (0.051)	0.005 (0.041)
Sample Mean	0.225	-0.583	-0.050	0.554	0.231	-0.011
Observations	269	259	273	1,042	1,018	1,072
R-squared	0.649	0.419	0.347	0.518	0.394	0.222
B: Sex	Female			Male		
Treatment Effect	0.059 (0.029)	0.109 (0.063)	0.039 (0.053)	0.051 (0.026)	0.125 (0.065)	-0.018 (0.053)
Sample Mean	0.494	-0.051	0.056	0.479	0.173	-0.086
Observations	630	610	650	681	667	695
R-squared	0.540	0.429	0.219	0.581	0.499	0.211
C: Age	Age > Median (=37.5)			Age <=Median(=37.5)		
Treatment Effect	0.072 (0.029)	0.162 (0.069)	0.055 (0.059)	0.021 (0.027)	0.066 (0.064)	-0.041 (0.050)
Sample Mean	0.519	0.212	-0.026	0.456	-0.069	-0.012
Observations	629	616	636	682	661	709
R-squared	0.582	0.465	0.263	0.609	0.538	0.300
D: Educ Attainment	BA student and above			Less than College		
Treatment Effect	0.050 (0.024)	0.081 (0.060)	-0.057 (0.049)	0.045 (0.031)	0.107 (0.071)	0.037 (0.056)
Sample Mean	0.520	0.158	-0.010	0.441	-0.058	0.005
Observations	754	732	774	557	545	571
R-squared	0.643	0.550	0.300	0.520	0.468	0.290

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

Table A9: Treatment Effects by Region

Effects by Region	(1)	(2)	(3)	(4)	(5)	(6)
	Ordered Vote	Peace Index	Econ Index	Ordered Vote	Peace Index	Econ Index
	Haifa			Northern District		
Treatment Effect	0.025 (0.064)	0.021 (0.202)	0.099 (0.142)	0.083 (0.092)	0.373 (0.217)	-0.126 (0.186)
Sample Mean	<i>0.547</i>	<i>0.177</i>	<i>-0.108</i>	<i>0.564</i>	<i>0.126</i>	<i>0.101</i>
Observations	180	173	184	125	122	128
R-squared	0.657	0.572	0.483	0.812	0.658	0.568
	Tel Aviv			Central		
Treatment Effect	0.099 (0.054)	0.150 (0.120)	-0.145 (0.093)	0.062 (0.043)	-0.041 (0.095)	0.077 (0.077)
Sample Mean	<i>0.592</i>	<i>0.176</i>	<i>-0.023</i>	<i>0.488</i>	<i>0.152</i>	<i>-0.060</i>
Observations	260	256	266	383	373	393
R-squared	0.681	0.633	0.432	0.570	0.544	0.319
	Jerusalem			West Bank		
Treatment Effect	-0.003 (0.048)	-0.145 (0.177)	0.033 (0.189)	-0.004 (0.059)	0.277 (0.192)	-0.008 (0.154)
Sample Mean	<i>0.322</i>	<i>-0.216</i>	<i>0.046</i>	<i>0.230</i>	<i>-0.431</i>	<i>-0.114</i>
Observations	121	117	126	102	101	105
R-squared	0.896	0.796	0.600	0.849	0.824	0.637
	Southern District					
Treatment Effect	0.147 (0.089)	-0.061 (0.188)	0.006 (0.150)			
Sample Mean	<i>0.464</i>	<i>0.039</i>	<i>0.120</i>			
Observations	140	135	143			
R-squared	0.686	0.677	0.616			

