VALUING PEACE: THE EFFECTS OF FINANCIAL MARKET EXPOSURE ON VOTES AND POLITICAL ATTITUDES

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Can participation in financial markets lead individuals to reevaluate the costs of conflict, change their political attitudes, and even their votes? Prior to the 2015 Israeli elections, we randomly assigned Palestinian and Israeli financial assets to likely voters and incentivized them to actively trade for up to 7 weeks. No political messages or nonfinancial information were included. The treatment systematically shifted vote choices toward parties more supportive of the peace process. This effect is not due to a direct material incentive to vote a particular way. Rather, the treatment reduces opposition to concessions for peace and changes awareness of the broader economic risks of conflict. While participants who were assigned Palestinian assets are more likely to associate their assets' performance with peace, they are less engaged in the experiment. Combined with the superior performance of Israeli stocks during the study period, the ultimate effects of Israeli and Palestinian assets are similar.

KEYWORDS: Conflict, financial markets, field experiment, peace, voting.

1. INTRODUCTION

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

The basic idea is straightforward: compared to commonplace daily transactions, financial markets expose individuals to the broader economy and, from a broader economic perspective, conflicts tend to be very costly (e.g., Blattman and Miguel (2010), World Bank (2011)). Indeed, the hypothesis that market exposure affects attitudes toward conflict is very old, dating back at least to Montesquieu (1748): "Commerce is a cure for the most destructive prejudices; it is almost a general rule that wherever the ways of man are gentle, there is commerce; and wherever there is commerce, there the ways of men are gentle." Theoretically, financial markets may change political attitudes as they can demonstrate

the shared risks from conflict and the returns from peace. Empirically, however, measuring the causal effect of financial markets is very difficult, as individuals' investment opportunities and decisions are associated with numerous factors that could potentially affect political choices. This paper presents results from the first study to experimentally assign individuals financial assets, allow them to trade in those assets, and trace the effects on their political views and behavior.

Our setting is the Israeli–Palestinian conflict, a geopolitically important and highly persistent conflict. Opposing interests and ethnic animosities—reinforced by more than 80 years of recurrent violence—have led many to consider the conflict intractable. Yet the potential *economic* gains from peace are large.[1] A month and a half prior to the highly contested 2015 Israeli elections, we randomly assigned 1345 Jewish Israeli voters to either a financial asset treatment or a control group. Individuals in the treatment group received either vouchers that could be used to invest in specific stocks or endowments of assets that tracked the value of specific indices or company stocks from both Israel and the Palestinian Authority. Participants were given incentives to learn about the performance of their asset and to make weekly decisions to buy or sell part of their portfolio. The initial value of the portfolio was either $50 or $100.

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

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

Section 6 examines the underlying mechanisms. The analysis here is more exploratory in nature, as we move away from a one-treatment–one-outcome framework to multiple (potentially underpowered) subtreatments and multiple outcomes, some of which are self-reported attitudes rather than chosen behavior. Nonetheless, we believe the results

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[1]The Rand Corporation estimates that a two-state solution, which it regards as the most likely to succeed, will yield Israelis an economic dividend of $123 billion over 10 years and Palestinians $50 billion (Anthony, Egel, Ries et al. (2015)). In contrast, a return to widespread conflict would lower Israeli gross domestic product (GDP) by $250 billion and Palestinian by $46 billion over the same period. See also Eckstein and Tsiddon (2004).

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

are illuminating. We start with two key alternatives: that our results reflect a direct personal *pocketbook* incentive to change one's vote or that they induce a change in one's policy preferences based on *sociotropic* (national rather than personal) considerations.[3] Given that peace overtures tend to raise both Israeli and Palestinian asset prices (Zussman, Zussman, and Nielsen (2008)), individuals who hold stocks on election day may have a direct material incentive to vote for parties that favor the peace process even if their political views remain unaffected. Inconsistent with the pocketbook channel, however, we find that the treatment effect is at least as strong for participants exogenously divested before election day. Instead, the evidence suggests that individuals exposed to financial markets develop different policy preferences over peace initiatives. They increase their support not only for the general principle of a two-state solution, but also for specific, and costly, concessions for peace. These effects are specific to the peace process: if anything, preferences over redistributive policies and government intervention in markets shift slightly to the *right*.

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

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

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

Finally, Section 8 studies the differences between being assigned to in-group (Israeli) versus out-group (Palestinian) assets. On the one hand, the out-group assets could have larger effects as they expose individuals to new sets of considerations and shared risks, and are more likely to demonstrate the connection between financial markets and the peace

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[3]See Healy, Persson, and Snowberg (2017) for a recent study that finds evidence for both motivations, and see Lewis-Beck and Stegmaier (2007) for a review.

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

Unlike campaigns that distribute potentially contentious information, an important feature of our intervention is that it is arguably empowering rather than paternalistic. It encourages individuals to learn about stock markets on their own and leaves them to draw their *own* conclusions about the economic costs of different policies. Further, while the treatment is rather intensive, it does not require prohibitively high stakes or long durations: assigning $50 worth of assets is almost as effective as assigning $100, and meaningful effects emerge after 4 weeks of exposure. These elements, along with the fact that it is not necessary to expose individuals to the assets of the opposing side, raise the potential for implementing the intervention at scale and in a wide range of settings.

This paper links to a large literature on conflict and underdevelopment. An important body of work shows that places that experienced violence historically tend to be more prone to future violence, often due to changes in culture or a polarization of social identities (e.g., Besley and Reynal-Querol (2014), Shayo and Zussman (2011, 2017), Sambanis and Shayo (2013)). A parallel literature examines how economic interests may offset such *passions* and mitigate violence (Hirschman (1977), Martin, Mayer, and Thoenig (2008), Rohner, Thoenig, and Zilibotti (2013), Jha (2013), Grosfeld, Sakalli, and Zhuravskaya (2019), Becker and Pascali (2019)). Indeed, exposure to novel financial assets appears to have had historical success at mitigating social conflict in three revolutionary states that subsequently led the world in economic growth: England, the United States, and Japan (Jha (2012, 2015)). Motivated by these historical cases, our contribution lies in examining whether properly designed financial exposure can have meaningful effects in a contemporary environment. Further, unlike this literature, we show that exposure to financial markets can affect policy preferences even without directly creating a significant personal financial stake.

