

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

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Abstract

Financial markets expose individuals to the risks and returns of the broader economy. Can they also lead to a reevaluation of the costs and benefits of conflict and peace initiatives? Can this happen even in the context of persistent ethnic conflict, and even affect voting decisions? Prior to the 2015 Israeli elections, we randomly assigned financial assets to likely voters and gave them incentives to actively trade for up to seven weeks. The assets included stocks of Israeli and Palestinian companies. We also randomly assigned their initial amounts and divestment dates. We find that the exposure to stocks caused systematic shifts in voting behavior and increased support for peace initiatives. These shifts appear to reflect two main channels. First, financial market exposure leads individuals to follow financial markets over time and to positively reevaluate the effects of potential peace initiatives on the national economy. Second, exposure to Palestinian stocks increases out-group empathy, reflected in higher support for inter-ethnic social integration. The effects of financial market exposure are larger for the risk averse and for inexperienced investors, who become more like experienced investors in favoring concessions for peace.

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1 Introduction

Can providing incentives to trade in financial markets change attitudes towards peace and conflict, and even voting decisions? This question is crucially important given the role played by persistent conflict in suppressing development around the world.¹ Public attention in conflict-afflicted societies is often focused on the more emotive aspects of strife, such as graphic violence, ethnic animosities and territorial disputes, rather than the economic costs. In this paper, we test whether a historically important, but now relatively neglected, mechanism—exposure to broader *financial markets*—can lead individuals to reevaluate the costs of conflict, changing their attitudes towards peace initiatives and even their voting choices.

We examine this in a context of highly persistent ethnic conflict—that between Israelis and Palestinians. This conflict is of profound importance for global political economy. However, polarized attitudes, conflicting interests and distrust reinforced by more than eighty years of violence have led many to consider it intractable. Yet, even in such a challenging context, financial markets have the potential to significantly affect attitudes towards national policies. Compared to more commonplace daily transactions, financial markets expose individuals to more encompassing national and international risks and considerations. These in turn can have several effects—both on individuals’ personal welfare and on how they evaluate national policies—that we examine.

Individuals that hold financial assets have personal stakes in the performance of the economy, and thus can incur personal financial losses from conflict and gains from peace. This can be particularly significant when individuals share the risks of the opposing party to the conflict by holding their stocks. Such risk-sharing with the out-group may also

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

generate familiarity and reduce animosity across ethnic lines. Beyond personal financial exposure, engagement with financial markets may highlight the effects of conflict—and the effects of policies to address conflict—on the broader economy, while also providing individuals with sets of prices with which to evaluate the risks and benefits of such policies.

Though there are a number of channels through which financial markets can influence political attitudes, empirically measuring their causal effects is very difficult due to the selection processes through which financial markets develop and individuals choose whether and how much to invest, and their specific choice of assets.² 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.³

A month and a half prior to the highly contested 2015 Israeli elections, we randomly assigned 1,345 Israeli voters to either a financial asset treatment or a control group. Individuals in the asset treatment received endowments of assets that tracked the value of specific funds or company stocks from both Israel and the Palestinian Authority, or an endowment of cash they could invest in an asset that tracked the Tel Aviv 25 index. They were also given incentives to monitor the performance of their asset and to make weekly decisions to buy or sell part of their portfolio. We further randomized the dates at which individuals would be entirely divested of their portfolio to be either before or after the elections, and randomly assigned the initial value of the portfolio (either NIS 200 (~\$50) or NIS 400 (~\$100)).

²Due to individual selection, important works examining the relationship between shareholding and firm outcomes are often explicit in interpreting correlational relationships (see e.g. Brav et al. (2008).) An interesting advance is by Bursztyn et al. (2014), who assign a financial asset randomly among those that chose to purchase it, and find that this has effects on take up by peers. On factors that influence national financial development, see, e.g. Rajan and Zingales (2003), Levine (2005), Rousseau and Sylla (2008) and Haber and Perotti (2008).

³Arguably the closest paper 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. We also build on other papers that use observational data to examine the empirical relationships between political attitudes and investment (e.g. Kaustia, Knüpfer and Torstila, forthcoming, Hong and Kostovetsky, 2012, Bonaparte and Kumar, 2013)

Individuals also participated in a series of social and political surveys. This allowed us to track not only their investment behavior but also their social and political views and, crucially, their voting decisions. Importantly, participants did not associate the political surveys to the financial study, thus mitigating potential social desirability biases or experimenter demand effects (Section 4 details how this was achieved and verified).

We find that exposure to financial markets causes large and systematic shifts in voting behavior. Overall, exposure to the stock market increases the likelihood of voting for the left in the 2015 elections by 5-6 percentage points (relative to a left vote share of 25% in the control group). It similarly reduces the likelihood of voting for the right by about 4 percentage points (relative to 36% in the control).⁴ Further, exposure to the stock market increases individuals' willingness to make concessions in order to settle the conflict between Israelis and Palestinians. It raises willingness to support not only the general principle of a two-state solution, but also a range of specific concessions for peace. Exposure to financial markets mainly affects the voting decisions of individuals who had not actively invested in the period preceding the experiment—by 7-8 pp to the left—and has less of an effect on experienced investors. Since experienced investors already tended to vote for pro-peace parties, the experiment appears to align the political attitudes of new investors with investors with prior experience.⁵

In the next section we provide a simple conceptual framework to help elucidate the potential mechanisms that might drive these effects and to guide our empirical investigation. Perhaps the most straight-forward mechanism is that individuals holding stocks on Election Day have *skin-in-the-game*, and can therefore internalize the gains from the peace process. Given that peace overtures tend to raise both Israeli and Palestinian asset prices (Zussman, Zussman and Nielsen, 2008), individuals may be more likely to vote for

⁴A desirable feature, from the standpoint of this academic study, is that Israel has a proportional representation system, with the whole country comprising a single constituency of 5.9 million eligible voters. Thus our study did not affect the election outcomes themselves.

⁵Those who traded before the experiment were 4 percentage points more likely to vote for the left relative to others with similar income and education levels. Of course, such previous trading was not randomly assigned, which is why a field experiment approach is desirable.

parties that favor the peace process. Indeed, Jha (2015) shows that a similar mechanism can explain elite support for representative government in revolutionary England. This channel, however, requires that individuals both understand these relationships and have significant stakes.

A closely related possibility is that financial market exposure also leads individuals to *learn* about the presence of a positive relationship between peace initiatives and financial market performance. Indeed, during the run-up to the elections, Israeli and Palestinian asset prices in our study were negatively correlated with predictions of increased seat shares for the right in daily polls (see Appendix Table A3).

A third possibility is that financial markets expose individuals to the broader economy beyond their everyday transactions and provides them with sets of prices with which to gauge the risks and benefits of policy changes. This can lead them to re-evaluate the effects of both the peace process and the conflict on the broader economy. Insofar as conflict has a negative economic impact (e.g. Eckstein and Tsiddon, 2004) and a two-state solution promises better economic outcomes than alternative scenarios (Anthony et al., 2015), such a re-evaluation may increase support for peace initiatives.

A fourth channel is that exposure to specific financial assets that generate familiarity and risk-sharing with the other party to the conflict (namely the Palestinians) may engender empathy and reduce animosity towards that group. We also explore other potential channels, including wealth effects, informational spillovers from following financial markets into political domains, and effects on subjective well-being.

The evidence is largely consistent with the third and fourth channels: individuals exposed to financial markets seem to reevaluate the effects of a peace agreement on the Israeli economy. They are also more knowledgeable about the stock markets and continue to follow financial news even months after the experimental intervention. Exposure to Palestinian stocks appears to have a particularly strong effect controlling for asset price changes. This does not seem to be due to extra skin-in-the-game but appears to

reflect increased empathy towards Arabs. Specifically, individuals exposed to Palestinian stocks show more support for inter-ethnic social integration (again, controlling for price changes).

In contrast, perhaps due to the relatively small stakes involved, we find more limited evidence for skin-in-the-game effects: the overall treatment effect is at least as strong among individuals that were exogenously divested prior to the elections as among those that held stocks on election day. However, among those assigned to Israeli stocks—arguably the most familiar to our subjects—having a direct financial stake on the day of the election does significantly raise the probability of voting for the left. We find little evidence for the other channels.

Finally, we examine how these effects vary by attitudes towards risk. The effects on support for peace initiatives are greater on the risk averse. This is consistent with a relative increase in individuals' evaluation of the riskiness of status quo policies.

Beyond the literature already mentioned, an important body of research shows that ethnic cleavages can have adverse effects on public goods provision and, importantly, on conflict (e.g. Alesina and La Ferrara, 2000, 2005, Montalvo and Reynal-Querol, 2005). At the same time, conflict itself tends to reinforce ethnic biases (Shayo and Zussman, 2011). This can generate vicious cycles in which ethnic identification and ethnic conflict reinforce each other (Sambanis and Shayo, 2013). Indeed, social and cultural biases relating to conflict have been shown to persist over time (e.g. Nunn and Wantchekon, 2011, Voigtländer and Voth, 2012, Shayo and Zussman, 2015).

Yet, at least as early as Montesquieu (1748), economic “interests” from capitalist activity have been seen as means to offset the “passions” that may be excited by violence (Hirschman, 1977). Economic integration and trade have long been proposed as means to reduce conflict and this has been a major motivation for European economic integration (Schuman, 1950). Empirically, bilateral (though not multilateral) trade between countries is negatively associated with conflict (Martin, Mayer and Thoenig, 2008,

Polachek and Seiglie, 2006). Even within countries, exogenous complementarities that foster exchange between ethnic groups appear to mitigate violence and to foster cultures of tolerance over long periods (Jha, 2013b, 2014). However, to the best of our knowledge, the effects of market interaction on attitudes towards conflict have not been studied at the individual level.

Our study is motivated by the theoretical promise, and in some key instances, historical success, of financial innovations in mitigating conflict (Jha, 2012, 2013a, 2015). In the benchmark model of portfolio choice, in the absence of transaction costs, all individuals should hold the market portfolio of risky assets. This may also align political incentives, as all individuals can gain from policies that improve the returns or lower the risks of the market portfolio, including risks stemming from political instability and conflict (Jha, 2012). However, individuals may face costs to learning both about specific stocks and about how to invest in the market, and the factors that shape the risks and returns of different assets (e.g. Merton, 1987, Huberman, 2001, Grinblatt and Keloharju, 2001). Thus exposure to the stock market and an opportunity to *learn by doing* in a simple setting could alter individuals' appreciation of the risks and benefits faced by the economy from conflict and peace initiatives, and align individuals' incentives with the fortunes of the broader economy. Indeed, exposure to novel financial assets appears to have had historical success at aligning the incentives of assetholders and subsequently mitigating social conflict in three revolutionary states that subsequently led the world in economic growth: England, the United States and Japan (Jha, 2012, 2015, Jha, Mitchener and Takashima, in progress).⁶ The prospect of building broader political support of private property rights protection also motivated privatization in former Communist states (e.g. Boycko, Shleifer and Vishny, 1994, Megginson and Netter, 2001, Biais and Perotti, 2002). The comparative experiences of these cases suggest important design elements, particularly of lowering barriers to secondary sales and divestment gradually and

⁶For a useful comparative analysis of financial development in these settings, see also Rousseau and Sylla (2008).

of providing regular incentives to allow initially non-sophisticated investors to learn by doing. However, whether such approaches could have a causal effect on political attitudes and behavior in a contemporary environment riven by persistent ethnic conflict remains an open question. It is to address this that we now turn.

2 Conceptual Framework

Consider an individual i who is deciding whether to support a peace initiative or a policy that maintains the status quo level of conflict. Suppose the individual's expected return from the peace initiative can be summarized by:

$$\pi_i = R_a a_i + R_w w_i + R_Y Y + R_L L + \gamma \Pi_F \quad (1)$$

where a_i is the individual's financial asset endowment, and w_i is a general set of other personal endowments (including wealth, real estate, etc.). R_a and R_w are the expected rates of return on these assets from pursuing a peace initiative (relative to the status quo). Beyond personal assets, we also allow individuals to care about national outcomes in making this decision. We separate these into the state of the economy (Y), and non-economic issues (L), such as national security, sovereignty over land and other, potentially emotive, issues. The associated rates of return from a peace initiative are R_Y, R_L and we assume $Y, L > 0$. Finally, we allow individuals to care about the benefit to the (foreign) out-group from the peace initiative Π_F , with welfare weight γ (which could be zero or even negative).