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

Table A10: Subjective Well-Being and Affect

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

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

Table A11: Descriptive Statistics and Balance, 2016 Follow-Up Sample

	Mean [SD]		Difference in Means				Obs.
	Treatment	Control	Without FEs		With Strata FEs		
			Diff.	P-value	Diff.	P-value	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Voted Right '13	0.220 [0.415]	0.231 [0.422]	-0.010 (0.033)	0.752	0.001 (0.006)	0.836	943
Voted Left '13	0.136 [0.343]	0.135 [0.342]	0.001 (0.027)	0.957	0.004 (0.004)	0.401	943
Peace Deal Index	0.089 [0.829]	0.123 [0.814]	-0.033 (0.065)	0.607	-0.014 (0.055)	0.794	943
Economic Policy Index	0.014 [0.575]	[0.018 [0.601]	0.032 (0.046)	0.486	0.021 (0.044)	0.635	943
Bought/Sold Shares in Last 6 Mths [0/1]	0.384 [0.487]	0.394 [0.490]	-0.011 (0.038)	0.783	-0.008 (0.021)	0.690	943
Male	0.532 [0.499]	0.534 [0.500]	-0.002 (0.039)	0.966	0.005 (0.014)	0.741	943
Age [Yrs]	40.641 [13.785]	42.096 [14.436]	-1.455 (1.094)	0.184	-1.016 (1.045)	0.331	943
Post Secondary Education	0.216 [0.412]	0.245 [0.431]	-0.029 (0.033)	0.378	-0.016 (0.032)	0.630	943
BA Student	0.135 [0.342]	0.115 [0.320]	0.019 (0.026)	0.466	0.014 (0.027)	0.603	943
BA Graduate and Above	0.453 [0.498]	0.476 [0.501]	-0.023 (0.039)	0.559	-0.022 (0.038)	0.555	943
Married	0.599 [0.491]	0.601 [0.491]	-0.002 (0.039)	0.952	0.014 (0.038)	0.725	943
Religiosity: Secular	0.661 [0.474]	0.673 [0.470]	-0.012 (0.037)	0.750	-0.013 (0.030)	0.675	943
Traditional	0.148 [0.356]	0.168 [0.375]	-0.020 (0.028)	0.480	-0.014 (0.028)	0.622	943
Religious	0.113 [0.317]	0.087 [0.282]	0.026 (0.024)	0.278	0.025 (0.020)	0.221	943
Ultra-Orthodox	0.078 [0.268]	0.072 [0.259]	0.005 (0.021)	0.795	0.002 (0.013)	0.905	943
Region: Jerusalem	0.099 [0.299]	0.096 [0.296]	0.003 (0.023)	0.893	-0.003 (0.021)	0.904	943
North	0.095 [0.294]	0.082 [0.275]	0.014 (0.023)	0.553	0.022 (0.020)	0.277	943
Haifa	0.150 [0.357]	0.125 [0.332]	0.025 (0.028)	0.372	0.036 (0.024)	0.140	943
Center	0.294 [0.456]	0.322 [0.468]	-0.028 (0.036)	0.433	-0.034 (0.029)	0.241	943
Tel Aviv	0.196 [0.397]	0.221 [0.416]	-0.025 (0.032)	0.424	-0.043 (0.027)	0.109	943
South	0.094 [0.292]	0.120 [0.326]	-0.026 (0.024)	0.264	-0.019 (0.021)	0.379	943
West Bank	0.072 [0.259]	0.034 [0.181]	0.038 (0.019)	0.045	0.040 (0.018)	0.025	943
Monthly Family Income [NIS]+	11216.066 [5555.706]	11390.244 [5269.586]	-174.177 (434.784)	0.689	-229.985 (424.105)	0.588	927
Willing to Take Risks [1- 10]	4.724 [2.263]	4.380 [2.173]	0.344 (0.176)	0.051	0.396 (0.168)	0.019	943
Time preference median or above	0.678 [0.468]	0.683 [0.467]	-0.005 (0.037)	0.889	-0.009 (0.037)	0.811	943
Financial literacy: % correct	72.264 [23.311]	71.223 [23.684]	1.042 (1.837)	0.571	1.343 (1.729)	0.438	943

Table A12: Long-Term Effects on Intended Vote and Support for Peace Concessions, 2016 Follow-Up Sample

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

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

Table A13: Long-Term Effects on Other Outcomes, 2016 Follow-Up Sample

	N	Mean	SD	Treatment Effect (SE)
<i>Peace Index [OLS]</i>	937	0.038	0.815	0.067 (0.053)
Two states for two peoples [1-Disagree, 5- Agree]	937	2.713	1.099	0.058 (0.093)
1967 borders with a possibility of land exchanges [1-5]	937	2.239	1.093	0.089 (0.093)
Jerusalem will be split into two separate cities - Arab and Jewish [1-5]	937	1.998	1.059	0.016 (0.094)
Palestinian refugees will get compensation & allowed to return to Palestine only [1-5]	937	2.218	1.049	0.194 (0.090)
<i>Social Relations Index [OLS]</i>	934	0.054	0.955	0.096 (0.065)
Arabs will live in Jewish neighborhoods [1-5]	934	2.224	1.057	0.139 (0.093)
Arabs will attend Jewish high schools [1-5]	934	2.314	1.094	0.163 (0.093)
<i>Business Index [OLS]</i>	934	0.045	0.954	0.073 (0.065)
Arabs and Jews will form joint businesses [1-5]	934	2.885	1.003	0.089 (0.091)
Arabs will manage Jewish companies [1-5]	934	2.666	1.075	0.131 (0.093)
<i>Arab parties will be part of the governing coalition [1-5]</i>	934	2.208	1.067	0.159 (0.095)
<i>Palestinians are the main culprits in the long conflict between them and the Jews [1-5]</i>	934	2.988	0.997	-0.033 (0.090)
<i>Israel should integrate with the West and maintain only necessary contacts with the Arab states. [1-5]</i>	934	2.612	0.843	-0.023 (0.087)
<i>What is the Main Issue in Israel Today? [OLS]</i>				
Mainly or Solely Socioeconomic [0/1] [OLS]	936	0.288	0.453	-0.035 (0.036)
Mainly or Solely Security and Political process [0/1][OLS]	936	0.147	0.355	0.054 (0.026)
<i>Consequences of a Two-State Agreement [1-Worsen substantially, 5- Improve a lot]</i>				
Israel's economy	937	3.572	1.208	0.060 (0.089)
Israel's security	937	3.295	1.353	0.089 (0.085)
Your personal economic situation	937	3.114	0.829	0.003 (0.093)
Your personal security	937	3.221	1.208	0.130 (0.085)
<i>Consequences of not holding negotiations for the foreseeable future [1-Improve a lot, 5- Worsen substantially]</i>				
Israel's economic situation	936	3.324	0.907	-0.107 (0.083)
Israel's security	936	3.412	1.065	-0.051 (0.090)
Your own economic situation	936	3.120	0.609	0.042 (0.088)
Your own personal security	936	3.296	0.831	-0.070 (0.096)