The paper also makes two methodological contributions. First, we implement random assignment to empirically identify the causal effects of both exposure to financial assets and opportunities to trade those assets on individual political behavior, knowledge, and attitudes.[4] We develop a simplified trading platform that allows inexperienced individuals to hold and trade assets that track real stocks at their actual market prices. Notably, participants do not need to go through the process of purchasing the assets themselves, as everything is done through our platform. This offers a method of conducting experiments with an important set of factors that have thus far proven very hard to randomize, certainly at scale. Second, we use double-blinded samples in parallel surveys to measure treatment effects. This mitigates problems that arise when subjects modify their self-reports in response to the treatment. Such social desirability biases can be particularly problematic in

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

studies of peacemaking. Our use of online panels can be scaled easily, particularly as internet penetration expands, can reach broad representative samples, and can potentially be applied to questions well beyond the political economy of conflict.

2. INSTITUTIONAL AND POLITICAL CONTEXT

Our study focuses on the March 2015 Israeli general elections. Israel is a parliamentary democracy with proportional representation. Elections are called at least every 4 years. However, disagreements within the ruling coalition led the 2015 elections to be held just a little over 2 years after the January 2013 elections. The intervening 2 years also witnessed asset price rises during peace negotiations brokered by John Kerry, and falls after their collapse, which culminated in the 2014 Gaza War (Figure B2). This recent history is particularly valuable because the 2013 elections provide a recent measure of participants' (pretreatment) vote choices.[5] We focus on Jewish voters, who comprise around 80% of the population.

It is important to stress that, rather than economic policies, the main dividing line between the right and the left in Israeli politics has traditionally been about the Israeli–Palestinian conflict.[6] The Israeli right (led by the Likud party) largely favors the status quo, viewing concessions for peace as highly risky and likely to lead to a major deterioration of the security situation. In contrast, the left (led in 2015 by The Zionist Union party) sees status quo policies, including permitting settlements in the West Bank, as already costly and likely to put Israel's security and democracy at further risk. Instead it favors restarting the peace process with the goal of finding a permanent solution to the conflict. Finally, while many Israeli parties can be clearly classified as left or right based on this dimension, other parties—which we will refer to as *center*—tend to focus on different issues and are widely seen as potential members of a coalition led by either the Likud or by The Zionist Union. These include the religious ultra-orthodox parties Shas and Yahadut HaTorah, as well as parties focused on civic and economic issues, Yesh Atid and Kulanu.

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

The second major party, The Zionist Union, combined the historic Labour party with the smaller, more centrist Hatnuah party. The Zionist Union won 19% of the 2015 vote. Its 2015 platform stated that "reaching a diplomatic settlement [of the conflict] is a foremost Israeli interest and a necessary condition for securing [Israel's] future as a Jewish

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

[6]Within our sample, in an ordinary least squares (OLS) regression of ordered vote choice in 2015 on pretreatment indices of individual attitudes toward peace concessions and toward economic policies, both indices are highly significant, with an R^2 of 0.296. However, of this R^2, the peace index is responsible for 94.1%, while the economic policy index only accounts for 5.4%.

and democratic country, enjoying widespread international support." The platform further committed to restarting negotiations "with the aim of reaching a permanent settlement with the Palestinians, based on the principle of two states for two peoples" (The Zionist Union (2015), p. 5).

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

3. EXPERIMENTAL DESIGN

3.1. *Population*

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

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

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

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

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[7]Whereas The Zionist Union's platform opens with detailed specific national security and peace initiatives, Yesh Atid's 2015 platform begins with corruption, followed by chapters on the cost of living, housing, education, health, and welfare. National security is discussed in Chapter 8 (just before small businesses). The chapter lists the threats Israel faces, but does not commit to a clear policy, concluding that "Israel needs to develop a comprehensive national security conception, based on the development and reinforcement of military, political and economic power resources—and a proactive and active foreign policy—and act accordingly" (Yesh Atid (2015), p. 110). Regarding the remaining parties: Meretz and the Arab Joint List are both clearly to the left of The Zionist Union in terms of support for peace negotiations and willingness to withdraw from territories occupied in 1967. At the other extreme, Haam Itanu and Habayit Hayehudi are highly supportive of Jewish settlements in the occupied territories and oppose any withdrawals, and as such are to the right of the Likud. Finally, Avigdor Lieberman's Israel Beitenu is harder to place as, occasionally, Lieberman positions himself as a centrist on some issues. Nonetheless he has always been either part of a right-wing coalition or opposing it from the right. Classifying Israel Beitenu as center does not affect our results.
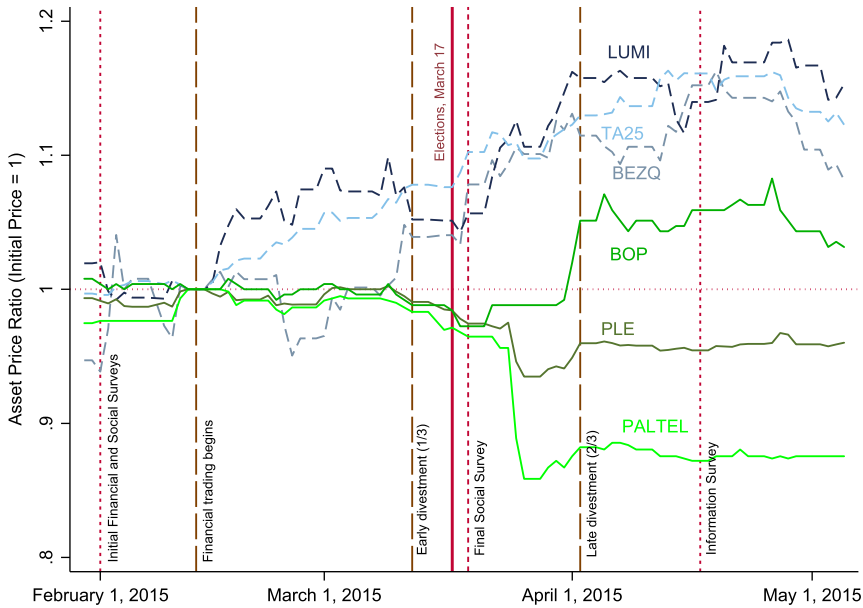
FIGURE 1.—Experimental sequence and asset prices. The study included the initial financial survey (fielded February 1, 2015), initial social survey (February 3, 2015), weekly financial surveys (with investment decisions, several), final financial survey (at divestment, either March 12 or April 2), final social survey (March 19, 2015), information survey (April 16, 2015), financial followup survey (July 19, 2015) and social followup survey (April 12, 2016). Israeli stocks are shown by dashed lines (Bezeq Telecoms (BEZQ), Bank Leumi (LUMI), and the Tel Aviv 25 (TA25)). Palestinian stocks are shown by solid lines (Palestine Telecoms (PALTEL), Bank of Palestine (BOP), and the Palestinian General Market Index (PLE)).