How would exposure to financial markets affect this decision? Taking the total derivative yields the condition under which individual support for a peace initiative increases:⁷

⁷Note that we assume, as seems plausible, that a specific individual's financial asset exposure is unlikely to affect the national economy Y or national security L directly, i.e. $dL = dY \simeq 0$. We also assume no change in the expected gain from the peace initiative to foreigners' welfare as a result of asset exposure, i.e. $d\Pi_F \simeq 0$.

$$d\pi_i = R_a da_i + a_i dR_a + R_w dw_i + w_i dR_w + Y dR_Y + L dR_L + \Pi_F d\gamma > 0 \quad (2)$$

Hence, in this setup, exposure to financial markets can raise support for the peace initiative only if at least one of the following is true:

Skin-in-the-game $R_a da_i > 0$: Given an existing belief that the peace initiative increases the return on a particular financial asset ($R_a > 0$, consistent with Zussman et al. (2008)), financial exposure meaningfully increases one's holdings of that asset ($a_i \uparrow$). *Note this effect will disappear if the individual is divested of the asset. Furthermore the effects should be greater for assets that are perceived to be more sensitive to the peace initiative, including those of the other party to the conflict.*

Re-evaluate policy effects on financial markets $a_i dR_a > 0$: Given a positive asset endowment, financial exposure leads individuals to believe that the peace initiative is likely to benefit their existing financial portfolios ($R_a \uparrow$, consistent with the correlation between asset prices and polls in Appendix Table A3). *Note this effect should only be present for individuals with existing financial assets.*

Wealth effect $R_w dw_i > 0$: Given an existing belief that the peace initiative will benefit one's wealth (i.e. $R_w > 0$), financial exposure meaningfully increases one's wealth ($w_i \uparrow$). *Note that this requires meaningful changes in wealth, and, assuming diminishing marginal returns to wealth, the effect should be stronger for poorer individuals.*

Re-evaluate policy effects on personal wealth $w_i dR_w > 0$: Given individuals' (positive) wealth, exposure to financial assets leads those individuals to expect that the peace initiative will increase the value of their existing endowments ($R_w \uparrow$). *Note the effect should be stronger for wealthier individuals.*

Re-evaluate policy effects on the broader economy $Y dR_Y > 0$: Financial markets expose individuals to the broader economy and provide them with prices with which to evaluate the risks and benefits of policy changes. This leads them to re-evaluate the effects of both the peace initiative and the conflict ($R_Y \uparrow$).

Re-evaluate policy effects on national security $L dR_L > 0$: Exposure to financial markets spills over into exposure to information on political issues. This leads individuals to re-evaluate the effects of the peace initiative on national security and other emotive issues ($R_L \uparrow$).

Out-group empathy $\Pi_F d\gamma > 0$: Exposure to out-group companies via asset holdings generates familiarity and risk-sharing that increases the extent to which individuals care about the out-group ($\gamma \uparrow$). Given that the out-group benefits from the peace initiative ($\Pi_F > 0$, consistent with Anthony et al. (2015)), this empathetic response raises individuals' support as well.

In what follows, we first test whether financial asset exposure has any effect on support for peace initiatives, especially as it is revealed in voting decisions. We then evaluate the evidence for the specific mechanisms delineated above. Before we do that, however, we briefly describe the political context and our experimental design.

3 Context

Our study focuses on the March 2015 Israeli general elections. Israel is a parliamentary democracy, with seats in the Knesset assigned to parties proportionately to the votes they receive (as long as they cross a vote threshold). Elections have to 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. This is particularly useful for our study as the 2013 elections provide a recent measure of people’s (pre-treatment) vote choices. We focus on Jewish voters, who form close to 80% of the population.

As in most elections in Israel in recent decades, the main dividing line between the right and the left concerned the Israeli-Palestinian conflict. Parties on the right (led by the *Likud*) largely favor the status quo, viewing concessions for peace as highly risky and likely to lead to a major deterioration of the security situation.⁸ In contrast, parties on the left (led by the *Zionist Union*) see status quo policies, including permitting settlements in the West Bank, as already costly and likely to further risk both Israel’s security and its democracy. Instead they favor restarting the peace process with the goal of finding a permanent solution to the conflict.⁹ While many parties can be clearly classified as

⁸This *holdup* problem is arguably a common feature in a number of other conflicts (Fearon, 1996).

⁹On the eve of the elections, on March 16 2015, the leader of the right-wing *Likud* party, Prime Minister Benjamin Netanyahu, argued that “Whoever moves to establish a Palestinian state or intends to withdraw from territory is simply yielding territory for radical Islamic terrorist attacks against Israel”, and stated that he would not allow a Palestinian state if elected (Reuters, 2015). By contrast, the platform of the *Zionist Union* stated that “reaching a diplomatic settlement [of the conflict] is a foremost Israeli interest and a necessary condition for securing the future of the state of Israel as a Jewish and democratic country” and called for restarting negotiations “with the aim of reaching a permanent settlement with the Palestinians, based on the principle of two states for two peoples.” For a useful

left or right, other parties—which we will refer to as *center*—could in principle join a coalition led by either the Likud or the Zionist Union.

4 Experimental Design

We recruited 1,681 anonymous individual participants from among Jewish Israeli citizens who had previously voted and who participate in a large Israeli internet panel.¹⁰ This larger panel of about 60,000 participants is nationally representative in terms of age and sex, and is commonly used for commercial market research, political opinion polling and academic studies. Though using the panel has some drawbacks—our sample is not entirely representative of the population in all dimensions (see below)—the use of this internet panel has a particularly useful feature: double-blindedness in the identity of the respondents from our perspective and in 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. We describe this in more detail below.

Individuals were invited to a study on investor behavior, and told that they would be participating in several surveys and would be asked questions on various issues. They were informed that they would be entered into a lottery to win either stocks or cash, and that the stocks participating in the study would be from the entire region.¹¹ Among those that consented to participate in the study, we conducted two parallel sets of surveys. Everyone received a set of surveys gauging their social and political attitudes, and in addition those that won the lottery received a weekly series of financial surveys in which they could make investment decisions.

Importantly, the participants did not know the social surveys were linked to the summary and exemplar of the debate, see e.g. Natan Sachs “Why Israel Waits: Anti-Solutionism as a Strategy” *Foreign Affairs*, November/December 2015.

¹⁰By limiting our sample to past voters, we automatically excluded minors. The panel also screened out all Israelis on the US Treasury watchlist.

¹¹To 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.

financial surveys. This was achieved by three features of our design. First, we avoided asking 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. Second, our surveys were among 110 sent to panellists by anonymous sources between February and March. Third, the assets we 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.¹²

To verify whether these measures were effective, the concluding financial survey contained a set of open questions. One of them asked what the participants believed that researchers “can learn from the study”. 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 (and 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 (which are accurate responses given that we study these in a companion paper).

[**Figure 1**]

We over-sampled non-orthodox center voters at twice their vote share (see also Figure 3).¹³ 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), a battery of questions measuring financial literacy, adapted from Van Rooij, Lusardi and Alessie (2011), risk aversion and time preference (from Dohmen et al. (2011) and Benjamin, Choi and Strickland (2010)),

¹²The only defense company in the TA-25, Elbit Systems, has a weight of only 3.26%. The valuations of such firms might respond negatively to peace overtures as in Guidolin and La Ferrara (2007).

¹³That is, individuals who voted for the secular parties Yesh Atid, Hatnu'ah or Kadimah in 2013.

and well-being (Benjamin et al., 2014). A few days later they were invited to an initial social survey which included questions on political behavior and social and political attitudes. Of the 1,681 who completed the initial financial survey, 1,418 completed the initial social survey as well. We restricted our sample to those who had voted in 2013, and screened out those who provided incomplete answers, those who had been grossly inconsistent when asked the same factual questions at different times and those who had completed the survey extremely quickly.¹⁴ This left us with 1,345 participants who we then randomly assigned to the various treatments. The combined result of this sampling strategy is that the sample used for random assignment approximates the broader Jewish population of Israel in terms of geographical region and sex, but tends to be more educated and more secular, but with fewer individuals over 55 and in the top income deciles than are present in the Jewish Israeli population as a whole (See Appendix Table A1).¹⁵

Among these 1,345 respondents, we employed a stratified block randomization procedure designed to increase balance across treatment groups in political and demographic variables.¹⁶ A sample of 309 were assigned to the control group, and 1036 were assigned to the asset treatment. Further, to examine the potential mechanisms mentioned above, participants in the asset treatment were endowed with either cash or stocks from Israel and the Palestinian Authority, each of high or low initial value, and each with redemption date either before or after the elections. Sub-treatments were cross-randomized

¹⁴The 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 between right and left parties.

¹⁵We will show that the main effect of being exposed to assets appears to be similar for those with income at or below the Israeli average as those above.

¹⁶Specifically, 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.

independently. The following table summarizes the basic design and initial allocation.

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

Every week, participants in the asset treatments could reallocate up to 10% of their holdings by buying or selling a particular financial asset. To 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. To simplify and further incentivize participation, we emphasized that there would be no commissions incurred for trading.

202 of the individuals assigned an endowment of cash could buy (and later sell) an asset that tracked the Tel-Aviv 25 Index, while one each traded for indices from Cyprus, Egypt, Jordan and Turkey. The 830 individuals who were 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.¹⁷

We chose to have more than one asset from each country for two reasons: one was to allow us to study investment behavior in company stocks relative to index funds (this is the subject of a companion paper). The second was to study the differential effects of exposure to rising or falling asset prices. Naturally, we could not anticipate the price

¹⁷These 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, though this is a subject of our future research in a different context.

changes themselves, but as assignment to the specific assets was randomized, the exposure to the price changes of those assets was also exogenous.

The specific companies were selected along two criteria: relative orthogonality to the Israeli-Palestinian peace process, as discussed above, 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.

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 asset treatment were given assets initially valued at NIS 200 (around \$50), with the rest valued at NIS 400 (around \$100). These sums are comparable to the average Israeli daily wage of around NIS 312 in December 2014, and are quite significant both compared to the standard pay of NIS 0.1 per question these participants receive in other surveys as well as compared to typical stakes in experimental economics.

All members of the treatment group were invited to complete an instructions survey in which they were informed of their asset allocation and given detailed explanations about the rules of the game. They were also asked a series of quiz questions to make sure they understand the nature of the assets they were assigned and how the value of their assets would be determined. 840 participants completed the instructions survey and agreed to continue. The incomplete takeup may be partly due to server overload, but probably also reflects self-selection as well as differential willingness to hold different assets. Not surprisingly, the lowest takeup was for the low (NIS 200) assets (77.2%, 78.4% and 78.6% for Israeli, Palestinian and cash endowments respectively). For the NIS 400 assets, cash had the highest takeup (91.3%), followed by Israeli (86.1%) and Palestinian (78.9%). 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 third party websites, particularly the Hebrew version of *investing.com*, to allow individuals to independently track and verify the historical performance and current price of their stocks. Participants were then asked to make their investment decisions and had until the opening of the stock market the following week to do so.

All trades were implemented via a trading platform incorporated into our surveys. Specifically, once the markets closed, we calculated for each individual: (1) the current number of stocks they own given previous trading decisions, (2) the value of these stocks given current prices and (3) the amount of cash at their disposal. We then informed them of their trading possibilities, namely how much they could buy (depending on the amount of cash at their disposal) and how much they could sell (depending on the amount of stocks owned). All trades were implemented at the current price, which was constant during the decision window. Sample trading screens are shown in the Appendix. Early divesters had three trading opportunities before the elections and were given their final statement the weekend just prior to the elections. Late divesters could still trade on the pre-election weekend as well as in the two weeks that followed. 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.

[**Figure 2**]

Recall that trading was limited to 10% of an individual's portfolio a week and that individuals were limited to trades between the assigned stock and cash for those in the stock treatments and between cash and a market index for those in the cash treatment. The trading limit encouraged individuals to learn by doing rather than simply divesting their portfolios immediately. The limit also ensured that on election day (or the weekend before the election for early divesters), the portfolio included at least 66% of the experimentally assigned asset. We also asked participants every week whether they invested in or divested from stocks outside the experiment to assess whether the treatment was leading to investment spillovers or being undone by outside trades.

Two days after the elections, on March 19, we surveyed all individuals on their vote choice as well as attitudes towards the peace process. This provided data on the vote choice of 1291 participants. For the voting data, we were further able to augment and compare these responses to the participants' routine updates to the survey company on their demographic and voting data, as well as to our own (anonymous) information survey in April 2015. There were very few discrepancies among the three, again consistent with an absence of social desirability bias.¹⁸ As a result, we have very little attrition in our main outcome variable: we observe the vote choice of 1309 out of the 1345 initially assigned to treatment (97.3% of asset treatment group and 97.4% of the control). Further, there is no evidence of differential attrition across sub-treatments (Appendix Table A2.)¹⁹

Before turning to the data, it is worth discussing some of our design decisions. The novelty of experimentally assigning opportunities to trade financial assets meant that there was limited prior scholarship to inform our choice of treatments and our power calculations. Thus while we considered a number of potentially interesting alternative treatments, for the present study, we ultimately selected those that we believed would

¹⁸Of the 1040 participants who answered both our post election survey and the survey company's, 95.6% reported voting for the same party in both. The coefficient on asset treatment from a regression of the probability of reporting a matching vote in the two surveys is -0.008 (SE=0.0144).

¹⁹There was slightly higher attrition on the questions measuring attitudes towards the peace process, with a response rate of 95% (1277/1345).

be the most informative within the limits of our budget constraint.

First, we considered assigning individuals not only Israeli and Palestinian stocks but also “neutral” stocks – such as the Cyprus, Jordanian and Turkish market indices and even the S&P 500. However, as our main motivation was to study the effects of holding financial assets that allowed individuals to gain from and internalize the returns and risks of reduced conflict, our first priority was to study the effects of exposure to the Israeli and Palestinian asset markets. Assignment to neutral stocks would have been at the expense of these treatments, which is why we ultimately decided to reduce this exposure to only 4 individuals.