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

Table A14: Election Polls and Asset Price Performance

Closing Asset Price Each Day (% of Feb 12 price)	(1)	(2)	(3)	(4)	(5)
% Seats Predicted for the Right	0.476 (0.528)	0.652 (0.407)	0.639 (0.380)		
% Seats Predicted for the Left	0.222 (0.240)	0.286 (0.246)	0.300 (0.173)		
% Seats Right x Israeli Stock	-1.593 (0.605)	-1.593 (0.607)	-1.593 (0.613)		
% Seats Right x Palestinian Stock	-0.377 (0.532)	-0.377 (0.534)	-0.377 (0.539)		
% Seats Left x Israeli Stock	-0.653 (0.472)	-0.653 (0.473)	-0.653 (0.478)		
% Seats Left x Palestinian Stock	-0.298 (0.241)	-0.298 (0.242)	-0.298 (0.245)		
% Seats Predicted for the Likud				0.181 (0.143)	0.246 (0.144)
% Seats Predicted for the Zionist Union				-0.162 (0.186)	-0.184 (0.162)
% Seats Likud x Israeli Stock				-0.560 (0.276)	-0.560 (0.279)
% Seats Likud x Palestinian Stock				-0.311 (0.147)	-0.311 (0.149)
% Seats Zionist Union x Israeli Stock				0.525 (0.383)	0.525 (0.388)
% Seats Zionist Union x Palestinian Stock				-0.077 (0.189)	-0.077 (0.192)
Asset Ticker Fixed Effects	Yes	Yes	Yes	Yes	Yes
Quadratic Time Trends	No	Yes	Yes	No	Yes
Week Fixed Effects	No	No	Yes	No	Yes
Observations	330	330	330	330	330
R-squared	0.569	0.574	0.580	0.493	0.505

This is an OLS regression. The dependent variable is the daily closing price of each of the assets in our study, normalized by their value as of February 12. The main explanatory variables include the % of Seats for Left and Right based on the simple averages of all polls on each day linked in "Opinion Polling for the Israeli Legislative Election 2015" in Wikipedia and supplemented by an aggregation website maintained by Haaretz (www.haaretz.com/st/c/prod/eng/2015/elections/center). The assets include all those participating in the study: Israeli Stocks include LUMI, TA25, BEZQ. Palestinian Stocks include: PLE, PALTEL, BOP. We also include Reference Stocks from the region: AMGNRLX (the Amman Stock Exchange General Index) EGX30 (the Cairo 30 Index), XU030 (the Istanbul Index), CYFT (the Cyprus/FTSE 20). The set of days are all that included at least one poll between January 30 to March 18. All regressions include asset fixed effects. Errors are clustered at the asset level. We sequentially add Quadratic Time Trends and Fixed Effects for each week. Notice that the reference stocks are largely unaffected by the polls. However, Israeli stocks lose value with increases in predicted shares for the right. Looking at the two main parties which were the focus of the election (and for whom an increase in seat share would reduce reliance on coalition partners) in Columns 4 and 5 reveals that an increase in seat share for Likud was associated with a fall in the value of both Israeli and Palestinian stocks in our study.

Table A15: Engagement and Perceived Determinants of Asset Value among Compliers

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

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

Table A16: Perceived Determinants of Asset Value and Political Attitudes among Compliers