3.2. *Sequence*

Individuals were invited to a study on investor behavior that would include several surveys on various issues (the invitation and survey instruments are available on our websites). They were told they would enter a lottery to win either a financial asset or an initial voucher of cash to invest in a financial asset and that these assets would track the value of specific stocks from the region.[8] Among those who consented to participate, we conducted two parallel sets of surveys: financial and social. The financial surveys included questions on prior investment history (including whether they had traded stocks in the last 6 months), and a battery of questions measuring financial literacy, risk aversion, and time preference. The social surveys included questions on political behavior, social and political attitudes, and well-being. Figure 1 shows the timeline of the experiment.

Participants assigned to the treatment group were further invited to an online instructions session in which they were informed of their asset allocation (Figure B3 shows a screenshot), given detailed explanations about the rules, and quizzed to make sure they understood how the value of their assets would be determined. Those who completed the session and agreed to continue received weekly updates about the price of their assigned asset and a statement of the composition and current value of their financial portfolio. We also provided links to the Hebrew version of investing.com to allow individuals to independently track and verify the historical performance and current price of their stocks.

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

TABLE I

ASSIGNMENT TO TREATMENTS

	Total	Redeem Pre-Elections			Redeem Post-Elections		
		All	Low ($50)	High ($100)	All	Low ($50)	High ($100)
Treatment	1036						
Israeli Stocks	414	141	70	71	273	136	137
Palestinian Stocks	416	141	71	70	275	137	138
Voucher to Invest	206	64	32	32	142	71	71
Control	309						

Invitations were sent out after markets closed on the last business day of the week (usually on Thursdays). Every week, participants could reallocate up to 10% of their holdings by buying or selling a particular financial asset, commission-free. This was to encourage continuous engagement with the stock market rather than simply choosing the entire portfolio (or selling all stocks) immediately. Furthermore, participants who did not enter a decision lost the 10% that they could have traded that week. They could decide neither to sell nor to buy, but they had to enter a decision to avoid the loss. All trades were implemented via a trading platform incorporated into our surveys.[9]

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

3.3. *Treatments*

Table I shows the treatment assignment: 309 participants were assigned to the control group and 1036 were assigned to the treatment group.[11] To help disentangle the effects of material incentives generated by holding stocks on election day from the effects of exposure to financial markets, a third of the treatment group were fully divested of their assets the weekend prior to the March 17 elections. The others could continue to trade

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

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

[11]We employed a stratified block randomization procedure designed to increase balance across treatment groups in political and demographic variables. Specifically, we create 104 blocks of 13 (less for one block), stratified sequentially on 2013 vote choice (with parties ordered from left to right), sex, a dummy for whether the individual traded stocks in the last 6 months, a dummy for whether the individual would recommend to a friend to invest in stocks from Arab countries, geographical region, discrepancies in their reported voting in the 2013 elections, and a measure of their willingness to take risks. This generates relatively homogeneous blocks. Within each block we then randomize individuals into the treatments.

in their assets until 2 weeks after the elections. We further varied the initial value of the endowment to be either New Israeli Shekel (NIS) 200 (around $50) or NIS 400 (around $100). While these sums are not large (the average Israeli daily wage in December 2014 was NIS 312), they are significant compared to typical stakes in experimental economics or to the standard pay of NIS 0.1 per question paid by the survey company.

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

Finally, 206 individuals were assigned vouchers they could use to buy (and later sell) a specific index fund (funds not used for buying the index earned zero return). The vast majority (202) of them could trade in the Tel Aviv 25 Index. In addition, four traded for indices from Cyprus, Egypt, Jordan, and Turkey.[13] Both the stocks and the voucher groups traded on the same platform and received the same information. However, given our 10% limit on weekly trading, stock holdings up to and including election day would necessarily be greater for those endowed with stocks. Furthermore, the main trading possibility in the voucher group was to buy stocks, while in the stocks group it was to sell.

3.4. *Minimizing Experimenter Demand Effects*

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

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

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

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

[15]The only defense company in the Tel Aviv 25 (TA-25), Elbit Systems, had a weight of 3.26%. Note also that we would expect the effect of a stock market intervention on support for peace initiatives to be weaker if we give large stakes in companies that benefit from conflict or if we endow individuals with large short positions of the broader index.
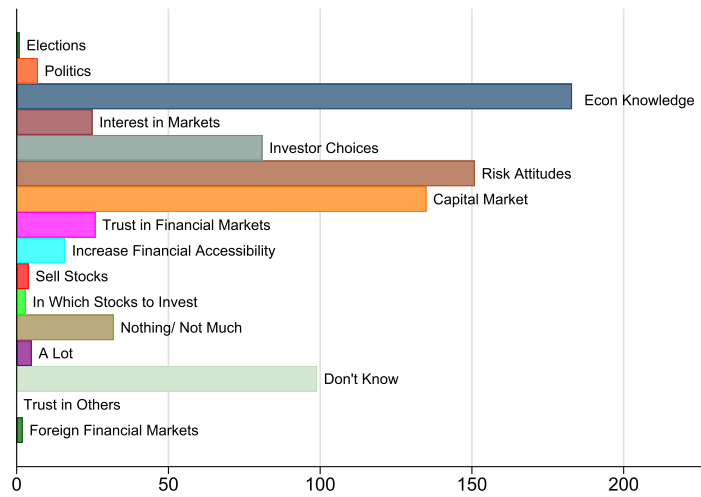
FIGURE 2.—What can the researchers learn from this study?