Second, we decided not to allow individuals to trade their initially assigned stock for other stocks but only for cash. The main reason was that we were exposing many individuals to trading in the stock market for the first time. Limiting the choice to a single asset per individual made the investment decisions simpler for novices to understand and for us to interpret.

Finally, our treatment combines a small cash transfer with an incentive to follow the stock markets. Two interesting alternatives would have been to include separate treatments, one which was purely a cash transfer, and one which provided information on stock performance without any monetary stakes. We did not believe that in the Israeli context, giving people \$50 to \$100 (comparable to a single day’s wage on average) would by itself have meaningful effects on their vote and political attitudes a month later.²⁰ However, by using a rich set of survey questions, our chosen design allows us to assess the importance of wealth effects. We also believed that providing monetary incentives for engagement in the stock market would be more consequential than a pure information treatment.

²⁰This is also why we limited their ability to divest immediately.

5 Data

Table 1 shows the means of a range of pre-treatment variables for those assigned to the control group, those assigned to receive financial assets, as well as within the main sub-treatments. We restrict attention to those for whom we have the 2015 vote outcome. As expected from stratified random assignment with low attrition rates, for almost all variables there are no significant differences across treatment and control. Importantly, we know how individuals voted just two years prior to the 2015 elections that we study. As the top two rows show, and consistent with our over-sampling of center voters, about 24% of our sample voted for right-wing parties and about 13% voted for left-wing (pro-peace process) parties, with similar proportions across treatment and control groups.²¹ Around 36% of our sample in both the treatment and control groups reported having traded stocks in the six months prior to the experiment. Both groups have similar time preferences (based on standard hypothetical choices) and similar financial literacy scores (based upon a test that we administered, adapted from Van Rooij et al. (2011)). The groups are also balanced by basic demographic characteristics, including sex, marital status, education, religiosity, geographical location and income. There are two exceptions that show small but statistically significant differences: those in our asset treatment are slightly younger (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 4.4 for the control).²² We control for age with both linear and quadratic terms in our regressions, and will show that the treatment effects in fact tend to be stronger on the *risk-averse*.

Finally, individuals in treatment and control show similar degrees of support in their pre-treatment attitudes towards a peace settlement and specific concessions for peace.

²¹Right wing parties in 2013 include Likud Beyteynu and Habayit Hayehudi. Left wing parties include HaAvoda (Labor), Meretz, and Hadash (none of the individuals in our sample voted for Arab parties in 2013 except for Hadash). As discussed below, we use various methods, including controls for the 2013 vote and individual fixed effects, to address any pre-treatment imbalances.

²²This measure is consistent with a separate measure of risk aversion we constructed based upon hypothetical lotteries.

Close to 60% of the control and the treatment group support a two state solution, though there is less support for specific concessions, especially the splitting of Jerusalem.

[Table 1]

6 Results

We begin with our central question: whether exposure to financial markets changed votes in the 2015 elections as well as attitudes towards the peace process – and if so, in what direction. Figure 3 shows the raw vote shares across the asset treatment and control groups. The left panel shows vote shares in the 2013 elections (prior to our intervention). Consistent with Table 1, treatment and control groups had similar distributions of votes across left, right and center parties in 2013. However there are substantial differences in their voting decisions in 2015 (right panel). While 24.6% of the control voted for the left, the left won 31.0% of the vote among the treatment group. At the same time, right parties won 31.3% of the votes of the treated group, down from 35.9% in the control.²³

[Figure 3]

We now conduct a more thorough analysis. To be conservative, we primarily present Intent to Treat Estimates, comparing the control group to those who were experimentally assigned the treatment regardless of whether they actually received and traded in the assets. Later we also report Treatment on the Treated estimates.

Table 2 presents OLS estimates of the effect of assignment to asset treatment on the probability of voting for the left in the 2015 elections. Column 1 shows the difference between treatment and control groups without controlling for other factors. As in Figure

²³The Left parties in 2015 are the Zionist Union, Meretz and the Arab Joint List. The Right parties are Likud, Habayit Hayehudi, Israel Beytenu & Yachad-Ha'am Itanu. Center parties are Yesh Atid, Kulanu, Shas and Yahadut HaTorah. There can be some disagreement about the designation of Ultra-Orthodox parties— Shas and Yahadut HaTorah— as center parties. Therefore our analysis focuses on voting for unambiguously left and right parties.

3, the asset treatment is associated with a 6.4 percentage points increase in the probability of voting for the left. Given that the 2015 vote share for the left among the control group was 24.6% (a proportion similar to the 25.3% overall vote share for Jewish left parties in the 2015 elections), these are substantial effects.

As seen in the histogram and in Table 1, even though we stratified on the 2013 vote, there are slight differences in the initial vote pattern in 2013 between the treatment and control groups among those for whom we have a recorded vote in 2015. In Column 2, we therefore control for an individual's 2013 vote. In addition, we expect that those with experience of trading in shares prior to the experiment might vote differently. We thus also control for whether an individual had bought or sold shares in the six months prior to the experiment. The explanatory power (R^2) of the regression increases from 0.003 to 0.284 with the addition of these regressors. However, the estimated treatment effect remains significant at 5.6 percentage points. It is also interesting to note that those that had invested prior to the experiment are already more likely to vote for the left by 10 percentage points even conditioning on the treatment. Thus, it appears that those who invested due to the treatment become more like existing investors in their political choices. Naturally, past financial market activity is not randomly assigned, and this correlation may in part reflect a number of other factors. However, the fact that treated individuals seem to converge towards the voting behavior of existing investors suggests that the effects of financial exposure on political choices that we uncover are more likely to persist over time.

Since our randomization was conducted within 104 stratification blocks, in Column 3, we add fixed effects for each block. Recall that these blocks were constructed to stratify on the 2013 vote, sex, past trading of stocks, attitudes towards stocks from Arab countries, geographical region, survey discrepancies and risk aversion. They thus contain information on attributes that can both influence political behavior and financial activity and be influenced by past trading. Controlling for these blocks indeed increases the R^2

to 0.42. However, the treatment effect remains stable and significant at 5.4 percentage points.

Column 4 presents our full specification. Along with past vote, trading history and strata fixed effects, it controls for differences in demographics and pre-treatment economic preferences. These include sex, age (and age squared), education categories (post-secondary, BA student and college graduate, relative to high school and below), religiosity categories (traditional, religious and ultra-orthodox, relative to secular), seven regions (North, Haifa, Jerusalem, Tel Aviv, the West Bank and the South, relative to the Center), five income categories, an indicator for married, self-reported willingness to take risks (on a 1 to 10 scale), an indicator for patience above the median (inferred from a series of hypothetical choices) and the individual's financial literacy score. Many of these variables correlate with an individual's propensity to vote for the left in 2015: more religious individuals are less likely to vote for the left, whereas the educated are more likely to vote for the left. However, even controlling for these factors, the treatment effect remains at 6 percentage points.

Experienced investors have naturally been exposed to financial markets before and so one may expect them to be less responsive to the treatment. Further, these individuals are also better positioned to be able to *undo* the treatment, selling assets so as to retain their preferred portfolios. In Columns 5 and 6, we therefore split the sample among the 36% that reported having traded within six months prior to the experiment and the majority that had not. The asset treatment had no effect on the propensity to vote left among those with trading experience (Column 5). In contrast, those without experience are around 8.3 percentage points more likely to vote for the left as a result of the treatment (Column 6).²⁴

[Table 2]

²⁴These differences in the treatment effects between experienced investors and the inexperienced also appear inconsistent with the presence of *Hawthorne* effects stemming from mere exposure to the study, that should therefore be common for all treated participants. Further, as we discuss below, a number of effects persist months after the experiment.

Table 3 repeats the exercise examining the propensity to vote for the right (relative to the left or center). Overall, the patterns are similar and consistent, though the effect sizes are somewhat smaller. Individuals exposed to the asset treatment are around 4 percentage points less likely to vote for the right across specifications. As Column 2 suggests, those who traded in the previous six months are also 4 pp less likely to vote for the right, controlling for vote in 2013 and for treatment status. However, the treatment effect here is similar across experienced and inexperienced investors (Columns 5-6).²⁵

[Table 3]

A valuable feature of our dataset is that it contains information about voting in both 2013 and 2015. This allows us to directly evaluate how the treatment affects within-individual changes in voting behavior over time. Furthermore, the panel variation allows us to quantify the broader extent to which voting decisions have changed over time among our subjects (as seen in Figure 3). Such changes may reflect overall voter polarization, the changing composition of parties, their ideological stances and other factors.²⁶ In Table 4 therefore, we estimate the following difference-in-difference equation:

$$Vote_{it} = \beta_0 + \beta_1 AssetTreatment_i + \beta_2 Post_t + \beta_3 (AssetTreatment \times Post)_{it} + \alpha_i + \epsilon_{it}$$

where $Vote_{it}$ is an indicator for the vote of individual i in year $t \in \{2013, 2015\}$; $Post_t$ is an indicator for 2015, and ϵ_{it} is an error term clustered at the individual level. In some specifications, we also include individual fixed effects α_i . β_1 measures any potential differences in the initial propensity to vote for a particular grouping of parties across treatment and control. β_2 captures differences between the 2013 and 2015 elections. The

²⁵A multinomial logit analysis of party choice in 2015 (controlling for vote in 2013 and trading experience) suggests that the asset treatment effects mainly reflect a significant decrease in the probability of voting for the right-of-center Likud and centrist Yesh Atid parties in favor of the left-of-center Zionist Union party (results not shown).

²⁶For example, one of the main center parties in 2013, Hatnuah, created a joint list with the Labor Party. The centrist Kadima party disappeared. On the other side, Moshe Kahlon, a former member of the Likud, created a new centrist party called Kulanu.

within-individual treatment effect is measured by β_3 : the difference in the change in the vote between 2013 and 2015 for the treated individuals relative to the control.

[Table 4]

The first four columns of Table 4 present the effects on voting for the left, while the last four present the effects on the right. Consistent with Figure 3, voting in our sample polarized between 2013 and 2015, with an increase in the vote share of both left and right by 12.3 and 11.3 percentage points respectively (first row, Columns 1,5). The second row provides a useful placebo test of our randomization: those treated with assets are not more likely to vote for either left (Column 1) or right (Column 5) before actually being exposed to treatment in 2015. However, consistent with our previous findings, individuals treated with assets were 5 percentage points more likely to vote for the left, and 4 percentage points less likely to vote for the right (though the latter effect is not statistically significant). In Columns 2 and 6 we introduce individual fixed effects. The estimated treatment effects are unchanged.²⁷

In Columns 3-4 and 7-8, we split the sample between *ex ante* experienced and inexperienced investors. Once again, the inexperienced show larger treatment effects on their propensity to vote for the left and similar effects for the right.²⁸

6.1 Attitudes towards Peace

So far we have seen that exposure to financial assets moves individuals' votes in the 2015 elections towards left parties that are more supportive of the peace process. However, votes for these parties may have been driven by other policy dimensions. In order to

²⁷It is worth noting that the vast majority of voters who switched blocks between 2013 and 2015 moved to an adjacent grouping of parties along the political spectrum (e.g. from right to center or from center to left). Only 18 out of the 1309 moved all the way across the spectrum.

²⁸Interestingly, while both subsamples moved away from the center in general, experienced investors moved more to the left between 2013 and 2015, while the inexperienced moved more towards the right (first row).

evaluate whether the treatment also resulted in increased support for peace, we asked the following series of questions in the post-election survey:²⁹

To what extent do you agree or disagree with each of the following principles for settling the conflict between Israel and the Palestinians?

[Possible responses: disagree, tend to disagree, tend to agree, agree]

- A *Two states for two peoples* [57% agree/tend to agree]
- B *The 1967 Borders will be the borders between the two countries with a possibility of land swaps* [40% agree/tend to agree]
- C *Jerusalem will be divided into two separate cities: Arab and Jewish* [27% agree/tend to agree]
- D *Palestinian refugees will receive adequate compensation and be allowed to return to the State of Palestine only* [40% agree/tend to agree]

Question A asks a broad question about support for a two state solution. Though there is some disagreement, more than 57% of our sample support this general principle after the elections. Questions B through D ask more specifically about the concessions individuals are willing to make to resolve the conflict. Notice that the share that agree to these specific concessions falls considerably, with only 27% supporting the splitting of Jerusalem and 40% supporting the other two principles. These numbers are consistent with the figures derived from a representative sample of the Israeli Jewish population in 2013 (Smooaha, 2015).³⁰

Below we show the effect of the asset treatment on an index composed of all four questions. However, it is illuminating to consider each individually. The first three columns of Table 5 present ordered probit estimates of the effect of the asset treatment on responses to each of the four questions on a four-point scale where 1 represents *disagree* and 4 *agree*. Each regression includes the full set of controls from Column 4 in Table 2.

²⁹These questions were drawn from the 2012 Index of Arab-Jewish Relations in Israel Smooaha (2013)

³⁰The overall trends in the populations reveal either stable or falling support for these principles between 2003-4 and 2013. Specifically, support for the two state solution among the Jewish population fell from 71.3% in 2003 and 66.7% in 2012 to 61.5% in 2013. Support for the more specific principles has been either stable or falling since 2003-4, reaching roughly the same levels seen in our data. In 2013, support for 1967 borders with land swaps was 40.3% (44.2 in 2003), for the splitting of Jerusalem it was 22.6% (23.3 in 2004) and for the return of refugees it was 48.2% (62.6 in 2003).

