The Impact of Financial Shares on Investment, Social Identity and Political Attitudes in Israel.

Pre-Analysis Plan 1.0

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Baseline Survey Begins: Feb 1, 2015
Intervention Begins: Feb 12, 2015
First Weekly Post-Treatment Survey Received: Feb 22, 2015
Main Outcomes Survey: March 18, 2015- April 2, 2015.
Follow-Up Survey (projected): July, 2015.

Abstract:

This research program examines two main sets of questions: (i) Does the provision of temporary exposure to financial assets affect subsequent financial literacy, risk preferences and cross-national and within-nation investment behaviors for men and women? (ii) Can financial assets that expose individuals to common risks and returns across nations and ethnic groups change their social and political attitudes and behaviours? To address these questions, we assign individuals in Israel cash, company stock and index funds, both from within the country and from the Palestinian authority, and use weekly financial surveys that allows them to trade some of their asset allocation for other assets, as well as concurrent surveys to evaluate changes in their social and political attitudes and voting decisions in the 2015 Israeli elections.

Background:

We recruited 1418 anonymous individual participants (from a pool of 60,000) from the Jewish voting population that participate in the Midgam internet panel in Israel. This panel is nationally representative in terms of age and sex.

We over-sample center voters at twice their vote share-- as these are more likely to respond to the treatment. Among those that consent to the study a whole, we will conduct two parallel surveys: a series of financial surveys where they are asked to weekly allocate assets in a portfolio, and a set of social surveys. Individuals will not know that the social surveys are linked to the financial surveys, to reduce social desirability bias.

All 1418 are asked to fill out a survey on investment behavior and financial literacy. These include prior investment history, (do they hold stocks and do they actively manage stocks)

measures of *financial literacy* from Van Rooij et al (2011), *risk aversion* and *time preference* (from Dohmen et al 2011 and Benjamin et al 2010).

We screen out the following:

1. Respondents that failed to complete the financial survey.
2. Respondents who took less than 3 minutes to do the financial survey (less than 5th percentile).
3. Non-voters
4. Those that recorded a vote choice in 2013 that was 8 or more ranked parties different in two different surveys, despite being for the same vote, which should imply the same answer. (This is enough to switch from left to right and vice versa, skipping a movement to the center.)

Among the remaining 1345 respondents, we employ a stratified block randomization procedure (details of the block randomization procedure are in the Appendix). We create 103 blocks of 13, with the blocks created to stratify sequentially on: 2013 vote choice (with parties ordered from left to right), sex, a dummy for whether they traded stocks in the last 6 months, discrepancies in their reported voting in the 2013 elections over two rounds of surveys (some people appear to change their reported past votes to more closely reflect their current voting intentions- please see above and below), and a measure of their willingness to take risks.

We then assign each individual within each block randomly to the following treatments:

Basic Treatment: Assigned Financial Assets/ Cash vs No Financial Assets/ Cash
A random sample of 1036 will be assigned financial assets or cash. A further sample of 309 will not receive the financial surveys or assets but will receive the social survey. They will act as a control group for a treatment that includes the financial survey, exposure to information about Israeli and Palestinian stocks and the weekly opportunity to trade those stocks.

Nested Treatments:
In the sample of 1036 assigned the financial treatment, the following are all randomized independently:
1. Size of endowment: 517 get stocks or cash worth NIS 200 (~$50), 519 will get portfolios initially worth NIS 400 (~$100).

2. Portfolio endowment of cash vs one of each of the six Israeli or Palestinian assets listed below. They are paired so one each is Israeli and Palestinian. Each individual will get an `asset' which tracks one stock. There are 138/ 139 individuals assigned to each of the following assets.

- **Banking Sector Stocks**: **Bank of Palestine and Bank Leumi** (which means "National Bank"). These are retail banks.
- **Telecoms Sector Stocks**: **Palestine Telecommunications** (the largest private employer in Palestine) **and Bezeq** (the former Israeli telecoms monopoly).

- **Index Funds**: the **Palestine Stock Exchange General Index Fund vs the Tel-Aviv 25 Index Fund**.

- **Cash:** A subset of 206 receive cash rather than stocks (but still get the same weekly surveys with the ability to buy some financial asset). This is to provide a control group for the potentially confounding wealth effect of stock endowments.

3. Redemption date: We will allow a random subset of individuals to redeem their portfolios in the week prior to the elections (March 17, 2015) and compare them to individuals who were allowed to redeem their portfolios subsequently.