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

4. DATA

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

Attitudes toward making concessions for peace at baseline and attitudes toward left or right economic policies—measures that we describe in more detail below—are also similar across treatment and control. Around 36% of our sample in both the treatment and the control groups reported having traded stocks in the 6 months prior to the experiment. The groups are also balanced by basic demographic characteristics, including sex, marital status, education, religiosity, geographical location, and income. The groups have similar

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

TABLE II

DESCRIPTIVE STATISTICS AND BALANCE TESTS[a]

	Mean		Difference in Means				
			Without FEs		With Strata FEs		
	Treatment	Control	Diff.	P-value	Diff.	P-value	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Voted Right'13	0.241	0.245	−0.004	0.881	0.000	0.964	1311
	[0.428]	[0.431]	(0.028)		(0.006)		
Voted Left'13	0.137	0.126	0.011	0.625	0.005	0.213	1311
	[0.344]	[0.332]	(0.022)		(0.004)		
Peace Index	0.051	0.004	0.047	0.378	0.038	0.399	1311
	[0.823]	[0.784]	(0.053)		(0.044)		
Economic Policy Index	0.007	−0.005	0.012	0.757	0.011	0.752	1311
	[0.574]	[0.596]	(0.038)		(0.036)		
Bought/Sold Shares in	0.355	0.368	−0.013	0.686	−0.018	0.290	1311
Last 6 Mths [0/1]	[0.479]	[0.483]	(0.031)		(0.017)		
Male	0.521	0.513	0.008	0.806	0.009	0.470	1311
	[0.5]	[0.501]	(0.033)		(0.012)		
Age [Yrs]	39.289	41.530	−2.240	0.012	−2.142	0.011	1311
	[13.394]	[14.293]	(0.892)		(0.844)		
Post Secondary	0.230	0.232	−0.002	0.946	0.002	0.953	1311
Education	[0.421]	[0.423]	(0.028)		(0.027)		
BA Student	0.148	0.152	−0.005	0.842	−0.005	0.834	1311
	[0.355]	[0.36]	(0.023)		(0.024)		
BA Graduate and Above	0.426	0.427	−0.001	0.976	−0.005	0.860	1311
	[0.495]	[0.495]	(0.032)		(0.031)		
Married	0.598	0.629	−0.032	0.326	−0.033	0.295	1311
	[0.491]	[0.484]	(0.032)		(0.031)		
Religiosity: Secular	0.627	0.636	−0.008	0.791	−0.014	0.582	1311
	[0.484]	[0.482]	(0.032)		(0.025)		
Traditional	0.164	0.172	−0.009	0.723	−0.005	0.823	1311
	[0.37]	[0.378]	(0.024)		(0.024)		
Religious	0.124	0.119	0.005	0.828	0.005	0.780	1311
	[0.33]	[0.325]	(0.022)		(0.018)		
Ultra-	0.085	0.073	0.012	0.493	0.014	0.222	1311
Orthodox	[0.279]	[0.26]	(0.018)		(0.012)		
Region: Jerusalem	0.091	0.096	−0.005	0.799	−0.004	0.800	1311
	[0.288]	[0.295]	(0.019)		(0.017)		
North	0.097	0.089	0.008	0.689	0.009	0.595	1311
	[0.296]	[0.286]	(0.019)		(0.017)		
Haifa	0.142	0.123	0.019	0.395	0.021	0.291	1311
	[0.349]	[0.328]	(0.023)		(0.020)		
Center	0.290	0.298	−0.008	0.798	−0.007	0.766	1311
	[0.454]	[0.458]	(0.030)		(0.023)		
Tel Aviv	0.194	0.212	−0.018	0.500	−0.024	0.276	1311
	[0.396]	[0.409]	(0.026)		(0.022)		
South	0.104	0.116	−0.012	0.560	−0.010	0.596	1311
	[0.305]	[0.321]	(0.020)		(0.018)		
West Bank	0.081	0.066	0.015	0.392	0.015	0.341	1311
	[0.273]	[0.249]	(0.018)		(0.016)		
Monthly Family Income	10,996	11,162	−165.192	0.651	−231.199	0.511	1286
[NIS]+	[5,567]	[5,324]	(365.176)		(352.004)		
Willing to Take Risks	4.716	4.344	0.371	0.012	0.366	0.009	1311
[1–10]	[2.265]	[2.24]	(0.148)		(0.139)		
Time preference median	0.657	0.642	0.015	0.638	0.014	0.645	1311
or above	[0.475]	[0.48]	(0.031)		(0.031)		
Financial literacy: %	70.664	69.726	0.938	0.543	0.870	0.550	1311
correct	[23.359]	[23.917]	(1.541)		(1.455)		

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

Panel A

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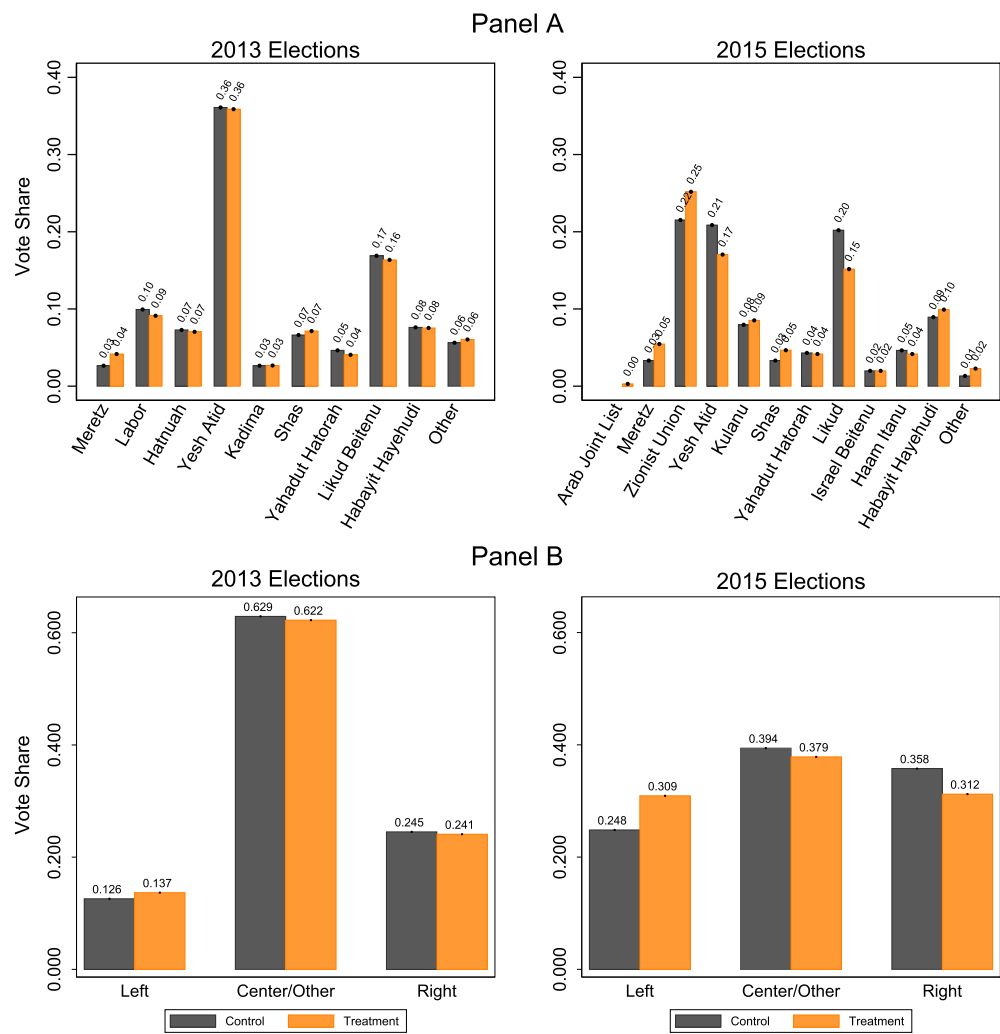
N=1311. The 'other' bars include 71 and 17 individuals who voted for for other parties in 2013 and 2015, respectively, as well as 1 and 27 individuals who did not vote in 2013 and 2015, respectively.