Overall, the asset treatment has a positive effect on the extent to which individuals agree with each of the four principles underlying a potential peace agreement (Column 1). The effects are stronger for the more specific and less widely accepted concessions (Panels B-D), and, once again, are more pronounced among those that did not trade prior to the experiment (Columns 2 and 3).

[**Table 5**]

To help unpack and interpret the results, we present OLS estimates of the treatment effect among inexperienced investors on their probability of agreement rather than disagreement (Column 4) and of voicing *strong* disagreement on each question (Column 5). Inexperienced participants that were exposed to the asset treatment are 7pp more likely to support the two state solution, but show smaller movements from disagreeing to agreeing on the specific concessions (Column 4). In addition, as Column 5 reveals, the treatment has a particularly large effect on reducing strong disagreement to the various principles.

6.2 Treatment on the Treated and Reweighted Estimates

The Intent to Treat Estimates that we have presented so far are not only conservative, they are particularly germane when one is interested in the treatment effect taking into account that some individuals may not participate. However, it is useful also to measure the treatment effect on those that participated. Table 6 presents estimates of the treatment effect on the treated for our main outcomes, using assignment to treatment as an instrument for treated status. Along with vote for the left and the right, we generate an index of support for peace concessions by simply summing over the four responses to each of the four peace principles above. This creates a scale that ranges from 4 (disagree with all) to 16 (agree with all).

[**Table 6**]

Columns 1 and 2 of Table 6 compare the familiar Intent to Treat (OLS) estimates with Treatment Effect on the Treated (IV-TOT), using the full set of controls. As one might expect, the TOT effects are higher. They indicate a 7.4 percentage points increase in the probability of voting left, a 5.4 pp decrease in voting right, and significant increase in support for peace concessions. Once again, these effects are larger for those without prior trading experience (Columns 3-4).

Recall also that we over-sampled center-voters. These voters could be considered more likely to move in response to treatment than their more ideological counterparts. In Column 5, we reweigh the sample so as to reflect the actual vote share of Jewish parties in 2013. The size of the estimated effect is lower (a 4.4 pp increase) on the probability of voting left, but is similar (a 5.1 pp decrease) for voting right, and for support for peace concessions. This reflects the fact that the treatment moves individuals from the right to the center, and from the center to the left (see footnote 27). Reducing the relative weight on *ex ante* center voters naturally attenuates the effect on the move from the center to the left and strengthens the effect on the move from the right to the center.

To summarize: we find that trading in financial assets has a strong effect both on individuals' voting decisions and their attitudes towards making specific concessions to resolve the conflict. These effects are particularly pronounced among those individuals who lacked prior experience in the financial markets. In the next section, we examine why.

7 Mechanisms

Our research design includes sub-treatments and a rich set of survey questions which allow us to explore some of the main mechanisms underlying the treatment effect of receiving financial assets. We first report the raw partial effects of each sub-treatment on the main outcomes, and then examine the evidence for the specific mechanisms outlined

in Section 2.³¹

7.1 Sub-Treatments

Participants were randomly assigned to different sub-treatments that varied on several dimensions: type of initial endowment (stocks or cash tradable for an index), the national origin of the stocks (Israeli or Palestinian), the initial value of the asset (low (200 NIS) vs high (400 NIS)); whether the asset tracked a company stock or an index fund; and the redemption date (before or after the elections). Four individuals in the cash treatment traded non-TA25 indices. Table 7 presents the partial effects of these factors on voting. The first column shows the mean effects, without any controls. While the left won 24.6% of the vote in the control group, asset exposure had positive effects on the left vote, regardless of whether individuals were initially endowed with Palestinian stocks, Israeli stocks or cash (Panel A). In fact, Israeli asset exposure, whether through stock endowments or cash that allowed individuals to buy the index, had similar, if not higher effects on left vote than exposure to Palestinian stock. These patterns persist when we add (in Column 2) the full set of controls from Table 2. Similar patterns can be seen with respect to voting for the right in Panel B.

[Table 7]

Our prior was that Palestinian asset exposure might have greater effects, both through a skin-in-the-game effect (since Palestinian assets are likely to be more sensitive to the peace process) and through an empathy effect, by generating familiarity and exposing individuals to shared risks with the other party to the conflict. We also expected that the relative price performance of each asset would strengthen or attenuate the resultant treatment effects by affecting willingness to participate in the stock market (as in Malmendier and Nagel, 2011). However, we could not anticipate the actual changes in

³¹Appendix Table A6 reports the treatment effects on all questions from the post-election social survey not reported in the main text.

asset prices prior to the elections. As it turned out, the Israeli assets in our study all out-performed the Palestinian assets (see Figure 2).

To address this, in Column 3 we control for the percentage price change of the asset to which each individual was exposed.³² Increases in an asset's price relative to its initial value are associated with somewhat higher probability of voting for the left and lower probability of voting for the right, but these associations are not precisely estimated. However, controlling for price changes, exposure to Palestinian stocks does in fact have an additional effect, increasing support for the left by 12pp and reducing it for the right by almost 11pp. The effects are even larger once we account for the incomplete takeup in the various treatments by estimating TOT effects (Column 4). The Israeli stocks and cash treatments have limited influence beyond what is captured in the asset price exposure. With respect to other sub-treatments, effects tend to be stronger for company stocks relative to index funds. All 4 non-TA25 individuals endowed with cash voted for center parties. Effects also vary with timing of divestment and with the level of stakes. We discuss these in more detail below as we systematically examine the mechanisms outlined in Section 2.

[Table 8]

7.2 Skin in the Game

We begin with the first potential mechanism: that the asset treatment provides direct skin-in-the-game by giving individuals a financial stake in the election outcome. Recall that late divesters that were initially assigned stocks necessarily have higher skin-in-the-game on election day than those assigned cash. Table 8, Column 1 therefore breaks the overall asset treatment effect on voting left (of 6pp in Table 2, Column 4) into cash and stock. Perhaps surprisingly, there is no evidence that the effect is larger for the

³²For the cash treatment this means the price change of the index that could be traded for (i.e., the Tel Aviv 25 for all but 4 participants). As noted above, since the assignment to the assets was exogenous, so too is the price change.

stock treatment. If anything, the cash effect is somewhat higher (9.2pp for those assigned cash to buy the TA-25 compared to 5.4pp for those assigned stock). Column 2 provides a more direct test by comparing the treatment effects among those who had experimentally-assigned skin-in-the-game on election day and those who were already divested. The average effects of the stock treatment are very similar regardless of the timing of divestment. The effect of the cash treatment is, in fact, larger for *early* divesters, suggestive of a stronger effect for realized gains and losses. Columns 5-6 repeat these tests for voting right, with even stronger conclusions. If anything, the cash effect appears stronger than the stock effect and early divestment has stronger effects than late divestment.³³

[Figure 4]

While there is little evidence for an overall skin in the game effect, there may still be differences across assets. If, for example, the expected returns from a peace initiative R_a are larger for Palestinian than for Israeli stocks, holding Palestinian assets should induce stronger shifts to the left. Indeed, holders of Palestinian stocks are relatively more likely to view inter-state relations and the threat of conflict as the most important determinants of their assets' value (see Figure 4, leftmost panel). On the other hand, Israeli asseholders are more likely to view their assets as responsive to the state of the national economy (right-most panel), which they may also believe will benefit from peace.

Columns 3 and 7 of Table 8 separate the effects of divestment timing by the nationality of the assets. Among those endowed with Israeli stock the treatment effects do appear to be greater for those that hold assets on election day, at least with respect to voting

³³The assignment to stocks versus cash or early and late divestment are not perfect proxies for the value of stocks actually held on election day, in part due to individuals' trading decisions. As stock holdings on election day are endogenous, we construct an instrument using the experimentally assigned components of the individual's portfolio (i.e. initial value, price of the specific asset and redemption date). Specifically, we calculate for each individual the value of stock holdings on election day had they decided to simply hold and retain their assigned assets. As Appendix Table A4 reports, we find no evidence for a separate effect of the actual asset holdings on election day beyond the initial exposure. This is also true breaking the asset treatment down into Palestinian, Israeli and Cash treatments.

for the left. For late divesters, the treatment increases the probability of voting for the left by 9 pp (relative to 2.6 pp for early divesters). In contrast, the Palestinian stock treatment effect is primarily driven by *early* divesters (raising the probability of voting left by 8.7 pp and lowering that of voting right by 8.4 pp). Columns 4 and 8 add controls for price changes, yielding similar qualitative results.

In summary, the skin-in-the-game mechanism appears limited to the Israeli asset treatment and to shifts in the left vote, and cannot explain the effect of Palestinian asset exposure or of cash.

7.3 Out-Group Empathy

Our analysis so far suggests strong effects of Palestinian stock treatments, particularly when controlling for price changes (Table 7). As we have seen, these effects are also not primarily due to skin-in-the-game (Table 8). We now explore a second potential source for these effects, namely that exposure to Palestinian companies via asset holdings generates familiarity and risk-sharing that increases the extent to which individuals empathize with (or reduce animus towards) the Palestinians ($\gamma \uparrow$). To assess this issue, we examine whether the effects seen in Table 7 also show up in responses to questions that are not directly related to the conflict, yet capture empathy and animosity towards Arabs as revealed in willingness to engage in inter-ethnic social and business interactions. Specifically, our post election survey included the following questions.³⁴

The following statements deal with the relations between the Arab and Jewish citizens of Israel. Please state the extent to which you would agree to this type of relationship. [Possible responses: 1: disagree, 2: tend to disagree, 3: tend to agree, 4: agree]

A *Arabs will attend Jewish high schools.* [44% agree/tend to agree]

B *Arabs will live in Jewish neighborhoods.* [39% agree/tend to agree]

³⁴The social questions are taken from Smooha (2013). Among the Jewish population in 2012, he finds that the proportions agreeing on (A) were 55% and on (B) 46%. These figures are also comparable among the Arab population. The business questions were our own.

C *Arabs and Jews will open joint businesses.* [68% agree/tend to agree]

D *Arabs will be managers in Israeli companies.* [56% agree/tend to agree]

Table 9 estimates treatment effects on these empathy questions using the same specification as in Table 7, Column 3. As before, we report results both using scales that sum up the responses to both sets of questions, as well on the probability that individuals agree or disagree with each specific statement. As the table reveals, the treatment effects on voting seen in Table 7 are paralleled by effects on willingness to engage in social interactions across ethnic lines. In particular, *ceteris paribus*, exposure to Palestinian stocks raises both the probabilities that individuals agree that Arabs attend Jewish high schools and that they live in Jewish neighborhoods by 15-18 pp. There is also a strong effect of exposure to increasing asset prices. In contrast, the effects on business relations, while positive, are smaller in size and statistically insignificant.³⁵

[Table 9]

7.4 Reevaluating the Policy Effects on the Broader Economy

Beyond potentially providing skin-in-the-game or inducing empathy, financial markets expose individuals to the broader economy and provide them with prices with which to evaluate the risks and benefits of policy changes. As mentioned above, this may lead individuals to re-evaluate the effects of both potential peace initiatives and of status quo policies ($R_Y \uparrow$). This would help explain the large effects among early divesters documented in Table 8.

In the survey immediately after the elections, we asked individuals directly how a peace agreement would affect Israel's economy, Israel's security, their personal safety and their personal economic condition. Panel A of Table 10 shows ordered probit estimates

³⁵As Appendix Table A6 reveals, other effects of Palestinian asset exposure also appear consistent with increased empathy. Those exposed to Palestinian stock are more supportive of Arab parties joining the government and less likely to blame the Palestinians for the conflict.

of the effects of asset exposure on these views, along a five point scale (from “worsen a lot” to “improve a lot”). Notice that while on average treated individuals are as likely as the control to expect to benefit personally from a two state solution, they become more likely to predict that *Israel’s* economy will benefit from a peace settlement. As with the voting effects, these results are especially pronounced for inexperienced investors.

[**Table 10**]

There are two related channels that might lead to such a reevaluation. One is that individuals pay more attention in the short-term to economic risks and benefits. A related possibility is that familiarity with the stock market reduces the costs to learning and understanding these considerations over time. Indeed, when asked an open question in the end-line financial survey about “what did you learn from the study”, by far the most common responses were to voice an increased familiarity with and confidence in investing in financial assets and the stock market (see Appendix Figure A3). A month after the elections (on April 17), we also asked our participants a battery of five questions assessing the accuracy of their knowledge about prevailing economic conditions, including the unemployment rate and inflation rate. As shown in Table 10 (Panel B), the asset 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.

The effects appear to persist over time. Four months after the elections, on July 19, we asked individuals which news outlets they read regularly. As Panel C reveals, while treated individuals do not change their consumption of non-financial news, they significantly increase the number of financial newspapers that they regularly read. In a companion paper, we also find significant effects on individuals’ financial literacy scores four months after the study, as well as reported investment and willingness to invest in specific types of stock in the future. Taken together, our results suggest that the effects

on individuals' interest and engagement with financial markets appear to persist in a manner consistent with learning and reevaluation.