The portfolio can be redeemed at the end of experiment according to actual value on that day. We will use the closing price on that day. We will also **tell them in advance what the exchange rate will be** so there is no exchange rate risk for the Palestinian or other cross-national stocks.

Baseline Variables and Controls:
We will check balance in our randomization procedure in their answers to the following sets of questions, which we also ascertain from our baseline surveys.

These ex ante variables will also act as control variables in at least one specification for the financial investment arm of the study, as well as baselines for calculating the changes due to the treatment in some specifications.

We will also provide a specification including these as control variables for the social attitudes component of the study. The preferred specification will include the controls chosen by those that most explain the variance in voting behavior in our control group. We will also interact these variables with the treatments.

From our Baseline Surveys:
1. Trust.
2. Time Preference
3. Support for Lowering Capital Gains Tax on Israeli Stocks
4. Willingness to Recommend Overseas Stocks
5. Willingness to Recommend Arab Stocks
6. Attitudes towards Redistribution and Equality
7. Political attitudes (see social/political survey below)

Data provided by the Panel:
1. Age
2. Marital Status

3. Number of Children
4. Geographical Region within Israel (8)
5. Education
6. Religious Community
7. Secular/ Orthodox
8. Main Occupation
9. Coarse Income categories
10. Country of Birth (and that of parents)
11. Year of Immigration

Attrition

Some individuals may choose to opt out of the survey after being given their initial asset allocation. We hypothesise that this will be greater for those with lower patience, those assigned lower-valued assets, and those assigned Palestinian stocks. We will nonetheless attempt to follow these individuals in subsequent surveys, including the social/ political surveys, to allow for intention-to-treat analysis. We will present results with assignment to treatment as an instrument for treatment status to address potential differential attrition.

Weekly Financial Survey:

All individuals will receive a weekly update on the performance of their stock. They will be allowed to sell upto 10% of their portfolio weekly in exchange for cash, or, if they hold cash, to reinvest in that stock. We will track when individuals make trades (they have from the Thursday TASE and PSE close to Sunday 9am (before the TASE and PSE opens) to make their investments.

Note that individuals will still have at least (0.90^7) = 48% in their initial allocation after the 8 weeks of the study (with at most 7 weekly financial surveys).

Thus we will track the following:

1. Time that the Survey began (since the Shabbat falls within the survey period, this will be an auxiliary test of religiosity, as described below.)
2. Length of Time the Survey Took.
3. Whether they buy or sold their stock and how much they choose to do so.
4. A series of questions to test their basic knowledge and subjective evaluation of the stock, both retrospectively over the past week, and prospectively for the coming week.
5. We will also ask which of a list of possible factors most affects the stock's performance', allowing for managerial quality, economic and political conditions as options. This will allow us to test between-group and within-group *fundamental*

attribution errors: eg. whether rising prices are attributed to skill in the in-group, while falling prices are attributed to exogenous circumstances, and whether this is greater for the in-group than for the out-group.

6. Whether they traded in non-experimental stocks during the past week, and whether they traded in international stocks.

Social/ Political Survey

In parallel to the financial surveys, individuals who consent will be invited to a series of social surveys. In this survey they will be asked questions both before the treatment, once in the middle (on March 2nd) and also following the financial experiment about their voting intentions (whether they intend to vote and for whom), and for those surveyed after the election, whom they voted for.

We ask the following questions:

1. Intentions to turn out on voting day.
2. Which party they are likely to vote for.
3. Whether the main issue of the election is economic or security.

4. We will also ask the extent to which they are "proud" of particular groups, which provides a gauge of self-identification. The list includes:

a. Israelis
b. Jews
c. Arabs
d. Secular
e. Traditional
f. Religious
g. Orthodox
h. Rich
i. Middle Class
j. Poor
k. Orientals
l. Ashkenazi
m. Immigrants

5. We ask them whether the two state solution will improve:

a. Israel's security and economy
b. Their personal security and economy.

6. Arab-Jewish relations: They will be asked if they supported an environment in which the following would occur:

a. Arabs attend Jewish high schools
b. Arabs live in Jewish neighbourhoods
c. Arabs and Jews open joint businesses
d. Arabs manage Israeli companies
e. A joint political coalition with Arab parties exists

7. We also ask their views on the appropriate peace bargain, allowing the following options:

a. Two countries for two nations
b. 1967 borders with land exchanges
c. Splitting Jerusalem
d. Refugees get compensation and settle in Palestine
e. Palestinians are mainly to blame for the conflict
f. Israel should integrate with the West and only maintain necessary contacts with the Arabs

8. We also ask a battery of questions about well-being based upon the aspects of well-being with the 10 highest marginal utilities in Benjamin et al. (2014). These include:

a. General well-being
b. Happiness of the family
c. Health
d. View of self as moral
e. Strength of family relationships
f. Financial security
g. General security about life and the future
h. Feeling of agency
i. Feeling that life is significant and valuable.