FIGURE 3.—Vote in treatment and control groups in 2013 and 2015. Party blocks in panel B, 2013: The left includes Meretz and Labour; the center includes Hatnuah, Yesh Atid, Kadima, Shas, and Yahadut HaTorah; the right includes Likud Beitenu and Habayit Hayehudi. Blocks in 2015: The left includes The Zionist Union, Meretz, and the Arab Joint List; the center includes Yesh Atid, Kulanu, Shas, and Yahadut HaTorah; the right includes Likud, Israel Beitenu, Haam Itanu, and Habayit Hayehudi.

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

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[17]These slight age differences actually work *against* the main effect, as, unlike in the United States, younger voters in Israel are *less* likely to vote for the left (see also Table B8). Similarly, as we show below, the effects are *stronger* for the *risk-averse*. To further check whether the number of significant differences might indicate a po-

5. MAIN RESULTS

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

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

Table III estimates the treatment effect on the probability of voting for left (columns 1–4) and right parties (columns 5–8) in 2015. We mostly report conservative intent to treat (ITT) estimates. We use robust standard errors: the clustering problem does not arise in our setting as we randomize at the individual level. Columns 1 and 5 replicate the raw mean differences in Figure 3-B. Columns 2 and 6 control for other factors that may shape vote choices (all measured pretreatment): vote in the 2013 elections, prior experience in trading stocks, sex, age, education, income, religiosity, geographical region, marital status, willingness to take risks, patience, and financial literacy, as well as 104 strata fixed effects. These controls are meaningful determinants of voting: the R^2 increases from 0.003 to 0.447 for voting left, and from 0.002 to 0.518 for voting right. Consistent with random assignment, however, the mean treatment effects are essentially unaffected. They again indicate a 6pp increased probability of voting for the left and a 4.4pp reduction in the probability of voting for the right (p-values 0.011 and 0.066, respectively).

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

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tential problem with the realization of our randomization procedure, we do the following. We randomly assign the sample of 1311 individuals in Table II to fictitious treatment and control groups, with the same proportions as those of the actual groups. We then perform the tests from columns 3 and 4, and count the significant differences, repeating this procedure 500 times. Less than 6% of the simulations have zero significant differences and less than 28% have less than two (the number we obtain). See also Figure B5.

TABLE III

TREATMENT EFFECTS ON LEFT AND RIGHT VOTE IN 2015[a]

	Vote for Left Party in 2015				Vote for Right Party in 2015			
	ITT	ITT	ITT reweighted	TOT	ITT	ITT	ITT reweighted	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.061	0.059	0.043	0.073	−0.045	−0.044	−0.051	−0.054
	(0.029)	(0.023)	(0.020)	(0.027)	(0.031)	(0.024)	(0.027)	(0.028)
Voted Right'13		−0.254	−0.201	−0.272		0.492	0.473	0.505
		(0.091)	(0.083)	(0.089)		(0.122)	(0.127)	(0.114)
Voted Left'13		0.596	0.614	0.608		−0.222	−0.249	−0.231
		(0.091)	(0.090)	(0.085)		(0.088)	(0.088)	(0.087)
Bought/Sold Shares in Last 6 Mths [0/1]		0.018	0.015	0.015		0.030	0.024	0.032
		(0.040)	(0.035)	(0.039)		(0.040)	(0.043)	(0.039)
Traditional		−0.138	−0.155	−0.133		0.102	0.128	0.099
		(0.032)	(0.029)	(0.031)		(0.032)	(0.036)	(0.030)
Religious		−0.166	−0.162	−0.165		0.241	0.232	0.240
		(0.032)	(0.031)	(0.030)		(0.049)	(0.049)	(0.046)
Ultra-Orthodox		−0.221	−0.208	−0.222		0.056	0.033	0.057
		(0.039)	(0.037)	(0.038)		(0.086)	(0.088)	(0.082)
Post Secondary		0.068	0.063	0.066		−0.060	−0.046	−0.059
		(0.033)	(0.027)	(0.032)		(0.034)	(0.037)	(0.032)
BA Student		0.088	0.072	0.088		−0.041	−0.025	−0.041
		(0.038)	(0.032)	(0.036)		(0.039)	(0.042)	(0.037)
BA Graduate & Above		0.062	0.038	0.062		−0.044	−0.021	−0.045
		(0.030)	(0.026)	(0.029)		(0.032)	(0.035)	(0.030)
Willing to Take Risks [1–10]		−0.001	0.002	−0.001		0.007	0.008	0.007
		(0.005)	(0.004)	(0.005)		(0.005)	(0.005)	(0.005)
Time preference above median		0.012	0.009	0.010		0.004	0.004	0.005
		(0.022)	(0.018)	(0.020)		(0.021)	(0.024)	(0.020)
Financial Literacy, % Correct		0.000	0.000	0.000		−0.001	−0.001	−0.001
		(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.000)
Strata FE	NO	YES	YES	YES	NO	YES	YES	YES
Demographic Controls	NO	YES	YES	YES	NO	YES	YES	YES
F (excluded instruments)				3129				3129
Observations	1311	1311	1311	1311	1311	1311	1311	1311
R-squared	0.003	0.447	0.570	0.443	0.002	0.518	0.556	0.518

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

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

Appendix Table B2 estimates the treatment effect party by party. Table B3 reports multinomial logit estimates. Consistent with the raw data in Figure 3, the treatment signif-

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

Henceforth, we summarize the voting decision in a single ordered vote choice variable, paralleling the blocks in Figure 3-B. This will be useful for studying mechanisms. We normalize the values to range from 0 for right, 0.5 for center or other, and 1 for left for comparability with the binary outcomes. Treatment effects using this measure are reported in Table B4. The linear effects on the ordered vote choice reveal a 0.052 leftward shift in the unweighted ITT, 0.047 in the reweighted ITT, and 0.064 in the TOT (*p*-values 0.006, 0.013, and 0.004, respectively). The effects tend to be higher when restricting attention to those without experience trading in stocks in the 6 months prior to the experiment.[18]