7.5 Reevaluating the Policy Effects on National Security

A key component of the political debate on the peace process concerns the impact of concessions, such as a withdrawal from the West Bank, on national security. As our theoretical framework implies, one mechanism by which exposure to financial markets might affect political attitudes is by leading individuals to re-evaluate the effects of the peace process on national security ($R_L \uparrow$). This could occur if, as a result of the treatment, individuals follow financial news and incidentally also gain information on political issues.

However, Table 10, Panel A, shows no evidence of a direct effect of the asset treatment on individuals' evaluation of how a peace settlement would impact Israel's security, either in general or for the inexperienced in particular. Further, in our April knowledge survey, we included a battery of questions assessing individuals' political knowledge.³⁶ Again, we find no evidence that the asset treatment affected individuals' political knowledge (Panel B). We also find no evidence that the changes in media consumption documented in Panel C are associated with either increased consumption of left-leaning news sources (*Haaretz*) or decreased consumption of right-leaning outlets (Sheldon Adelson's *Israel Hayom*) which might have led to a reevaluation of national security issues.

7.6 Other Mechanisms

Finally, we evaluate the remaining channels that our conceptual framework implies might explain the results. One possibility is that financial exposure leads individuals to believe that the peace process is likely to benefit their existing financial portfolios ($R_a \uparrow$, con-

³⁶These included 13 questions on the positions of the candidates (*What is Herzog's position concerning the establishment of a Palestinian state as part of a political settlement?*), events during the run-up to the elections (*What was the main subject of Netanyahu's Congress speech?*), and simple factual questions (*Who was Minister of Defense in the previous government (until December 2014)?*).

sistent with the correlation between asset prices and polls in Appendix Table A3). This reevaluation effect should be particularly pronounced for individuals with pre-existing financial assets. However, the effects we find tend to be small for experienced investors (e.g. Table 2).

A closely related mechanism is that exposure to financial assets leads individuals to reevaluate the effects of the peace process, such that it will increase the value of their existing endowments ($R_w \uparrow$, implying $w_i dR_w > 0$). However, as Table 10, Panel A suggests, the asset treatment does not appear to lead individuals to reevaluate the effects of the two state solution on their personal economic situation on average. Moreover, we find no evidence that the effects are larger for high-income individuals (see Appendix Table A5).

The third channel is a wealth effect: financial exposure might meaningfully increase one's wealth ($w_i \uparrow$ implying $R_w dw_i > 0$). However, this would explain the results only if individuals *already believe* that the peace process will benefit their wealth (i.e. $R_w > 0$). Note also that this requires meaningful changes in wealth, and, assuming diminishing marginal returns to wealth, the effect should be stronger for *poorer* individuals.

It is somewhat unlikely that the small initial amounts we provide—at most 400 NIS (\$100) for the high treatment—would change an individual's overall wealth meaningfully enough to influence voting a month later.³⁷ We can, however, examine whether the asset treatment induced any measurable differences in individuals' subjective well-being. Immediately after the elections, we asked individuals not only about their overall life satisfaction but also a battery comprising the top 10 predictors of well-being based on Benjamin et al. (2014, Table 2).³⁸ As Table 10 Panel D suggests, however, the asset treatment did not significantly affect *any* of these indicators of subjective well-being.³⁹

³⁷For reference, the average daily wage in Israel was NIS 312 in December 2014.

³⁸We excluded the mental health question, which might have been considered intrusive in the cultural context.

³⁹This also rules out the possibility that the treatment effect is due to changes in mood or affective states of mind.

We also test whether the effects of asset exposure are larger for the poor, as one would expect with the direct wealth effect. Appendix Table A5, Columns 1 and 5 estimates the interaction of the asset treatment with pre-treatment reported income. We find no significant difference in the effect for the relatively poor.⁴⁰ Taken together, our treatment effects do not appear to be due to a wealth effect or an overall re-evaluation of the effects of the peace process on individuals' wealth or existing financial assets.⁴¹

7.7 Risk Aversion

Our analysis thus far suggests that exposure to the stock market leads individuals to reevaluate the returns from the peace process relative to the status quo level of conflict. Such a reevaluation could be either due to a reduction of the perceived risks of a peace initiative, or to an increased perception of risks associated with the status quo policies, or both. To shed light on this further, we use the information we collected on attitudes towards risk.

If the asset 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

⁴⁰A related way to test for the wealth effect is to exploit the fact that any wealth effect should be consistently greater for those in the high allocation condition, rather than having heterogeneous effects that magnify the other effects already discussed. Appendix Table A5, Columns 2-4, 6-8 show, however, that the effects of being assigned to the high treatment are at best, mixed. There is no effect on voting left, though a stronger effect on the right. This right effect comes from individuals assigned to a high allocation of Palestinian stocks, in particular.

⁴¹Appendix Table A6 (top row) also rules out a simple extension to the conceptual framework. Specifically, there is no evidence that the treatment changes the weights individuals place on socio-economic versus security issues (Y and L in equation 1). Additionally, the table finds mixed evidence for changes in the extent to which individuals identify with particular groups. While there is no evidence that the treatment changes the extent to which individuals take pride in particular groups, the treatment does appear to change individuals' willingness to self-identify as belonging to specific religious and ethnic sub-groups. In general, positive price changes and exposure to Palestinian stocks appear to somewhat reduce such sub-group identification, while, for a given price change, exposure to Israeli assets (both stocks and cash) tends to raise it. Similar patterns appear on whether individuals support funding for isolated settlements.

an initiative.⁴² If, on the other hand, the asset treatment causes individuals to perceive greater risks from continuing with the status quo (i.e. the treatment leads the perceived returns under the status quo to be second order stochastically dominated relative to the control), then the treatment effect should be stronger among the more risk averse.⁴³

Table 11 estimates the effect of the asset treatment, interacted with individuals' self-assessed pre-treatment risk aversion, on voting, support for the peace process and predictions about its effects.⁴⁴ Interestingly, the effects of the asset treatment on the risk averse tend to be stronger. Risk averse individuals that were experimentally exposed to the financial markets are relatively more likely to support concessions for peace, and are more likely to predict that a peace settlement will improve Israel's security, Israel's economy, their own safety and, to some extent, their own economic situation. These differential effects on the risk averse are consistent with exposure to financial markets causing individuals to reevaluate the riskiness of continuing with status quo policies.

[Table 11]

⁴²To 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 asset 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 asset 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.

⁴³Continuing the example, suppose that absent the treatment, the payoff from the SQ is 55 and from PI 50. But now suppose the asset 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.

⁴⁴This measure of risk aversion is highly correlated with an alternative measure based upon hypothetical lotteries, and is negatively correlated with risk-related characteristics such as pre-treatment investment.

8 Conclusion

This is the first paper to measure the effects of providing incentives for individuals to trade in the stock market on their attitudes towards peace and electoral choices. We find that providing individuals with both means and incentives to trade in the stock market raises their support for peaceful compromise, changes their evaluation of the consequences of party platforms and affects their political choices. These effects are more pronounced among those who lacked prior experience with the stock market and among the risk-averse. The evidence points to three channels. First, individuals exposed to financial markets seem to reevaluate the effects of a peace agreement on the national economy. They are also more knowledgeable about the stock market and continue to follow financial markets even months after the experimental intervention. Second, Jewish Israelis exposed to Palestinian stocks appear to develop increased empathy towards Arabs. Finally, among those assigned Israeli stocks—arguably the most familiar to our subjects—having a direct financial stake on election day has a significant effect on voting for the left.

We believe the method we develop of using a combination of an on-line trading platform with parallel anonymous surveys can be broadly useful to others interested in examining the effects of financial markets. The method allows the researcher to exogenously vary factors of interest (in our case – the type of assets assigned, their value, their redemption date and the types of trade allowed) in large pre-specified populations, and directly collect data on a range of different outcomes of interest. Indeed, in companion research we also measure the impact of financial market exposure on financial literacy, willingness to invest in the stock market, and behavioral differences that arise from being exposed to specific company stocks versus more-diversified national indices.

As with any novel study, replication and extensions of this work are vital – both to investigate the generality of our results and to flesh out the mechanisms more fully. Would the effects we find generalize to other contexts? Could similar effects emerge among, e.g., Hindus and Muslims? Might the effects be different in the context of a depressed economy

and a falling stock market? With respect to mechanisms, we find limited effects of the direct skin-in-the-game incentive provided by the stock market in our intervention, except for participants endowed with Israeli stocks. However, we cannot rule out the possibility that our intervention simply provided too small a direct incentive, making further studies with increased stakes particularly desirable. As mentioned in Section 4, separating the pure-information effect from the incentives-to-trade effect would also be important. We also disallowed short sales to avoid potentially fanning the flames in an already fissile situation, but measuring the political effects of such design elements is also a topic we intend to explore.

Turning to policy, contemporary policy suggestions in areas of persistent ethnic conflict tend to focus either on diplomacy or on international peacekeeping. Our results suggest a potential role for financial instruments as well. One intriguing possibility, is that rather than focusing on providing aid to governments or even directly to populations in conflict zones, donors could examine providing individuals with resources earmarked to invest in stock in their national or regional exchanges, which can only be sold gradually over time. If our results generalize, then beyond the direct aid provided, such policies may lead recipients to internalize and take more account of the gains and risks of conflict and peacemaking to society more generally. In so doing, financial exposure may be conducive to peace.

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Table 1: Summary Statistics and Covariate Balance

	Control	Asset Treatment	Palestinian Stock Endowment	Israeli Stock Endowment	Cash Endowment	Divest After Election
N	301	1008	407	403	198	673
Voted Right '13	0.246 (0.431)	0.241 (0.428)	0.236 (0.425)	0.248 (0.432)	0.237 (0.427)	0.245 (0.431)
Voted Left '13	0.123 (0.329)	0.137 (0.344)	0.140 (0.347)	0.134 (0.341)	0.136 (0.344)	0.135 (0.342)
Bought/Sold Shares in Last 6 Mths [0/1]	0.369 (0.483)	0.355 (0.479)	0.361 (0.481)	0.337 (0.473)	0.379 (0.486)	0.351 (0.478)
Willing to Take Risks [1-10]	4.355 (2.235)	4.718** (2.265)	4.744** (2.317)	4.715** (2.183)	4.672 (2.328)	4.782*** (2.287)
Time preference median or above	0.641 (0.480)	0.658 (0.475)	0.673 (0.470)	0.630 (0.483)	0.682 (0.467)	0.645 (0.479)
Financial Literacy Test: Correct Answers [0-7]	4.874 (1.673)	4.945 (1.636)	4.929 (1.665)	4.978 (1.583)	4.914 (1.688)	4.909 (1.654)
Male	0.515 (0.501)	0.522 (0.500)	0.536 (0.499)	0.496 (0.501)	0.545 (0.499)	0.526 (0.500)
Age [Yrs]	41.52 (14.32)	39.307** (13.389)	39.494* (13.422)	39.943 (13.610)	37.626*** (12.783)	39.334** (13.365)
Married	0.631 (0.483)	0.597 (0.491)	0.572 (0.495)	0.620 (0.486)	0.601 (0.491)	0.585 (0.493)
Education [Yrs]	14.17 (2.209)	14.185 (1.978)	14.181 (2.006)	14.230 (1.929)	14.104 (2.024)	14.221 (2.040)
Religiosity: Secular	0.635 (0.482)	0.628 (0.484)	0.619 (0.486)	0.633 (0.483)	0.636 (0.482)	0.611 (0.488)
Traditional	0.173 (0.379)	0.164 (0.370)	0.174 (0.380)	0.161 (0.368)	0.146 (0.354)	0.178 (0.383)
Religious	0.120 (0.325)	0.124 (0.330)	0.128 (0.334)	0.114 (0.318)	0.136 (0.344)	0.132 (0.339)
Ultra-Orthodox	0.0731 (0.261)	0.084 (0.278)	0.079 (0.269)	0.092 (0.289)	0.081 (0.273)	0.079 (0.270)
Region: Jerusalem	0.0963 (0.296)	0.090 (0.287)	0.088 (0.284)	0.089 (0.286)	0.096 (0.295)	0.098 (0.298)
North	0.0897 (0.286)	0.097 (0.296)	0.086 (0.281)	0.092 (0.289)	0.131 (0.339)	0.094 (0.292)
Haifa	0.123 (0.329)	0.142 (0.349)	0.140 (0.347)	0.139 (0.346)	0.152 (0.359)	0.144 (0.351)
Center	0.296 (0.457)	0.291 (0.454)	0.290 (0.454)	0.305 (0.461)	0.263 (0.441)	0.290 (0.454)
Tel Aviv	0.213 (0.410)	0.194 (0.396)	0.206 (0.405)	0.179 (0.384)	0.202 (0.403)	0.198 (0.399)
South	0.116 (0.321)	0.104 (0.306)	0.120 (0.326)	0.104 (0.306)	0.071* (0.257)	0.101 (0.302)
West Bank	0.0664 (0.249)	0.081 (0.274)	0.069 (0.253)	0.092 (0.289)	0.086 (0.281)	0.076 (0.265)
Monthly Family Income [NIS]+	11,172 (5330)	11,005 (5564)	10,700 (5443)	11,104 (5556)	11,435 (5818)	10,908 (5486)
Peace Deal Support Scale [4-16]	8.695 (3.306)	8.828 (3.455)	8.898 (3.453)	8.763 (3.518)	8.822 (3.343)	8.823 (3.482)
Two states for two peoples [0/1]	0.634 (0.483)	0.593 (0.492)	0.611 (0.488)	0.582 (0.494)	0.579 (0.495)	0.581 (0.494)
1967 borders with a possibility of land exchanges [0/1]	0.425 (0.495)	0.396 (0.489)	0.384 (0.487)	0.406 (0.492)	0.401 (0.491)	0.395 (0.489)
Jerusalem will be split into two separate cities - Arab and Jewish [0/1]	0.229 (0.421)	0.273 (0.446)	0.286* (0.453)	0.267 (0.443)	0.259 (0.439)	0.277 (0.448)
Palestinian refugees will get appropriate compensation & allowed to return to Palestine only [0/1]	0.394 (0.489)	0.443 (0.497)	0.448 (0.498)	0.423 (0.495)	0.472* (0.500)	0.441 (0.497)

This table provides the mean (and SD) of pre-treatment variables for the 1309 individuals with observed 2015 vote. The data on peace deal support questions are for the 1277 with observed post-treatment support. The Peace Deal Support Scale is a sum of the responses to the 4 peace concessions shown below it, with responses coded 1-disagree, 2-tend to disagree, 3-tend to agree, 4- agree. The four peace questions show proportions that "tend to agree" or "agree". +: midpoint of SES income categories available for 1284 individuals. Significance levels of difference from the control group: * 10%, **:5%, ***1%.