9. Finally we ask if the individuals voted in the last election, and if so, for whom. As noted above, we throw out those for whom there was a large (8 party) discrepancy. For those with discrepancies that we do not throw out, we use their original recorded vote as the benchmark, with the survey response as a robustness check. It is worth noting that for a number, the discrepancy matches past party coalitions as well as seems to be reflective of resolution of cognitive dissonance— among those with discrepancies across the surveys, those that intend to vote for a party in the upcoming election are somewhat more likely to report that they voted for the same party in the previous election as well.

Intermediate and Final Social Surveys:

The intermediate and final social surveys will track changes in the responses above as well as additional questions that may arise due to exogenous changes in events. The final social survey

will consist of two components: a short exit poll the day after the March 17 election, and a more detailed survey of social and political sentiment as above administered within a week following the elections.

Final Financial Survey (to be completed)

1. In the Final financial survey, individuals will be asked questions above to see whether exposure to the stock has changed their preferences and financial literacy.
2. We will also ask if individuals traded non-experimental stocks during the survey period.
3. Individuals will also be given the option to make a charitable contribution from their winnings to a range of charities that span the political spectrum. This will provide a further costly measure of political preference.
4. We will also ask their self-reported likelihood of holding cross-national and domestic stocks in the future.
5. We will also ask "Do you want more information about how to register for an account that helps you to track and trade local and international stocks in the future?" [Yes / No]. If yes: we will provide a URL for a broker. This provides a behavioural measure of willingness to invest that will complement the tracking survey of their actual investment propensities.
6. Individuals will also be asked an open ended question about what they thought the study was about, to check whether they associated it with the social and political survey.

We will follow up a month later to check whether they followed through with stock purchases and why or why not.

Hypotheses

We hypothesize that exposure to financial assets may have treatment effects on two different types of behavior: **financial and social/ political**, which we will treat as distinct domains.

Base specification:

$$\boldsymbol{Y_i = f(Asset_i, Palestinian_i, Cash_i, High_i, Index_i, Late_i, X_i, W_i, \epsilon_i)}$$

where outcome $\boldsymbol{Y_i}$ (eg vote) is measured at the individual level; $\boldsymbol{f}$ is a link function that will be linear, sigmoidal, ordered probit, etc. appropriate to the outcome, $\boldsymbol{Asset_i}$ is an indicator of assignment to a financial asset, $\boldsymbol{Palestinian_i}$ is an indicator of assignment to a Palestinian financial asset; $\boldsymbol{High_i}$ is an indicator of assignment to the NIS 400 treatment; $\boldsymbol{Cash_i}$ is assignment to the cash treatment; $\boldsymbol{Index_i}$ is an indicator of assignment to the index relative to a

firm, and $\boldsymbol{Late_i}$ is an indicator of redemption date following the elections. $\boldsymbol{X_i}$ indicates the series of baseline controlsand $\boldsymbol{W_i}$ indicates an additional set of controls that will differ by hypothesis, and will be chosen based upon analysis of the control group data such that the variables are unrelated with the treatment but are significant predictors of the outcome. $\boldsymbol{\epsilon_i}$ is an idiosyncratic error term. Since we randomise at the individual level, we do not expect there to be correlation in errors within groups in assignment, but do anticipate heteroskedasticity, so we will employ robust standard errors.

- We will also present the specifications with dummy variables by assignment to stock for each firm or index.

- We will also present the following interacted treatments.

  $$\boldsymbol{Y_i = f(Asset_i, Palestinian_i, Cash_i, High_i, Index_i, Late_i, High_i * Palestinian_i, High_i * Cash_i, Late_i * Palestinian_i, Late_i * Cash_i, Index_i * Palestinian_i, X_i, W_i, Z_i, \epsilon_i)}$$

- We will also present specifications using only the subsamples assigned to Israeli and Palestinian stocks, and only to Israeli stocks versus cash, depending on the hypotheses that we wish to examine, as presented below.
- As discussed above, we will also present intent to treat estimations, if feasible, if treated individuals respond to the follow up surveys. This will involve using assignment to treatment as an instrument for treatment status.