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

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

Table A17: Social and Business Attitudes towards Israeli Arabs

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

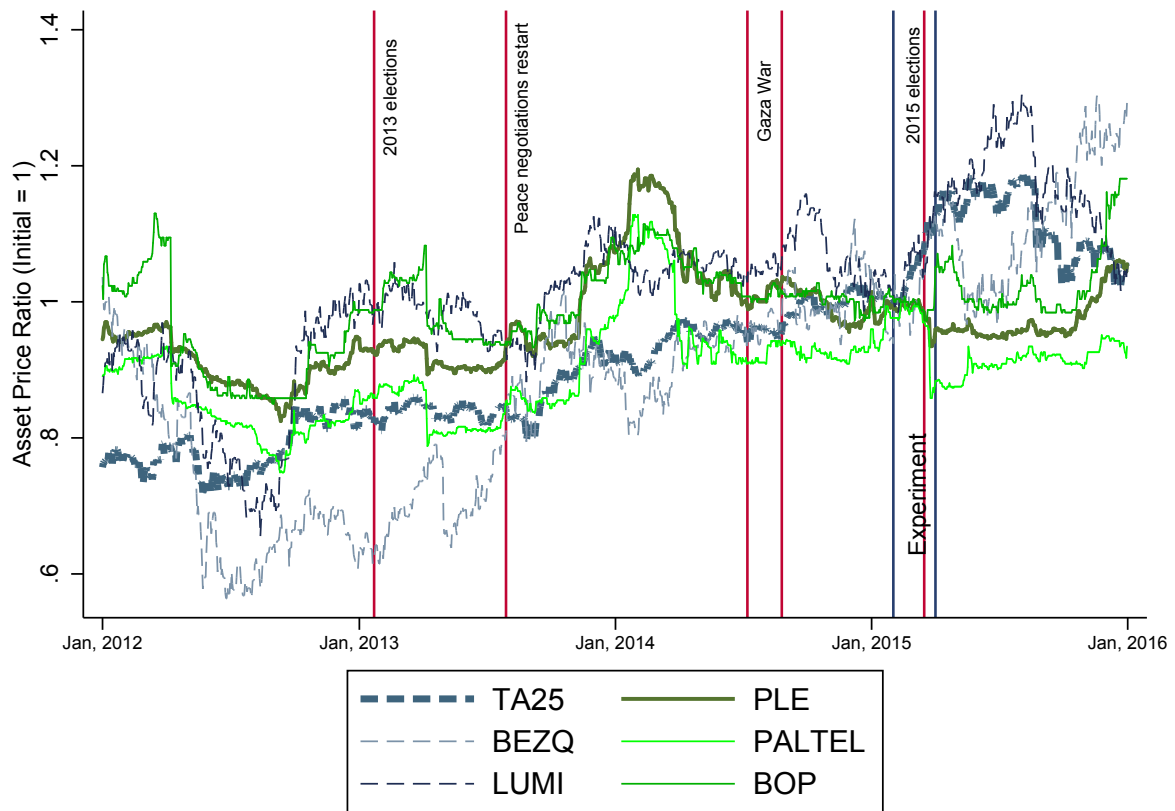
Notes: The table reports the treatment effects on a series of questions on social and business attitudes towards Israeli Arabs. Each row reports either an OLS regression on a Z Score Index, following Kling et al 2007, or an ordered-probit regression on the component dependent variables indicated in the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

Table A18: Additional Questions from the post-Election Survey

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

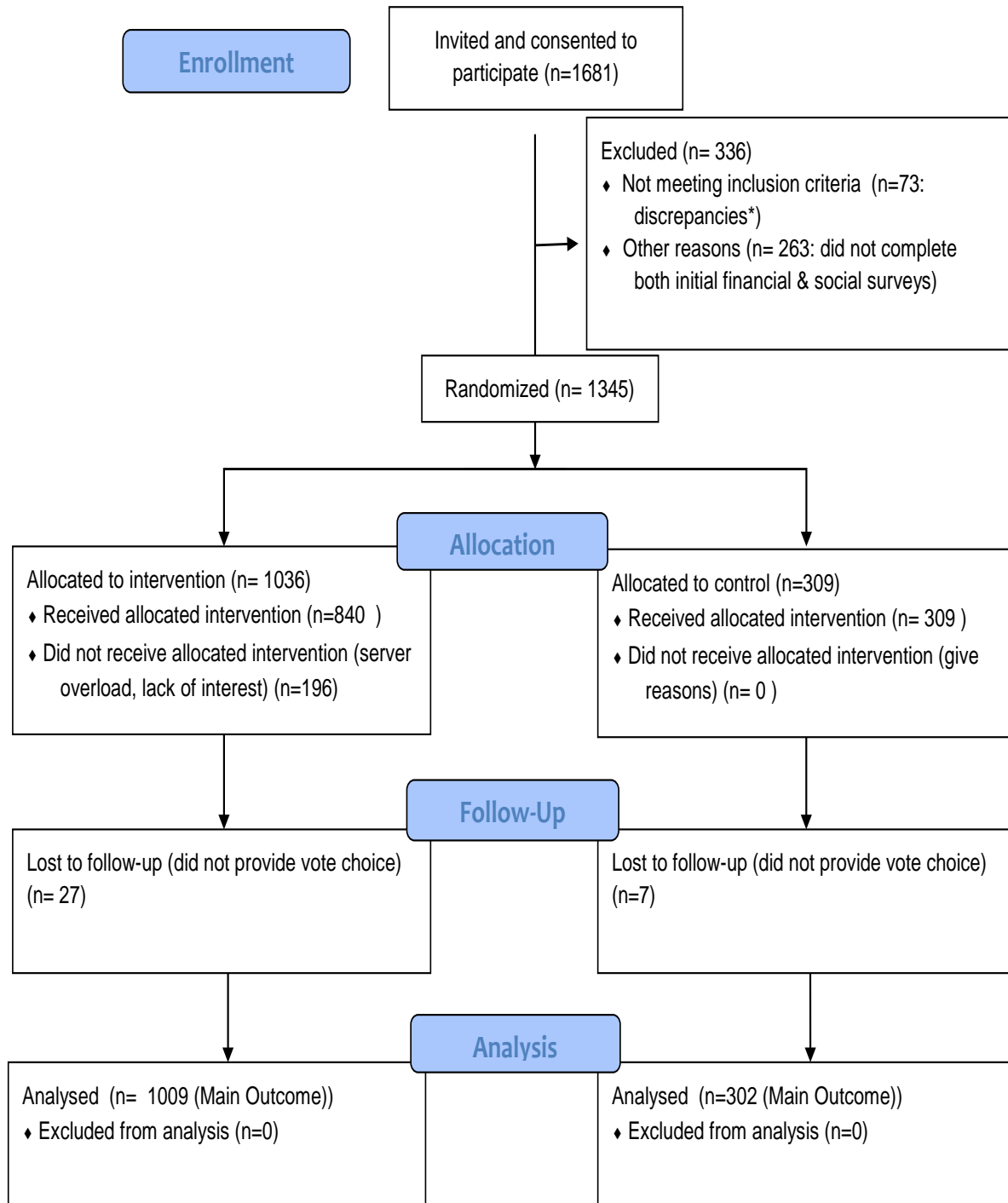
The table reports the treatment effects on all remaining questions from the post-election survey (Final Social Survey, March 19). Each row reports the treatment effect from an ordered-probit regression with the dependent variable indicated in the first column. All regressions control for the full set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

Figure A1: Asset Prices in Context, 2012-2016.