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

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

6. MECHANISMS

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

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

TABLE IV

Effects of Election Day Stockholdings on Ordered Vote Choice[a]

	OLS	OLS	OLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)
Treatment	0.052	0.038	0.045	0.057	0.059
	(0.019)	(0.020)	(0.019)	(0.020)	(0.020)
Divest Before Election		0.039			
		(0.019)			
Voucher Treatment			0.033		
			(0.022)		
Stock value-actual on election day (100s NIS)				−0.005	−0.006
				(0.006)	(0.007)
F (excluded instruments)					1100
Strata FE	YES	YES	YES	YES	YES
Demographic Controls	YES	YES	YES	YES	YES
R-squared	0.549	0.550	0.550	0.549	0.549
Observations	1311	1311	1311	1311	1311

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

The Appendix reports balancing tests across subtreatments. As before, subtreatments are balanced relative to the control on almost all dimensions (Table A2). The subtreatments are also well balanced between themselves (e.g., late divestors vs. early divestors; high initial endowment vs. low initial endowment, etc.; see Table A3).

6.1. *Economic Incentives or Changes in Policy Preferences?*

Peace overtures tend to raise both Israeli and Palestinian asset prices (Zussman, Zussman, and Nielsen (2008)). Thus, even if holding stocks does not lead individuals to change their views on any policy issues, it may still provide a direct *pocketbook* incentive to vote for parties that favor the peace process.[19] In Table IV we employ three strategies that give us exogenous variation in the degree of asset exposure on election day. First we separate the effect on individuals who were exogenously divested in the week *prior* to the elections from those who divested after (columns 1 and 2). The effect on those already divested is actually 0.039 *higher*, not lower.[20] Next we compare individuals who were initially assigned a portfolio purely of stock to those initially given vouchers with which to buy stock. Given our trading restrictions, the former held at least 66% of their assets in stock on election day, compared to 35% at most for those endowed with vouchers. However, the coefficient on the voucher treatment is statistically insignificant (column 3). Finally, we examine the effects of the actual asset holdings of each participant on election, including

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

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

instrumenting for election-day asset holdings based on the portfolio of a passive investor who registers a decision every week to hold her initial asset allocation (columns 4 and 5). However, there is no evidence for an additional effect of actual stock holdings beyond the average treatment effect.[21]

Thus the voting results do not appear to reflect any direct material incentives generated by the stocks we provided. Two points are worth stressing, however. First, our results do not rule out the possibility that larger financial stakes could induce direct incentives to change political behavior. Second, the incentives generated by the stock positions we provide could either be undone by participants trading outside the experiment or they may even be augmented as individuals become more familiar and engaged with stock markets. Similarly, individuals exogenously divested by election day might undo this treatment by investing privately. Anticipating such possibilities, we explicitly stratified on those who had traded stocks within 6 months prior to the experiment as they would be better positioned to undo the treatment. In the weekly investment surveys we also asked participants whether or not they had traded outside the experiment. Overall, there indeed seems to be an increase in the propensity to trade outside as the study proceeds (see Figure B7 and Jha and Shayo 2019). However, this variation explains little of the treatment effect (Figures A2 and A3).

Rather than the direct material incentives provided by stockholdings, individuals might change their vote choices if exposure to financial markets induces them to change their policy preferences over the peace process or in other domains. We therefore asked participants two sets of questions: on attitudes toward the peace process and on economic policy. The questions on the peace process are drawn from a national survey conducted since 2003 (Smooha (2015)). They include both a broad question on their level of approval (on a four point scale) for a two-state solution, as well as on specific concessions for peace, including the 1967 borders as the borders between the two states, the splitting of Jerusalem, and the return of Palestinian refugees to the state of Palestine.[22] On economic policy, we include questions from the World Values Survey, assessing attitudes toward income inequality and governmental responsibility for the poor. To these we add a question on the privatization of services and industries, and a question on reducing the capital gains tax on investment in the stock market. We use these questions to form both a peace index and an economic policy index, following Kling, Liebman, and Katz (2007), where higher values indicate more of a *left* position.

Table V presents the overall effect of exposure to financial assets on the two indices, as well as the effect component by component. Overall, the treatment has a strong positive effect on the summary index of agreement with the four principles underlying a potential peace deal (column 2). The effects appear stronger for the more specific and less widely accepted concessions. As with the effects on the vote, the point estimates tend to be more

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

[22]The proportions approving of these principles in our sample closely resemble the numbers in the representative sample of the Jewish population in the most recent survey, conducted in 2013. The overall trends in the population reveal either stable or falling support for these principles between 2003–2004 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 2003–2004, reaching roughly the same levels seen in our data. In 2013, support for 1967 borders with land swaps was 40.3% (44.2% in 2003), for the splitting of Jerusalem it was 22.6% (23.3% in 2004), and for the return of refugees it was 48.2% (62.6% in 2003). See Smooha (2015) for details.

TABLE V

TREATMENT EFFECTS ON ATTITUDES[a]

	Full Sample				Inexperienced		
	Mean [SD]	Treatment Effect	Obs.	R^2/ Pseudo R^2	Treatment Effect	Obs.	R^2/ Pseudo R^2
Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Indices (OLS)							
Peace Index	0.066	**0.110**	1277	0.455	**0.157**	819	0.479
	[0.833]	**(0.044)**			**(0.054)**		
Economic Policy Index	−0.019	**−0.026**	1111	0.210	**−0.104**	697	0.209
	[0.598]	**(0.041)**			**(0.054)**		
Specific Outcomes (Ordered Probits):							
Two states for two peoples	2.522	**0.101**	1277	0.231	**0.230**	819	0.265
	[1.140]	**(0.079)**			**(0.102)**		
1967 borders with a possibility	2.164	**0.164**	1277	0.213	**0.278**	819	0.238
of land exchanges	[1.083]	**(0.079)**			**(0.102)**		
Jerusalem will be split into two	1.822	**0.189**	1277	0.206	**0.213**	819	0.238
separate cities—Arab and Jewish	[1.039]	**(0.086)**			**(0.110)**		
Palestinian refugees will get compensation	2.135	**0.194**	1277	0.079	**0.262**	819	0.084
& allowed to return to Palestine only	[1.075]	**(0.077)**			**(0.099)**		
Incomes in Israel should be made more equal	4.249	**−0.009**	1110	0.044	**−0.057**	697	0.050
(vs. need larger diffs as incentives)	[2.302]	**(0.076)**			**(0.102)**		
Services and industries should be	4.530	**0.033**	1111	0.052	**−0.037**	697	0.070
owned by the Government (vs. privatized)	[2.429]	**(0.073)**			**(0.097)**		
Government responsible for helping the poor	3.299	**−0.162**	1110	0.052	**−0.291**	696	0.062
(vs. people should take care of themselves)	[2.087]	**(0.077)**			**(0.101)**		
Oppose reducing capital gains tax on	2.652	**0.053**	1104	0.073	**−0.029**	692	0.076
investments in the stock market (vs. support)	[0.999]	**(0.080)**			**(0.107)**		