Notes:

1. There will be foreseen events (like Netanyahu's speech to Congress on March 3) and **unforeseen events** that emerge during the two months prior to and during that we will also track the influence of, as described below, since we will have some weekly data.
2. The cash treatment also allows us to separate the effect of information from asset-holding, as individuals with cash and financial assets receive the same information. It also allows us to control for potential Hawthorne effects (as they receive the same survey).
3. We will also compare voting intentions t of our treated groups both to our control group and to broader tracking polls of voting intentions from the Israeli Election Studies, inverting our sampling weights to allow comparability to the population at large.

Financial Responses:

In this domain, we will study how random assignment of shares to individuals influences investment preferences and behaviour.

Baseline controls include measures of previous vote, risk preference, financial literacy as well as controls for education levels, region, occupation and income.

We will provide one-sided tests of the following, (and two-sided where mentioned), as well as multiple-hypothesis testing corrections within each lettered set, as described below.

In addition, we will also run an alternative specification in which we included a full set of dummies for assignment to each of the individual stocks. This is because the assignment of individual stocks will induce **winners and losers**. Some stocks will go up and others will go down. We do not know in advance which will go up or down, but as we give them individual stocks, there will be variation in returns and the `intent to treat' will still be random since the assignment to a particular initial stock is random. We can subsequently therefore test mechanisms such as Malmendier and Nagel 2011 (on positive returns inducing changes in risk aversion, stock investment etc.)

Main Financial Hypotheses:

To the best of our knowledge, we are the first study to randomize assignment to stocks. This allows us to causally estimate the effects of temporary stock exposure on a series of different types of preferences and behaviour.

1. **Stakes:** We expect that the effects of our treatments will be larger for those in the `high' stake category relative to those in the low stake category. If there is a `backlash', we expect this backlash to be smaller for those in the high stake category. We can also use this to check if risk aversion (converting stocks to cash) changes with endowment.

2. **Trust**: Guiso et al 2008 show that those that trust more are more likely to invest. We can test whether the causality runs from share exposure to trust. In common with the `starting small'/ screening literature (eg Ghosh and Ray 1996 etc) and the literature on experiences in shaping preferences (Huberman 2001, Malmendier and Nagel 2011), we hypothesise that stock exposure will also result in greater trust if the prices of the assigned stock rise, leading to greater investment over the test period and greater likelihood of longer term

willingness to invest in stock. Conversely, falling asset values we expect to result in lower willingness to invest in the future.

3. **Well-Being:** To parse the mechanism, if we find a positive effect on trust, we examine how positive returns on stock affect happiness (positively we think, particularly for (f).
   a. General well-being
   b. Happiness of the family
   c. Health
   d. View of self as moral
   e. Strength of family relationships
   f. Financial security
   g. General security about life and the future
   h. Feeling of agency
   i. Feeling that life is significant and valuable.

4. Time Preference:

1. We hypothesize that those with more ex ante patience will experience greater treatment effects in general. This may be because, by giving individuals stock, we activate their thinking about giving up short term benefits for longer term gains-- or ``interests'' rather than ``passions'' (Hirschman 1977).
2. (correlational): we hypothesize that those with greater ex ante time preference are more likely to hold stock and less likely to divest early.

5 **Support and willingness to recommend stocks.** We will test for treatment effects in the following, both individually and with joint hypothesis testing corrections.

   a. Lowering Capital Gains Tax on Israeli Stocks
      i. We expect that those treated with the stock treatment will be more likely to support lowering of the capital gains tax.
      ii. Further we expect this to be stronger for those invested in Israeli stocks.
      iii. Endowment effect: We will compare individuals who are endowed TA-25 stock endowments to those endowed with cash, who have the option to purchase stock. We expect that those endowed with the TA-25 are more likely to support a lowering of the capital gains tax, controlling for wealth.
   b. Willingness to Recommend Overseas Stocks
      - We expect this to rise for those treated with Palestinian stocks, conditional on the performance of the specific stock.
   c. Willingness to Recommend Arab Stocks
      - We expect this to rise for those treated with Palestinian stocks, conditional on the performance of the specific stock.'

6. Attitudes towards Redistribution and Equality

We will test for these both individually (with a two sided test) and with joint hypothesis testing corrections.

a. A wealth effect should suggest that individuals may be less willing to agree to redistribution.
b. At the same time, if stocks affect individuals' sense of social identification, they may be more likely to be support redistribution and equality. We hypothesise that the effects will be greater for those treated with Israeli stock and specifically the Israeli index.
c. Endowment effect: We will compare individuals who are endowed TA-25 stock endowments to those endowed with cash, who have the option to purchase stock. We expect that those endowed with the TA-25 are more likely to favour redistribution, controlling for wealth.