Each week, participants were asked to gauge the performance of their asset in the prior three years (2012-2015). During that time, Israeli and Palestinian asset prices had risen after the onset of peace negotiations, and fallen after their collapse.

Figure A2: CONSORT Diagram



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

Figure A3: Initial Allocation Screen: Example.

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

- המניות כוללות בנקים וחברות תקשורת.
- המדדים עוקבים אחר הערך של כמה מהחברות הציבוריות הגדולות בכל מדינה (בדרך כלל מדד מסוים כולל בין 20 ל-30 חברות).

ישו לי לב במיוחד לנסה שבו זכות ולמספר המניות שברשותי. אותו מספר המניות יעמוד לרשותך גם בשבוע הבא. לפיכך, אם המחיר של הנכס יעלה - ערך הנכסים שלך יעלה בהתאם. אם המחיר של הנכס ירד - ערך הנכסים שלך ירד בהתאם. הרשימה מסודרת בסדר אלפביתי לפי סימול המניה או המדד באנגלית.

שם	שם באנגלית	סימול	מטבע	מחיר הנכס היום (במטבע מקומי)	מספר המניות שברשותי	ערך הנכסים שלי כנצטבע מקומי (מקומי)	ערך הנכסים שלי (בש"ח)
בנק אקבנק, טורקיה	Akbank Turkey	AKBNK	TRY	8.55			
מדד של בורסת רבת עמון בירדן	Amman SE General Index Fund	AMGNRLX	JOD	2,186.18			
בזק (חברת תקשורת ישראלית)	Bezeq	BEZQ	ILS	663.10			
בנק ירדן	Bank Of Jordan	BOJX	JOD	2.80			
בנק פלסטין	Bank Of Palestine	BOP	JOD	2.78			
מדד של 20 המניות הגדולות בקפריסין	Cyprus/FTSE Top 20 Index Fund	CYFT	EURO	44.44			
מדד של 30 המניות הגדולות בבורסת קהיר במצרים	Egypt EGX 30 Index Fund	EGX30	EGP				
מצרים טלקום	Telecom Egypt	ETEL	EGP				
ירדן טלקום	Jordan Telecom	JTEL	JOD				
בנק לאומי לישראל	Bank Leumi	LUMI	ILS	1,288.00			
פלסטין טלקומוניקיישן (חברת תקשורת פלסטינית)	Palestine Telecommunications	PALTEL	JOD	5.94	6.122	36.36	200
מדד של הבורסה הפלסטינית בשנים	Palestine Stock Exchange Index Fund	PLE	JOD	504.76			
מדד תל-אביב 25	Tel Aviv TA-25 Index Fund	TA25	ILS	1,452.46			
טורקסל (חברת תקשורת טורקית)	Turkcell	TCELL	TRY	14.80			
בנק יוניון הלאומי של מצרים	Union National Bank of Egypt	UNBE	EGP	5.90			
מדד של 30 המניות הגדולות בבורסת איסטנבול בטורקיה	Borsa Istanbul 30 Index Fund	XU030	TRY	106,359.21			
כסף מזומן	CASH	CASH	ILS	1.00			

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

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

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

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

Figure A4: Weekly Trading Screen: Example.

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

- Link to website with info on assigned stock:** Points to the URL <http://il.investing.com/equities/bezeq-ord>.
- Composition, price and updated value of portfolio:** Points to the "מצב תיק הנכסים שלך" section.
- Buying decision (if current portfolio includes cash):** Points to the "קניה" section.
- Selling decision (if current portfolio includes stocks):** Points to the "מכירה" section.

Text on the screen:

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

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

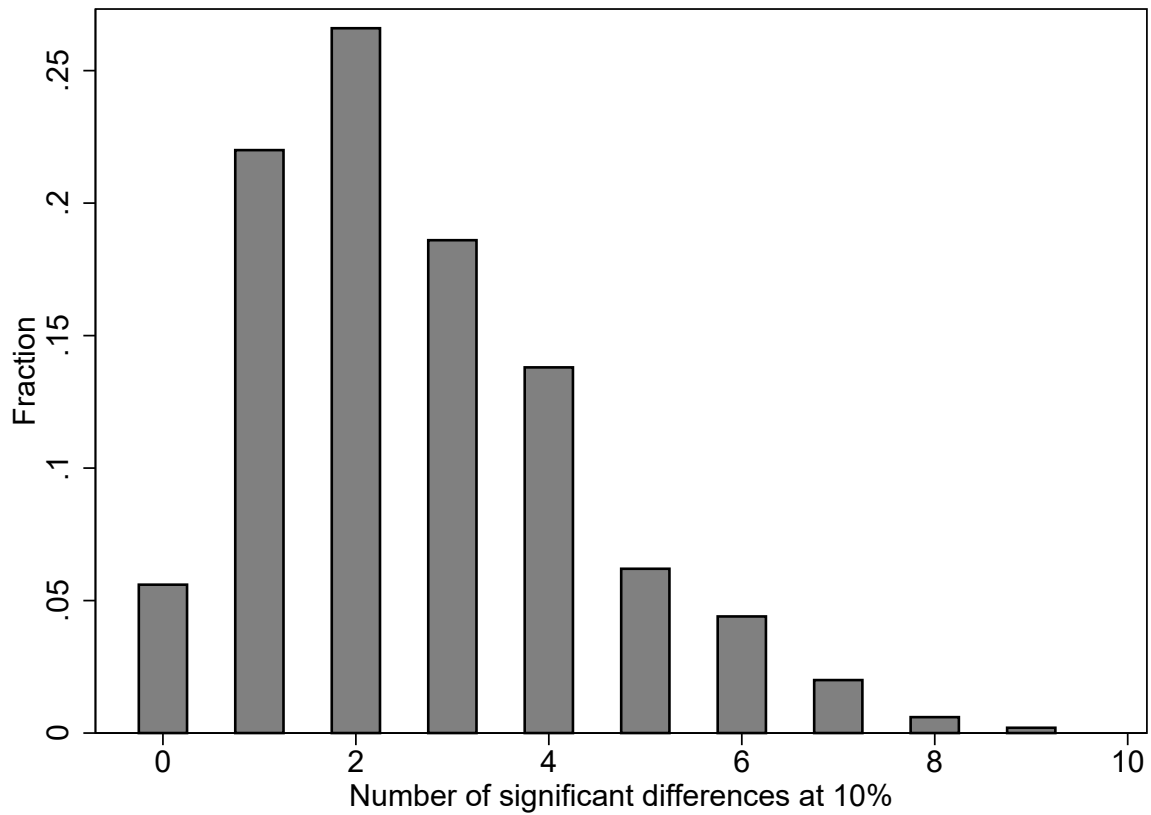
החלטות ההשקעה שלך
 אנא חזן להלן את החלטות הקניה והמכירה שלך. קניה ומכירה של נכסים אינן כרוכות בעמלה.

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

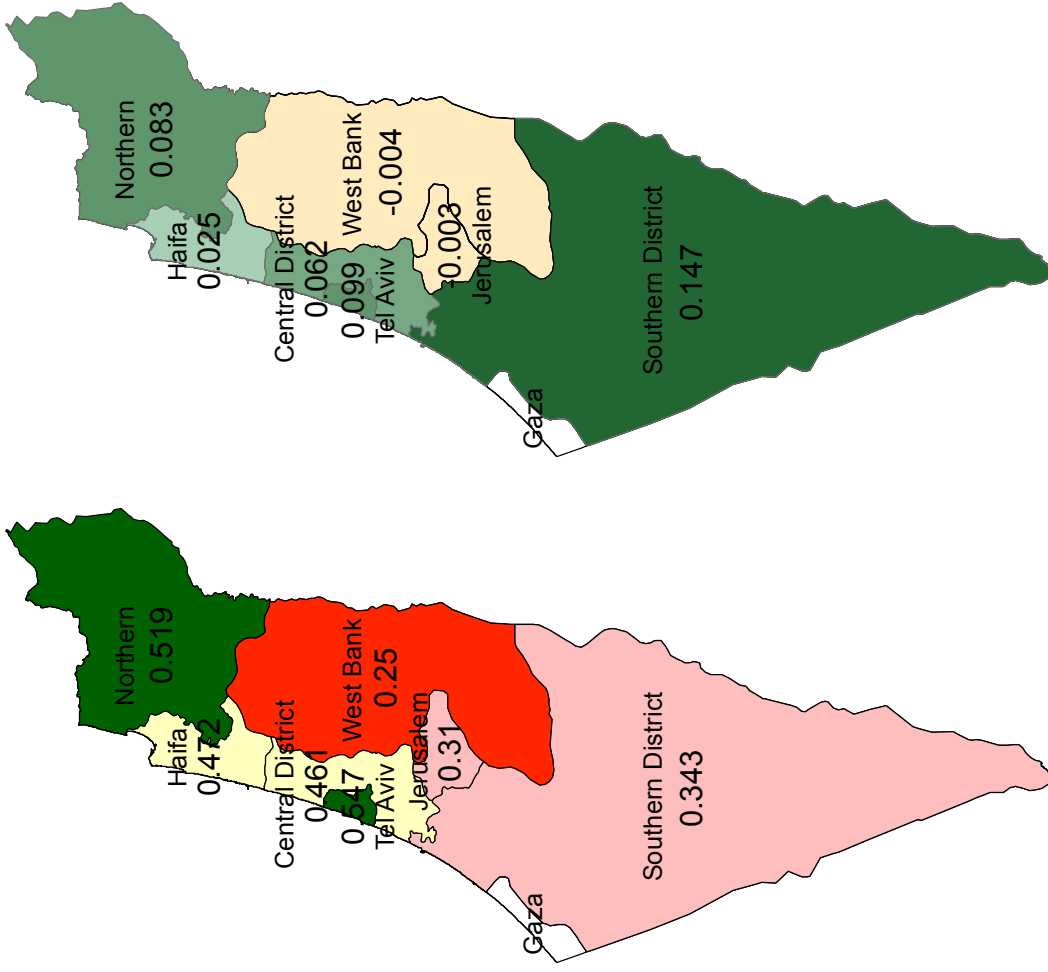
מכירה
 באפשרותך למכור עד 10% מהמניות שברשותך.
 המכירה תהיה לפי המחיר המעודכן שצויין למעלה, 668.1 ש"ח.
 הכסף מהמכירה ייצבר לזכותך במזומן ולא יהיה צמוד לשום נכס פיננסי.
 אנא הקלד את אחוז המניות שברצונך למכור.
 באפשרותך לבחור כל מספר בין 0 ל-10 |10| (נא להזין מספרים שלמים בלבד)
 אם אינך מעוניין למכור את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