Table 2: Exposure to Financial Assets and Voting for the Left in 2015

Sample	All (1)	All (2)	All (3)	All (4)	Experienced (5)	Inexperienced (6)
Asset Treatment	0.064** (0.029)	0.056** (0.024)	0.054** (0.023)	0.060** (0.023)	0.008 (0.045)	0.083*** (0.029)
Voted Right '13		-0.252*** (0.018)	-0.196* (0.105)	-0.254*** (0.091)	-0.539* (0.313)	-0.076 (0.105)
Voted Left '13		0.559*** (0.032)	0.695*** (0.094)	0.596*** (0.091)	0.533** (0.243)	0.659*** (0.100)
Bought/Sold Shares in Last 6 Mths [0/1]		0.099*** (0.023)	0.031 (0.041)	0.018 (0.040)		
Traditional				-0.138*** (0.032)	-0.149** (0.059)	-0.112*** (0.039)
Religious				-0.166*** (0.032)	-0.191*** (0.059)	-0.132*** (0.040)
Ultra-Orthodox				-0.221*** (0.039)	-0.227*** (0.088)	-0.191*** (0.046)
Post Secondary Education				0.067** (0.033)	0.039 (0.081)	0.085** (0.037)
BA Student				0.088** (0.038)	-0.002 (0.086)	0.122*** (0.043)
BA Graduate and Above				0.062** (0.030)	-0.013 (0.068)	0.091** (0.036)
Willing to Take Risks [1-10]				-0.000 (0.005)	-0.006 (0.010)	0.002 (0.006)
Time preference above median				0.011 (0.022)	0.052 (0.041)	-0.002 (0.026)
Financial Literacy Test: Correct Answers [0-7]				0.004 (0.007)	-0.020 (0.016)	0.015* (0.008)
Strata Fixed Effects	No	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	No	Yes	Yes	Yes
Observations	1,309	1,309	1,309	1,309	469	840
R-squared	0.003	0.284	0.422	0.446	0.446	0.492

Notes: This table reports OLS estimates of the effect of exposure to the asset treatment on the probability that an individual voted for a left party in 2015. Robust standard errors in parentheses ***1%, **5%, *10%. Column 3 includes 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 risk aversion. Column 4 adds controls for demographics and pre-treatment preferences, that 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. Columns 5 and 6 show the effects on the subsample of those that had traded 6 months before the experiment (Column 5) and those that had not (Column 6).

Table 3: Exposure to Financial Assets and Voting for the Right in 2015

Sample	All (1)	All (2)	All (3)	All (4)	Experienced (5)	Inexperienced (6)
Asset Treatment	-0.046 (0.031)	-0.042* (0.025)	-0.039* (0.024)	-0.044* (0.024)	-0.040 (0.041)	-0.042 (0.031)
Voted Right '13		0.657*** (0.025)	0.525*** (0.113)	0.492*** (0.122)	0.519 (0.357)	0.215 (0.174)
Voted Left '13		-0.133*** (0.022)	-0.330*** (0.096)	-0.222** (0.088)	-0.101 (0.198)	-0.335*** (0.123)
Bought/Sold Shares in Last 6 Mths [0/1]		-0.045** (0.021)	0.012 (0.040)	0.030 (0.040)		
Traditional				0.102*** (0.032)	0.149*** (0.055)	0.085** (0.042)
Religious				0.241*** (0.049)	0.354*** (0.089)	0.174*** (0.061)
Ultra-Orthodox				0.056 (0.086)	0.110 (0.201)	-0.020 (0.101)
Post Secondary Education				-0.060* (0.034)	-0.114 (0.072)	-0.039 (0.040)
BA Student				-0.041 (0.039)	-0.120 (0.079)	-0.009 (0.046)
BA Graduate and Above				-0.044 (0.032)	-0.134** (0.062)	0.002 (0.038)
Willing to Take Risks [1-10]				0.007 (0.005)	0.025*** (0.009)	-0.002 (0.006)
Time preference above median				0.004 (0.021)	-0.018 (0.040)	0.023 (0.026)
Financial Literacy Test: Correct Answers [0-7]				-0.013* (0.007)	-0.012 (0.014)	-0.014 (0.009)
Strata Fixed Effects	No	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	No	Yes	Yes	Yes
Observations	1,309	1,309	1,309	1,309	469	840
R-squared	0.002	0.401	0.487	0.518	0.516	0.550

Notes : This table reports OLS estimates of the effect of exposure to the asset treatment on the probability that an individual voted for a right party in 2015. Robust standard errors in parentheses ***1%, **5%, *10%. Column 3 includes 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 risk aversion. Column 4 adds controls for demographics and pre-treatment preferences, that 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. Columns 5 and 6 show the effects on the subsample of those that had traded 6 months before the experiment (Column 5) and those that had not (Column 6).

Table 4: Difference in Difference Estimates of Votes between 2013 and 2015

Sample	Voted for Left				Voted for Right			
	All (1)	All (2)	Experienced (3)	Inexperienced (4)	All (5)	All (6)	Experienced (7)	Inexperienced (8)
Indicator for 2015	0.123*** (0.023)	0.123*** (0.023)	0.225*** (0.040)	0.063*** (0.026)	0.113*** (0.024)	0.113*** (0.024)	0.090*** (0.041)	0.126*** (0.028)
Asset Treatment	0.014 (0.022)				-0.005 (0.028)			
Asset Treatment x 2015	0.050* (0.026)	0.050* (0.026)	0.015 (0.047)	0.072** (0.031)	-0.042 (0.026)	-0.042 (0.026)	-0.043 (0.046)	-0.042 (0.032)
Individual FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Number of Individuals	1,309	1,309	469	840	1,309	1,309	469	840
Observations	2,618	2,618	938	1,680	2,618	2,618	938	1,680
R-squared	0.041	0.129	0.217	0.086	0.009	0.043	0.022	0.060

Notes: This table provides Difference-in-Difference estimates of the effect of exposure to the asset treatment on the probability that an individual voted for a left party in 2015 (Columns 1-4) or a right party (Columns 5-8) in 2015. Regressions are estimated by OLS. Robust standard errors, clustered at the individual level, are in parentheses ***1%, **5%, *10%. 2015. Columns 1 and 4 provide an intercept for 2015, the (placebo) asset treatment effect in 2013 and the asset treatment effect in 2015. Columns 2 and 6 control for individual fixed effects. Columns 3-4 and 7-8 compare the effects in the subsamples of those that had and had not traded assets within 6 months before the experiment.

Table 5: Financial Assets Exposure and Support for Peace Concessions

Estimator	Ordered Probit			OLS: 1 if Agree / Tend to Agree	OLS: 1 if Disagree
	All	Experienced	Inexperienced	Inexperienced	Inexperienced
Sample	(1)	(2)	(3)	(4)	(5)
A. Two states for two peoples (Mean = 2.522, SD = 1.14)					
Asset Treatment	0.101 (0.079)	-0.082 (0.142)	0.230** (0.102)	0.070* (0.036)	-0.075** (0.034)
R ² / Pseudo R ²	0.231	0.236	0.265	0.428	0.411
B. The 1967 Borders will be the borders between the two countries with a possibility of land swaps (Mean = 2.164, SD = 1.083)					
Asset Treatment	0.164** (0.079)	0.056 (0.141)	0.278*** (0.102)	0.029 (0.037)	-0.087** (0.036)
R ² / Pseudo R ²	0.213	0.221	0.238	0.385	0.412
C. Jerusalem will be divided into two separate cities: Arab and Jewish (Mean = 1.822, SD = 1.039)					
Asset Treatment	0.189** (0.086)	0.226 (0.148)	0.213* (0.110)	0.036 (0.033)	-0.057 (0.037)
R ² / Pseudo R ²	0.206	0.207	0.238	0.338	0.396
D. Palestinian refugees will receive adequate compensation and be allowed to return to the State of Palestine only (Mean=2.135, SD=1.075)					
Asset Treatment	0.194** (0.077)	0.122 (0.139)	0.262*** (0.099)	0.063 (0.041)	-0.110*** (0.042)
R ² / Pseudo R ²	0.0787	0.118	0.0840	0.160	0.193
Observations	1,277	458	819	819	819

This table shows the effect of financial asset treatment on an individual's support for potential concessions for peace. Columns 1-3 show the coefficient on asset treatment from Ordered Probit regressions of whether individuals *disagree* (1), *tend to disagree* (2), *tend to agree* (3) and *agree* (4) with each potential peace concession. Columns 3-5 include only individuals who had not traded 6 months prior to the experiment. For interpretability Column 4 shows OLS effects on the probability of either *tend to agree* or *agree*. Column 5 estimates the OLS effect on the probability of choosing the lowest degree of support (*disagree*). All regressions control for the full set of demographics and randomization strata from Table 2, Col. 4. Robust standard errors in parentheses. ***1%, **5%, *10%.

Table 6: Treatment Effect on the Treated and Reweighted Estimates

Estimator Sample	OLS	IV (Treatment Effect On the Treated)			OLS
	All	All	Experienced	Inexperienced	Reweighted
	(1)	(2)	(3)	(4)	(5)
A. Left Vote 2015					
Asset Treatment	0.060** (0.023)	0.074** (0.029)	0.010 (0.053)	0.106*** (0.037)	0.044 (0.026)
Observations	1,309	1,309	469	840	1,309
R-squared	0.446	0.442	0.447	0.480	0.569
B. Right Vote 2015					
Asset Treatment	-0.044* (0.024)	-0.054* (0.029)	-0.047 (0.048)	-0.053 (0.039)	-0.051** (0.023)
Observations	1,309	1,309	469	840	1,309
R-squared	0.518	0.517	0.518	0.548	0.555
C. Peace Deal Support Scale					
Asset Treatment	0.467** (0.190)	0.573** (0.233)	0.274 (0.422)	0.842*** (0.288)	0.505** (0.198)
Observations	1,277	1,277	458	819	1,277
R-squared	0.457	0.457	0.472	0.478	0.503

This table compares the Intent To Treat estimates of the effect of asset exposure (Column 1) to estimates of the Treatment Effect on the Treated (Columns 2-4). This estimate uses assignment to treatment as an instrument for actually being treated with asset exposure. Right Vote and Left Vote are indicators for vote in the 2015 elections. The Peace Deal Support Scale is the sum of the answers to the four peace deal questions and takes values from 4 to 16. Column 5 reweighs the data by the actual vote share for each Jewish party in 2013. All regressions control for the full set of demographics and randomization strata. Robust standard errors in parentheses in columns 1-4. Standard errors are clustered by party vote in 2013 in Column 5. Significant at ***1%, **5%, *10%.