[a]The top panel reports OLS (ITT) estimates of the treatment effect on attitude indices. The peace questions were asked in the March 19 survey. The economic questions were asked in the July 19 survey [The effect on the economic policy index for compliers vs control, asked March 12 (early divesters)/April 5 (late divesters) is also negative and insignificant (−0.0274[0.039])]. The bottom panel reports ordered probit estimates of the treatment effect on the specific questions composing the indices. Col 1 provides means and standard deviations [in brackets]. Each summary index is the average of z-scores of its components, with the sign of each measure oriented so that attitudes commonly associated with the left have higher scores. The z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation (Kling et al. (2007)). Robust standard errors in parentheses. All regressions include the full set of controls from Table III (Col 2).

pronounced among those less experienced in financial markets prior to the experiment (column 5). Strikingly, attitudes toward peace appear to change as much for the religious and ultra-orthodox as for secular and traditional voters (Table B8 columns 2 and 5).

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

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

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

6.2. *Salience, Information, and Peace Dividends*

We now unpack why preferences over the peace process may have changed. One plausible reason is that financial markets expose individuals to new economic considerations or make existing economic considerations more salient. To assess the latter, we asked, *"Some people say the most important issue in Israel today is the socioeconomic situation. Other people say it is security and the political process. What do you think is the most important issue in Israel today?"* Answers ranged from "only the socioeconomic situation" through "both are equally important" to "only security and the political process." Though the treatment effects are imprecisely estimated, the point estimates suggest that treated individuals are on average 3.9pp more likely to say that the main issue is only or mainly the socioeconomic situation (Table VI, panel A). The effect is driven mainly by those who had considered both sets of issues equally important.

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

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

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

This lack of enhanced political knowledge is also reflected in media consumption. In the financial followup survey in July, we asked individuals which news outlets they read

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

TABLE VI

SALIENCE, INFORMATION, AND ECONOMIC CONSEQUENCES OF THE PEACE[a]

Sample:		All		Inexperienced
	Mean	[SD]	Treatment Effect (SE)	Treatment Effect (SE)
A. What is the Most important Issue in Israel today? (OLS)[March 2015]				
Solely or Mainly Socio-Economic [0/1]	0.4074	[0.492]	**0.039 (0.030)**	**0.023 (0.039)**
Solely or Mainly Security and the Political Process [0/1]	0.1387	[0.346]	**0.007 (0.022)**	**0.005 (0.027)**
			1291	**828**
B. Economic and Political Facts (OLS) [Apr 2015]				
Political Platforms & Facts Score [Prop Correct of 13]	0.694	[0.212]	**0.002 (0.013)**	**−0.010 (0.018)**
Economic Facts Score [Prop Correct of 5]	0.533	[0.276]	**0.017 (0.016)**	**0.020 (0.021)**
Stock mkt perform. answer within 3pp of actual	0.393	[0.489]	**0.066 (0.033)**	**0.091 (0.042)**
Observations			**1238**	**782**
C. Media Consumption (OLS) [July 2015]				
Which of the following newspapers/websites do you usually read?				
Number of financial outlets [0–3]	1.117	[1.120]	**0.203 (0.074)**	**0.195 (0.093)**
TheMarker [0/1]	0.338	[0.473]	**0.079 (0.032)**	**0.086 (0.040)**
Globes [0/1]	0.398	[0.490]	**0.087 (0.033)**	**0.072 (0.042)**
Calcalist [0/1]	0.380	[0.486]	**0.038 (0.034)**	**0.038 (0.042)**
Number of non-financial outlets [0–5]	1.393	[1.032]	**−0.080 (0.075)**	**−0.135 (0.097)**
Haaretz [0/1]	0.151	[0.358]	**0.005 (0.023)**	**−0.028 (0.029)**
Israel Hayom [0/1]	0.431	[0.495]	**−0.052 (0.035)**	**−0.066 (0.045)**
Observations			**1120**	**705**
D. Consequences of a Two-State Agreement (OLS/Ordered Probits) [March 2015] Suppose Israel reaches a permanent agreement with the Palestinians based on the principle of two states for two peoples. How do you think this will affect... [1 (worsen a lot), 2 (worsen somewhat), 3 (no change), 4 (improve somewhat), 5 (improve a lot)]				
Sociotropic Index (OLS)	0.011	[0.948]	**0.041 (0.054)**	**0.130 (0.068)**
Israel's economy? (O. Probit)	3.294	[1.329]	**0.126 (0.073)**	**0.223 (0.094)**
Israel's security? (O. Probit)	2.956	[1.392]	**−0.010 (0.076)**	**0.097 (0.097)**
Personal Index (OLS)	−0.013	[0.929]	**0.003 (0.056)**	**0.030 (0.070)**
your personal economic situation? (O. Probit)	3.048	[1.047]	**−0.013 (0.077)**	**0.005 (0.101)**
your personal security? (O. Probit)	2.888	[1.237]	**−0.002 (0.075)**	**0.059 (0.094)**
Observations			**1281 / 1282**	**823**

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

regularly. There is no significant change in consumption of nonfinancial news (panel C); neither is there any difference in the *slant* of the nonfinancial media consumed by the treatment and control: they are as likely to read left-leaning news sources (*Haaretz*) and right-leaning outlets (Sheldon Adelson's *Israel Hayom*). As we return to below, however,

treated individuals do significantly increase the number of financial outlets that they fol-low.[24]

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

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

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

Thus, the treatment (somewhat) heightens the salience of economics and changes assessments of the sociotropic economic gains from the peace process. Further, rather than a change in individuals' objective political knowledge and consumption of other forms of news, treated individuals increase their consumption of financial news and their knowledge of stock market performance. In fact, as we document in our companion paper (Jha and Shayo (2019)), at the end of the trading period, treated individuals not only self-report being more familiar and confident in interacting with the stock market and more aware of risk–return trade-offs, their performance also improves on objective financial literacy tests. The effect is particularly accentuated for hard-to-teach principles such as the relative riskiness of individual stocks versus mutual funds. Treated individuals also show increased propensities to invest in the stock market after the experiment.