המשך

Figure A5: Balancing Tests Simulations



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

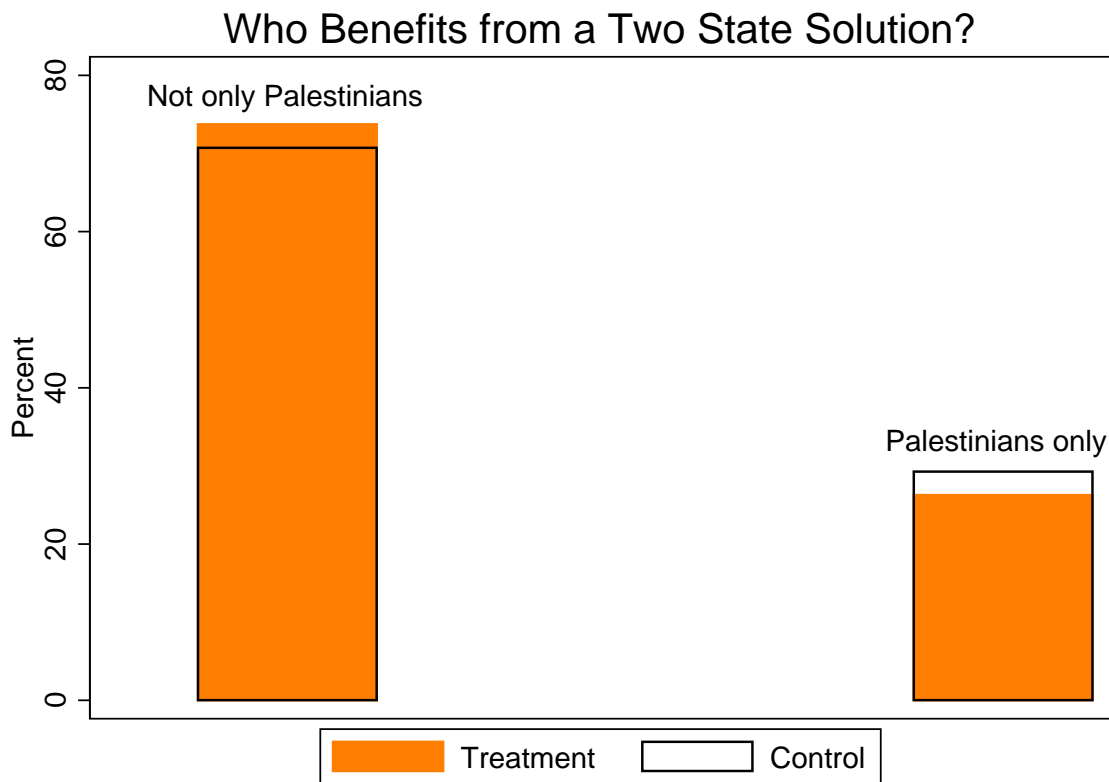


(a) Average Ordered Vote Choice 2015 (b) Treatment Effect

Figure A6: Treatment Effects on the Ordered Vote Choice by Region, 2015 Elections