Table 7: Effects by Sub-Treatment

	OLS			IV-TOT
	No Controls	Full Controls	Full Controls	Full Controls
	(1)	(2)	(3)	(4)
A: Vote for the Left				
Palestinian Stock Treatment	0.088** (0.044)	0.072** (0.035)	0.122** (0.052)	0.155** (0.061)
Israeli Stock Treatment	0.109** (0.044)	0.103*** (0.035)	0.033 (0.062)	0.036 (0.071)
Cash Treatment	0.130** (0.050)	0.112*** (0.038)	-0.013 (0.100)	-0.027 (0.114)
High Allocation	-0.016 (0.029)	-0.012 (0.023)	-0.012 (0.023)	-0.019 (0.027)
Divest After Election	-0.032 (0.031)	-0.019 (0.025)	-0.020 (0.025)	-0.023 (0.029)
Stock Index	-0.038 (0.034)	-0.044 (0.027)	-0.082** (0.039)	-0.102** (0.046)
Cash - non TA25	-0.348*** (0.036)	-0.350** (0.174)	-0.194 (0.214)	-0.170 (0.216)
% Price Change of Exposed Asset by Election Day			0.018 (0.013)	0.023 (0.015)
Constant	0.246*** (0.025)	0.261 (0.192)	0.264 (0.191)	0.309* (0.181)
Joint F/chi2 (treatments)	53.56	2.385	2.353	20.81
Prob > F/chi2	0.000	0.020	0.016	0.008
R-squared	0.008	0.451	0.452	0.446
B: Vote for the Right				
Palestinian Stock Treatment	-0.056 (0.045)	-0.042 (0.034)	-0.105** (0.050)	-0.129** (0.058)
Israeli Stock Treatment	-0.075* (0.045)	-0.064* (0.033)	0.025 (0.056)	0.032 (0.064)
Cash Treatment	-0.100** (0.050)	-0.085** (0.036)	0.075 (0.092)	0.097 (0.105)
High Allocation	-0.042 (0.029)	-0.044** (0.021)	-0.044** (0.021)	-0.050** (0.025)
Divest After Election	0.076** (0.030)	0.058*** (0.022)	0.060*** (0.023)	0.070*** (0.026)
Stock Index	-0.006 (0.035)	0.000 (0.026)	0.048 (0.038)	0.058 (0.046)
Cash - non TA25	-0.305*** (0.037)	-0.243*** (0.092)	-0.443*** (0.147)	-0.481*** (0.161)
% Price Change of Exposed Asset by Election Day			-0.023* (0.013)	-0.028* (0.014)
Constant	0.359*** (0.028)	0.410** (0.200)	0.406** (0.201)	0.376** (0.189)
Joint F/chi2 (treatments)	36.63	3.590	3.311	28.37
Prob > F/chi2	0.000	0.001	0.001	0.000
R-squared	0.010	0.524	0.525	0.523
Observations	1,309	1,309	1,309	1,309

This table reports the partial effects of the sub-treatments on the probability of voting choices in 2015.

Regressions in Columns 2-4 include the full set of controls from Table 2, Col. 4. *Cash - non TA25* is an indicator for the 4 individuals included in the cash treatment who traded for indices other than TA25.

Robust standard errors in parentheses. ***1%, **5%, *10%.

Table 8: **Skin-in-the-game**

	Voted for Left				Voted for Right			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
N = 1309 individuals								
Stock Treatment	0.054** (0.024)				-0.037 (0.024)			
Stock Treat. x Divest Pre-Elections		0.056* (0.031)				-0.071** (0.029)		
Stock Treat. x Divest Post-Elections		0.052** (0.026)				-0.019 (0.026)		
Palestinian Stock x Divest Pre-Elections			0.087** (0.037)	0.079* (0.042)			-0.084** (0.037)	-0.105*** (0.040)
Palestinian Stock x Divest Post-Elections			0.014 (0.030)	0.007 (0.036)			0.005 (0.031)	-0.016 (0.035)
Israeli Stock x Divest Pre-Elections			0.026 (0.039)	0.043 (0.058)			-0.057 (0.035)	-0.011 (0.049)
Israeli Stock x Divest Post-Elections			0.090*** (0.030)	0.109** (0.055)			-0.043 (0.030)	0.006 (0.053)
Cash Treatment	0.092*** (0.032)				-0.068** (0.032)			
Cash Treat. x Divest Pre-Elections		0.157*** (0.049)	0.158*** (0.049)	0.184** (0.082)		-0.126*** (0.043)	-0.126*** (0.043)	-0.056 (0.073)
Cash Treat. x Divest Post-Elections		0.062* (0.037)	0.062* (0.037)	0.089 (0.078)		-0.041 (0.038)	-0.040 (0.038)	0.033 (0.073)
% Price Change of Exposed Asset by Election Day				-0.004 (0.010)				-0.010 (0.009)
R-squared	0.449	0.450	0.453	0.453	0.519	0.522	0.523	0.524

This table provides OLS estimates on the probability that an individual voted for the left in 2015 (Columns 1-4) and the right (Columns 5-8). All regressions control for the full set of demographics and randomization strata from Table 2, Column 4, and include a dummy for the 4 individuals assigned to non-TA 25 cash endowments. Robust standard errors in parentheses. Significant at ***1%, **5%, *10%.

Table 9: Empathy

	(1)	(2)	(3)	(4)	(5)	(6)
	Social Scale	Arabs attend Jewish High Schools	Arabs live in Jewish Neighborhoods	Business Scale	Arabs and Jews form Joint Bus.	Arabs manage Jewish Cos
	[1-8]	[0/1]	[0/1]	[1-8]	[0/1]	[0/1]
Palestinian Stock Treatment	0.535** (0.240)	0.152** (0.061)	0.178*** (0.063)	0.378 (0.245)	0.024 (0.060)	0.031 (0.065)
Israeli Stock Treatment	-0.335 (0.297)	-0.084 (0.075)	-0.109 (0.079)	-0.223 (0.298)	-0.086 (0.073)	-0.024 (0.079)
Cash Treatment	-0.900* (0.481)	-0.238* (0.122)	-0.269** (0.126)	-0.238 (0.477)	-0.080 (0.119)	0.029 (0.128)
High Allocation	0.121 (0.109)	0.019 (0.028)	0.011 (0.029)	0.100 (0.111)	0.046* (0.026)	0.022 (0.029)
Divest After Election	-0.271** (0.115)	-0.073** (0.029)	-0.054* (0.030)	-0.210* (0.119)	-0.032 (0.028)	-0.019 (0.031)
Stock Index	-0.507*** (0.186)	-0.147*** (0.047)	-0.108** (0.049)	-0.345* (0.192)	-0.052 (0.047)	-0.091* (0.049)
Cash - non TA25	-0.014 (0.846)	-0.128 (0.232)	0.038 (0.209)	-0.712 (0.637)	-0.342 (0.230)	-0.346* (0.199)
% Price Change of Exposed Asset by Election	0.166*** (0.064)	0.042*** (0.016)	0.046*** (0.017)	0.083 (0.064)	0.013 (0.016)	0.006 (0.017)
Joint F (treatments)	2.838	3.598	2.488	3.487	2.081	2.434
Prob > F	0.004	0.000	0.011	0.001	0.035	0.013
R-squared	0.402	0.342	0.272	0.361	0.328	0.288
Observations	1,279	1,279	1,279	1,279	1,279	1,279

This table reports the OLS partial effects of the sub-treatments on an individuals' attitudes. The dependent variable in Column 1 is a Social Integration Scale (ranging from 1-8, mean 4.42, SD: 2.05, summing over individuals' responses (on a scale of 1: disagree; 2: tend to disagree; 3: tend to agree; 4: agree) to two statements: "Arabs will live in Jewish neighbourhoods" and "Arabs will attend Jewish high schools". Columns 2-3 provide the probability of agreeing or tending to agree on those components. The dependent variable in Column 4 is a Business Integration Scale (ranging from 1-8, mean 5.32, SD: 2.02), summing over individuals' responses to two statements: "Arabs will manage Jewish firms" and "Arabs and Jews will form joint businesses", with the associated probability of agreeing or tending to agree to those components in Columns 5-6. All regressions include the full set of controls from Table 2, Col. 4. Robust standard errors in parentheses. ***1%, **5%, *10%.

Table 10: Predicted Consequences of Peace, Knowledge and Well-Being

Sample	All				Inexperienced	
	Mean	SD	Asset Treat.	SE	Asset Treat.	SE
A. Consequences of a Two-State Agreement (Ordered Probits) [Mar 19]						
Suppose Israel reaches a permanent agreement with the Palestinians on the principle of two states for two peoples. How do you think this will affect... [1 (worsen a lot), 2 (worsen somewhat), 3 (no change), 4 (improve somewhat), 5(improve a lot)]						
Israel's security?	2.956	(1.392)	-0.010	(0.076)	0.097	(0.097)
Israel's economic situation?	3.294	(1.329)	0.126*	(0.073)	0.223**	(0.094)
your own personal security?	2.888	(1.237)	-0.002	(0.075)	0.059	(0.094)
your own economic situation?	3.048	(1.047)	-0.013	(0.077)	0.005	(0.101)
Observations			1281 / 1282		823	
B. Economic and Political Knowledge (OLS) [Apr 17]						
Economic Knowledge Score [Prop Correct out of 5]	0.533	(0.276)	0.017	(0.016)	0.020	(0.021)
Stock mkt performance answer within 3pp of actual	0.393	(0.489)	0.066**	(0.033)	0.091**	(0.042)
Political Knowledge Score [Prop Correct out of 13]	0.694	(0.212)	0.002	(0.013)	-0.010	(0.018)
Observations			1,238		782	
C. Media Consumption (OLS) [Jul 19]						
Which of the following newspapers/websites do you usually read?						
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)
Number of financial outlets [0-3]	1.117	(1.120)	0.203***	(0.074)	0.195**	(0.093)
Observations			1,120		705	
D. Subjective Well Being (Ordered Probits) [Mar 19]						
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	

The table reports the coefficient of Asset treatment from a separate regression with the dependent variable mentioned in the first column. All regressions control for the full set of demographics and randomization strata. Robust standard errors in parentheses. On March 19, 2015, we asked individuals to predict the effects of a two state solution at two levels--personal and national--and on two dimensions: security and the economy (Panel A). On April 17, we asked individuals 13 political knowledge questions, of which 2 were questions on salient events in the run-up to elections, 6 were questions on the positions taken prior to the elections by the two leading candidates for the right and left-- Netanyahu and Herzog, and 5 were on political facts. Economic knowledge questions asked individuals to provide estimates on the unemployment rate, inflation rate, whether the stock market rose and fell and its change in value, and the change in housing prices. All answers were scored correct if they were within 3pp of the correct answer (Panel B). On July 19, we asked individuals which newspapers they usually read from among the following: *Globes*, *The Marker*, *Haaretz*, *Vesti*, *Yediot Ahronoth*, *Israel Hayom*, *Kalkalist* and *Maariv*. Of these, *Globes*, *Marker* and *Kalkalist* are financial outlets. (Panel C). On March 19, we also asked individuals about their Subjective Well Being (Panel D). These included the top ten aspects that predict personal wellbeing from Benjamin et al. (2014, Table 2), excluding mental health. ***1%, **5%, *10%.

Table 11: Treatment Effects on the Risk Averse

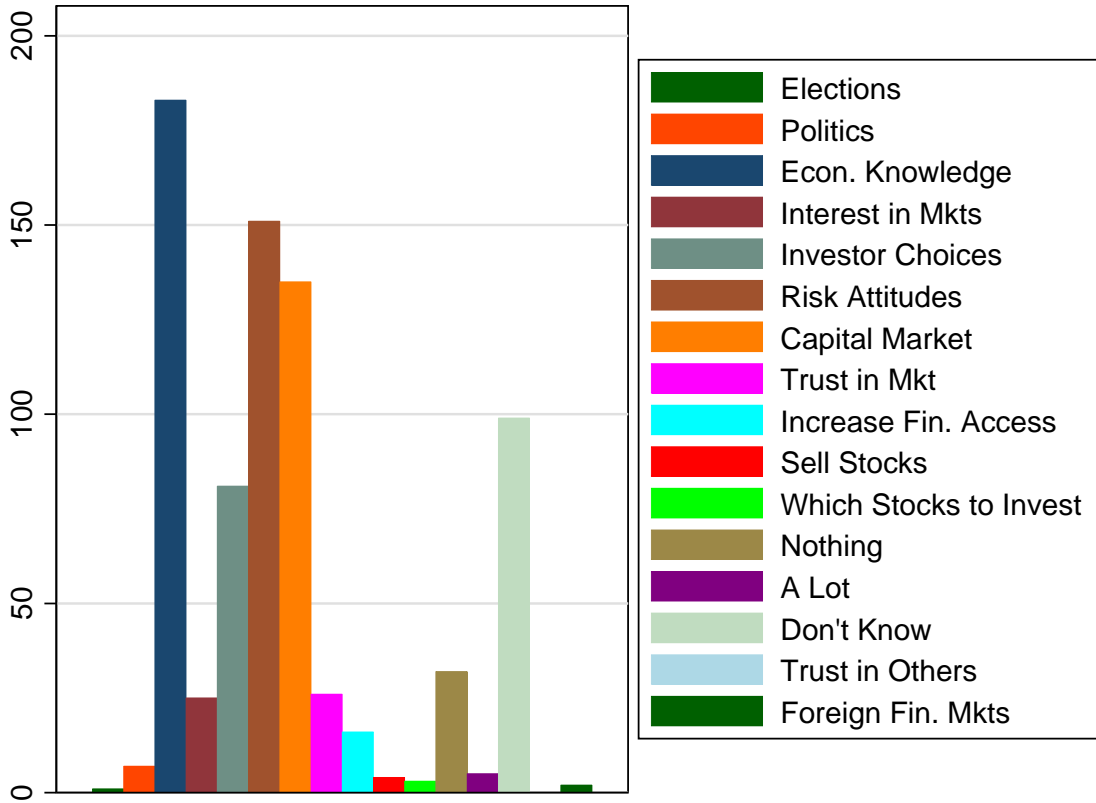
A. Main Outcomes (OLS)	Voted Left (1)	Voted Right (2)	Peace Deals Index (3)	
Asset Treatment	0.023 (0.038)	-0.009 (0.042)	-0.345 (0.320)	
Risk Averse	-0.024 (0.045)	0.030 (0.047)	-0.754** (0.367)	
Asset Treatment * Risk Averse	0.060 (0.050)	-0.051 (0.052)	1.250*** (0.406)	
Joint F (Asset Treatment Vars)	3.819	2.175	7.700	
Prob>p	0.0222	0.114	0.000477	
Observations	1,309	1,309	1,277	
R-squared	0.447	0.518	0.460	