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[24]It is possible that financial media may have a different way to cover political news. For example, consumption of *TheMarker*—a financial newspaper that is actually physically bundled with left-leaning *Haaretz* in its print form—increased by 7.9pp, a figure rivalling that of the arguably more neutral financial newspaper, *Globes* (8.7pp). However controlling for consumption of any financial news or that of each of the three financial newspapers—*TheMarker, Calcalist* or *Globes*—separately can explain only a slight component of the treatment effects on the vote or the peace index (see Figures A2 and A3).

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

TABLE VII

DIFFERENTIAL EFFECTS BY RISK AVERSION[a]

	Ordered Vote Choice	Peace Index	Econ Pol. Index	Effects of a Peace Settlement	
				Sociotropic Index	Personal Index
	(1)	(2)	(3)	(4)	(5)
Treatment	0.016	−0.079	−0.099	−0.098	−0.129
	(0.032)	(0.075)	(0.073)	(0.093)	(0.095)
Risk Averse	−0.027	−0.176	−0.043	−0.140	−0.126
	(0.037)	(0.086)	(0.083)	(0.104)	(0.108)
Treatment * Risk Averse	0.055	0.291	0.115	0.218	0.205
	(0.041)	(0.095)	(0.089)	(0.116)	(0.120)
Demographic Controls	YES	YES	YES	YES	YES
Strata FE	YES	YES	YES	YES	YES
Observations	1311	1277	1111	1282	1281
R-squared	0.550	0.458	0.212	0.395	0.349

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

6.3. *Risk Aversion*

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

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

Table VII estimates the treatment effect, interacted with pretreatment risk aversion, on voting, policy preferences, and predictions about the effects of a peace settlement. Notice that ex ante risk-averse individuals—in both treatment and control—are not significantly different from their more risk-tolerant counterparts in either their ordered vote choice or in their economic policy preferences (columns 1 and 3, respectively). However, while risk-averse individuals in the control group are significantly *less* supportive of peace concessions, risk-averse individuals who were exposed to financial markets show significantly greater increases in support for peace concessions (column 2). Similar differences show up in perceptions of how a peace settlement would affect both Israel's economic and security situation, and the individuals' own. These heightened treatment effects on the risk-

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

TABLE VIII

VOTING INTENTIONS, ONE YEAR POSTINTERVENTION[a]

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

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

averse are consistent with exposure to financial markets, causing individuals to perceive a larger risk of continuing with status quo policies relative to the risk from negotiating for peace.

7. PERSISTENCE

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

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

3.1–3.7pp (Table B13). We also observe a positive but statistically insignificant effect on the 2016 peace index.[27]

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

8. IN-GROUP VERSUS OUT-GROUP ASSETS

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

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

Recall that the participating Israeli assets all outperformed the Palestinian assets (Figure 1). It may thus be hard to disentangle the effects of in-group versus out-group assets from the effect of price changes. Columns 3 and 4 estimate the effect of the price change of the assigned asset up until the day before the election (March 16), beyond the effect of being assigned to the treatment. The treatment effect on vote choice is higher for assets that performed better, but performance does not appear to affect willingness to support concessions for peace. Including both price change and the assets' nationality

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

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

TABLE IX

EFFECTS OF IN-GROUP VERSUS OUT-GROUP FINANCIAL ASSETS[a]

	ITT (1)	TOT (2)	ITT (3)	TOT (4)	ITT (5)	TOT (6)
Panel A: Ordered Vote Choice						
Palestinian Assets	0.032	0.042			0.042	0.055
	(0.022)	(0.026)			(0.024)	(0.030)
Non-Palestinian Assets	0.065	0.078			0.038	0.043
	(0.020)	(0.023)			(0.036)	(0.040)
Treatment			0.041	0.051		
			(0.020)	(0.023)		
Price change of asset by elections (basis points)			0.454	0.517	0.507	0.660
			(0.222)	(0.258)	(0.557)	(0.616)
F (excluded instruments)		1454		1504		958.7
Observations	1311	1311	1311	1311	1311	1311
R-squared	0.550	0.547	0.550	0.548	0.550	0.548
Panel B: Peace Index						
Palestinian Assets	0.111	0.142			0.120	0.155
	(0.051)	(0.061)			(0.058)	(0.068)
Non-Palestinian Assets	0.110	0.131			0.086	0.098
	(0.047)	(0.054)			(0.086)	(0.094)
Treatment			0.109	0.136		
			(0.046)	(0.055)		
Price change of asset by elections (basis points)			0.044	−0.023	0.442	0.632
			(0.520)	(0.597)	(1.297)	(1.428)
F (excluded instruments)		1482		1522		978.6
Observations	1277	1277	1277	1277	1277	1277
R-squared	0.455	0.455	0.455	0.455	0.455	0.455
Demographic Controls	YES	YES	YES	YES	YES	YES
Strata FE	YES	YES	YES	YES	YES	YES

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

(columns 5 and 6), the Palestinian asset effects are somewhat stronger relative to columns 1 and 2—both on voting and on the peace index—and the effects of the non-Palestinian assets are attenuated. However, the differences between the effects of Palestinian and non-Palestinian assets remain statistically insignificant.

Next, consider participation. Those assigned Palestinian stocks are less likely to take up the treatment (78.6% relative to 82.7% for the non-Palestinian). Further, even among those who took up assets, participants with Palestinian stocks tend to be less engaged: they spend less time on the weekly surveys, answer fewer factual questions about the asset and its past price performance correctly, and are not as good at predicting the next week's price performance (Table B16, panels A and B). Though individuals assigned Palestinian stocks did actively trade more in the weeks prior to the elections, this is because they are more likely to sell their asset, not buy.

Finally, there is some suggestive evidence that those assigned Palestinian assets make different inferences that might counterbalance the lower level of engagement. In particular, they are 40pp more likely than those who received Israeli assets to credit peaceful

relations with neighbors as the most important driver of their assets' value (rather than company management, workers, national economic policies and conditions, and domestic political factors; see Table B16, panel C). Additionally, those compliers who saw their financial asset's value as being driven more by peaceful relations are also more likely to support peace concessions (Table B17).

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

9. CONCLUSION

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

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

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

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