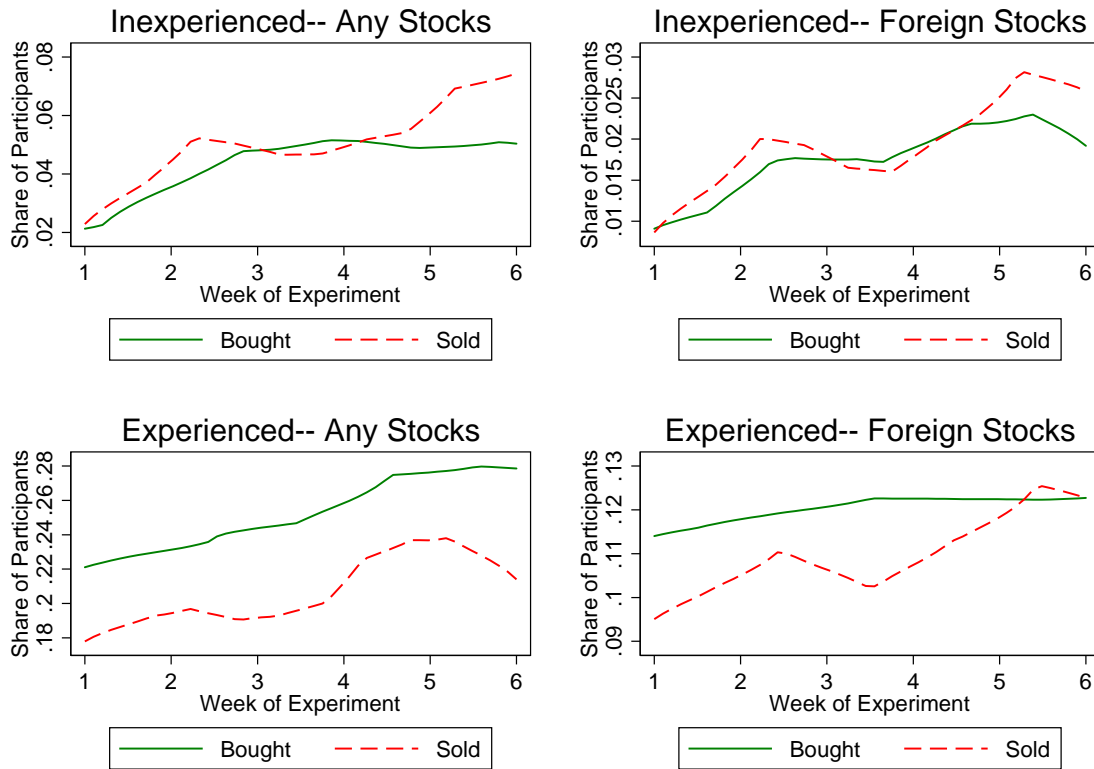
The ordered vote choice is defined as 0=Right, 0.5=Center and 1=Left.

Figure A7: Is a Peace Settlement Zero Sum? Long-Term Differences in 2016



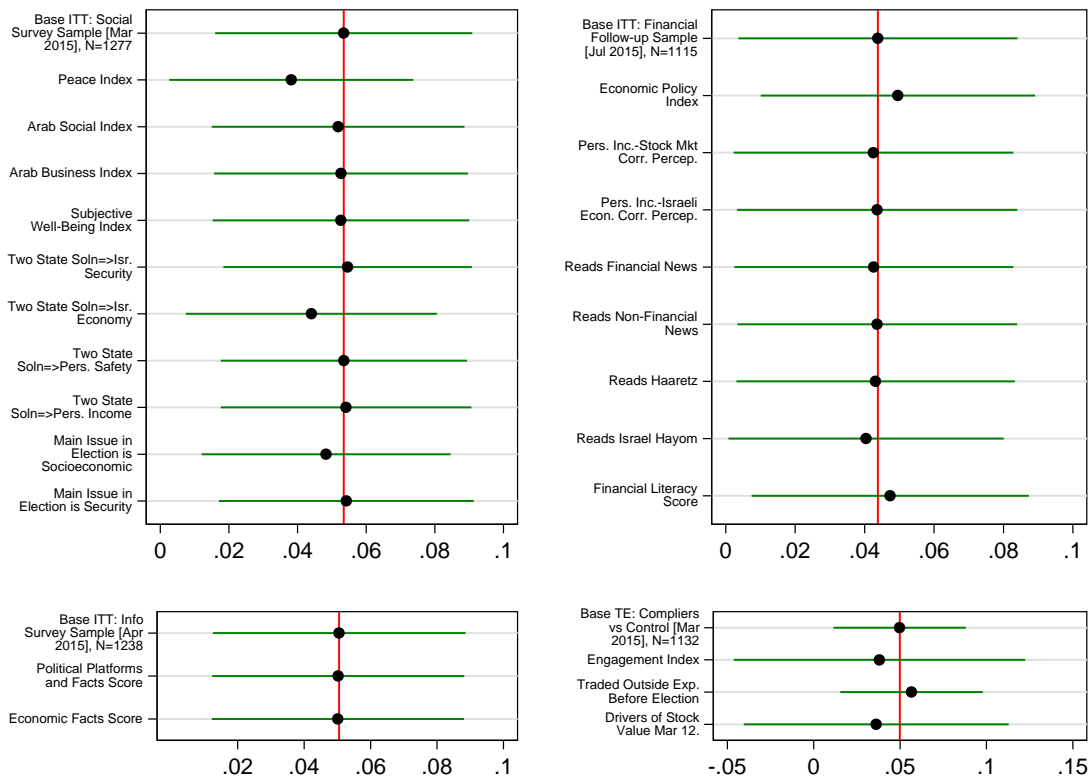
In the 2016 follow-up survey we asked who would benefit from a permanent settlement based around a two state solution. As the Figure reveals, 29.27% of the control believed that a settlement would benefit only the Palestinians– this falls to 26.27% in the treatment group.

Figure A8: Trading Activity Outside the Experiment



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

Figure A9: How Much of the Treatment Effect Can Be Explained by Different Mechanisms?



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