B. Support for Peace Deals (Ordered Probit)	Two State Soln (4)	1967 Borders (5)	Split Jerusalem (6)	Refugees (7)
Asset Treatment	-0.242* (0.134)	-0.129 (0.135)	-0.054 (0.137)	-0.076 (0.129)
Risk Averse	-0.343** (0.153)	-0.370** (0.155)	-0.253 (0.162)	-0.149 (0.149)
Asset Treatment * Risk Averse	0.541*** (0.171)	0.462*** (0.168)	0.383** (0.179)	0.417** (0.166)
Joint chi2 (Asset Treatment Vars)	11.68	12.40	8.860	12.32
Prob>p	0.003	0.002	0.012	0.002
Observations	1,277	1,277	1,277	1,277
Pseudo R-squared	0.234	0.215	0.207	0.080

C. Consequences of Two-State Agreement for: (Ordered Probit)	Israel's Security (8)	Israel's Economy (9)	Own Security (10)	Own Economic (11)
Asset Treatment	-0.206 (0.131)	-0.059 (0.125)	-0.180 (0.127)	-0.183 (0.131)
Risk Averse	-0.203 (0.150)	-0.211 (0.139)	-0.156 (0.145)	-0.200 (0.148)
Asset Treatment * Risk Averse	0.310* (0.164)	0.291* (0.158)	0.278* (0.161)	0.269 (0.165)
Joint chi2 (Asset Treatment Vars)	3.568	6.485	3.003	2.670
Prob>p	0.168	0.039	0.223	0.263
Observations	1,282	1,282	1,281	1,281
Pseudo R-squared	0.149	0.145	0.139	0.132

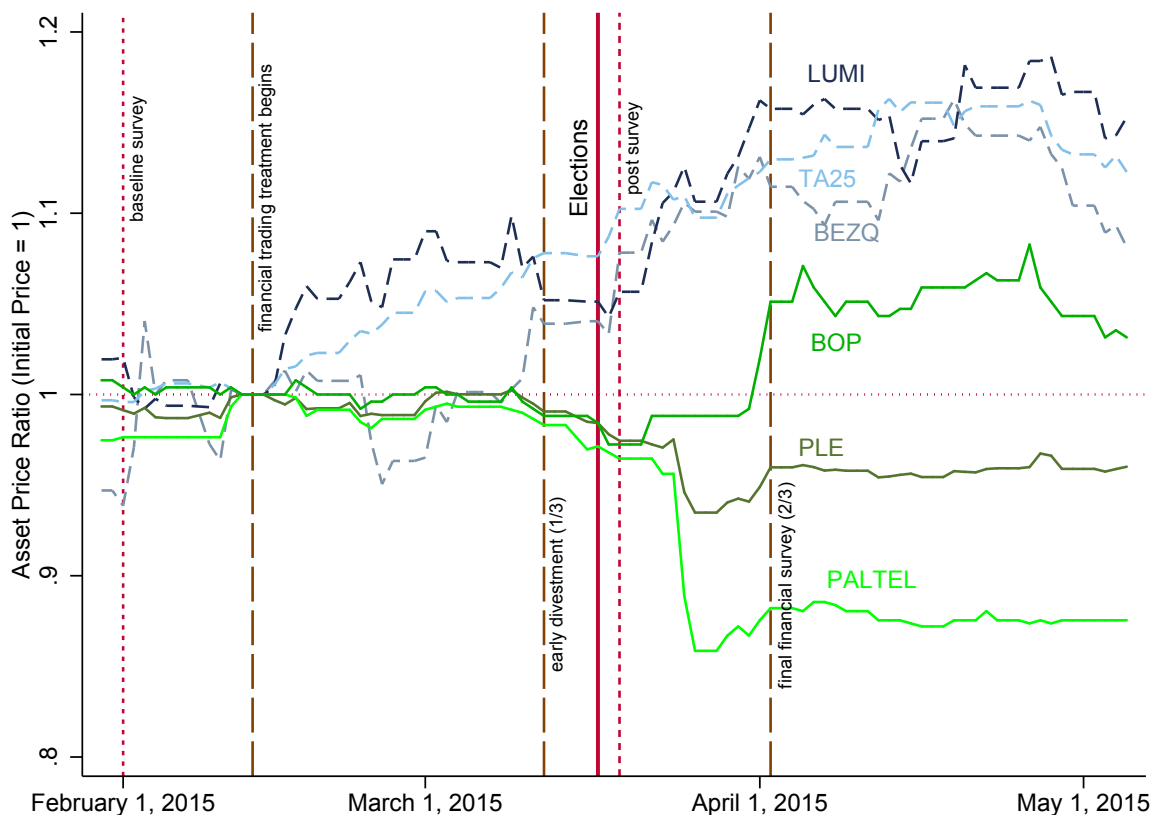
This table shows the differential effects of asset treatment on risk averse individuals, defined as those with ex ante subjective risk aversion at the median or below. All regressions control for the full set of demographics and randomization strata from Table 2 Column 4, except that we replace the willingness to take risk measure with a dummy for being risk averse. Panel A reports the OLS effects on the main outcomes: the vote choice and the peace deal index (on a scale of 4-16). Panel B reports Ordered Probit coefficients for each component separately (on a 1 (disagree) 2 (tend to disagree) 3 (tend to agree) 4 (agree) scale). These include: *Two states for two peoples*, *The 1967 Borders will be the borders between the two countries with a possibility of land swaps*, *Jerusalem will be divided into two separate cities: Arab and Jewish*, and *Palestinian refugees will receive adequate compensation and be allowed to return to the State of Palestine only*. Panel C reports Ordered Probit coefficients for responses to the question: *Suppose Israel reaches a permanent agreement with the Palestinians on the principle of two states for two peoples. How do you think this will affect... [1 (worsen a lot), 2 (worsen), 3 (no change), 4 (improve), 5(improve a lot)]*. Robust standard errors in parentheses. 1%***,5%**,10%.

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



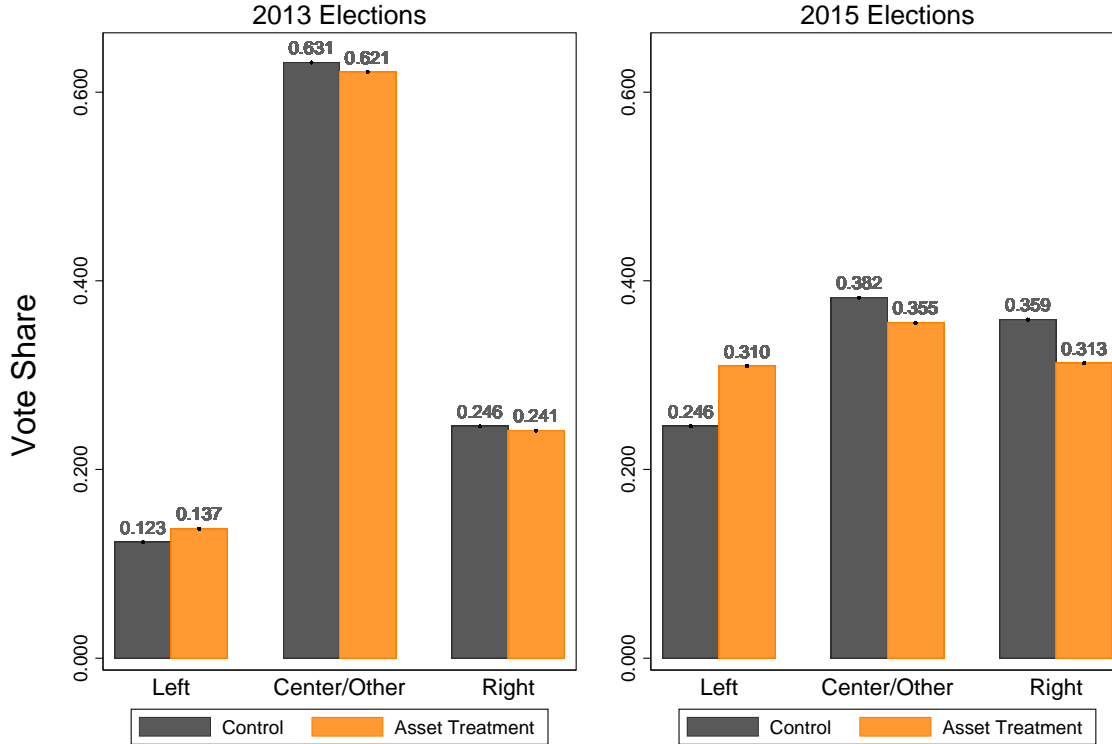
These are the results of an open-response question at the end of the trading period (March 12 or April 2) to the question “What do you think the researchers can learn from the study?”. Respondents only include the 840 participants who actually received treatment. Notice that, despite the study being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

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



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

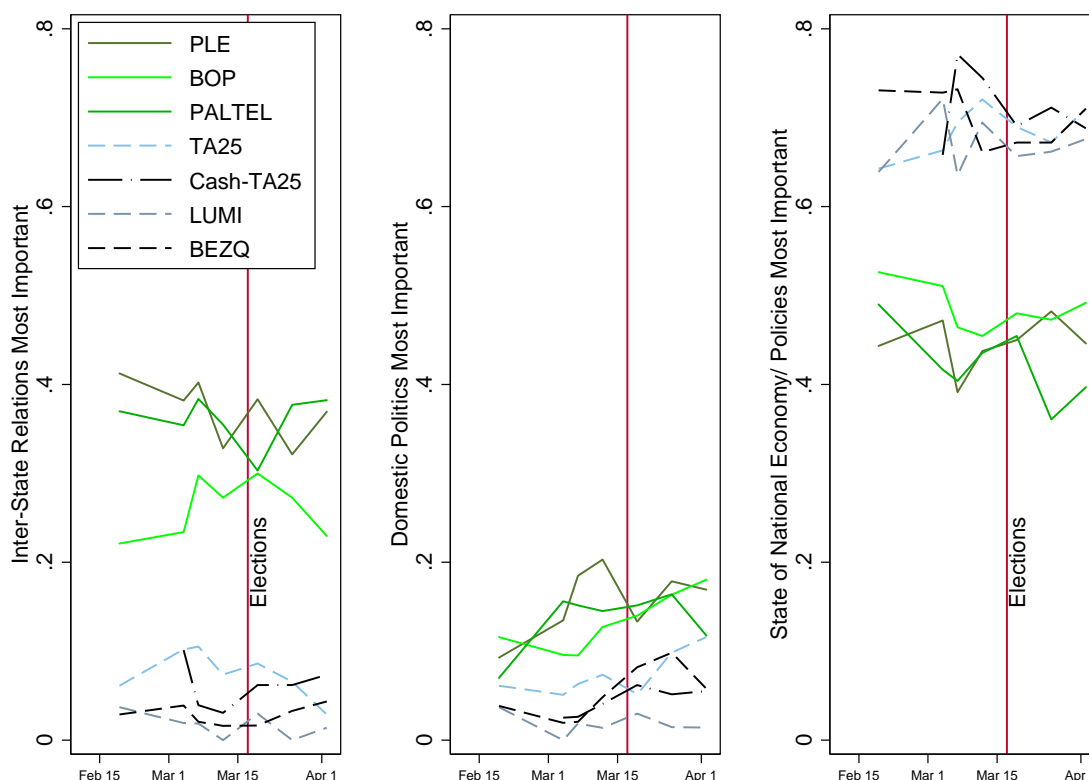
Figure 3: Vote in Treatment and Control Groups in 2013 and 2015



N=1309. 'Other' includes 59 individuals in 2013 and 17 in 2015. 27 individuals did not vote in 2015.

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

Figure 4: What was the major determinant of your asset's value?



These graphs show how the 840 participants who received an asset treatment answered a weekly question *Various factors affect the success of a particular company. When you think about the performance of [assigned stock or index], which of the following is most important?* Panel A shows the proportion choosing *regional political conditions: normal relations with the neighbors and the lack of conflicts and wars..* Panel B shows the proportion choosing *Internal political conditions: the quality of the government and lack of corruption.* Panel C shows the proportion choosing *Conditions in the economy: price stability, the level of the national debt, the quality of regulation.* Notice that holders of Palestinian stocks are relatively more likely to view both inter-state relations, and domestic politics as the most important determinants of the assets' value, and holders of the indices tend to do so more than holders of individual company stock. Further, large proportions in both groups also tend to view the state of the national economy and the policies that affect it as the most important determinant of their assets' value. Relatively few chose the other options, which included: the quality of management of the firm(s) and the quality of workers and employee conditions.