7. **Wealth effects:** We hypothesise that the treatment will result in stronger changes in outcomes for those for whom the treatment is a greater proportion of their income. We measure wealth by coarse income categories (ses), education and occupation. We will present results that both control for these categories and interact them with our treatment, and test this hypothesis with a one-sided test on the coefficient of the interaction term.

8: Pre-existing exposure, learning and unravelling of treatment:

(a) In assigning treatment, we explicitly stratify upon whether individuals traded stock in the past 6 months. We will test for heterogeneous treatment effects among those that were already active in the stock market. Please note:
   1. If they are undoing the treatment to regain their preferred portfolio, we should see lower effects among those that ex ante held or traded stocks.
   2. It is also possible that those with no experience with stocks in the past may have stronger effects, as they gain their first exposure to the market and there is greater learning.

(b) These have mutually reinforcing effects, so we will test the joint hypothesis with a one sided test on the coefficient of the interaction term on past stock trading.

(c) It may also be the case that additional trading behaviour, if any, reinforces the treatment by providing a vehicle through which individuals ``double down'', therefore we allow for a two-sided test on the interaction.

(d) To separate the two hypotheses in (a), each week we also ask if individuals are trading non-experimentally assigned stocks, domestic and foreign, during the experimental

period. This question itself will also serve as an outcome measure of spillover effects on financial investment behaviour due to the treatment.

(e) (Correlational) Such extra-experimental stock activity also will serve as a measure of involvement that may mediate the treatment effect, as described in (13) below.

9. Risk: we hypothesise that stock exposure will also result in greater trust if the prices of the assigned stock rise, leading to greater investment over the test period and greater likelihood of longer term willingness to invest in stock (as in Malmendier and Nagel 2011). Conversely, falling asset values we expect to result in lower willingness to invest in the future.

10. Financial Literacy. Financial literacy has proven difficult to influence through education programs (Hastings et al 2013).

1. We hypothesise that financial literacy will rise even with a two month period of ``learning by doing'' exposure both to cash and assets over baseline, and to stock over cash.
2. We hypothesise that in the weekly tracking surveys, trading of stock outside the experiment may rise over the experimental period in the stock treatment, and of international stocks in the Palestinian treatments.
3. We also hypothesise that the political effects of the stock treatment will be greater for individuals who know that stocks are a form of ownership.

11. Financial Literacy, Risk Attitudes and Gender Interactions: Bucher- Koenen et al (2014) provide cross- country evidence for gender differences in financial literacy across countries, with women lagging men, even in environments with favourable economic conditions. Women also tend to be more risk-averse. At the same time, Barber and Odean (2001) find that men trade more and are more over-confident.

- We can test whether there is a heterogeneous effect on women from learning by doing among those assigned stocks. Because they start from a lower base, women's financial literacy may increase more, and that they will experience greater increases in the measures in (10).

- However, it may be the case that men, particularly controlling for previous exposure to stocks, will trade more, which will increase their propensity to ``learn by doing'' and thus to increase their financial literacy.

We will examine these mechanisms by testing the interaction between gender and assignment to no assets as well as cash or stock on the following outcomes:

1. Risk taking, measured by the extent to which individuals choose a larger share of stocks in their portfolio

2. Actual financial learning by doing, measured by the knowledge individuals have regarding the performance of their assigned stock
3. We will also track whether risk-taking and financial learning increase over the duration of the experiment. We hypothesise that once again, there will be an endowment effect: those assigned to stocks will exhibit greater effects than those assigned to cash.

12. Gender differences in investment behavior: there is a large and growing literature on differences in economic behavior across genders (e.g. do women shy away from competition?). Random assignment of financial assets allows us to study gender differences in investment behavior when individuals of both genders are exogenously endowed with the exact same asset. In particular, we hypothesize that women will be less active in their weekly investment decisions. In particular, compared to men, women will sell fewer stocks when endowed with stocks; but also buy less stock when endowed with cash.

12. Seeing the Future and Sophistication: Because of an Israeli holiday (Purim) on Thursday, March 5th, we will administer the survey on Wednesday, rather than on Thursday on that day, with prices determined by the Wednesday close in Israeli markets. However, this means that individuals will be able to "**see the future**" in Palestinian stock on Thursday, as those markets are not closed and they are trading based upon the price of Wednesday's close. We can track whether sophisticated traders (those with higher ex ante financial literacy) choose to emulate `high-frequency / flash trading' to invest more in Friday stock market gainers in the Palestinian market, and the timing of when they do their investing.

13. Involvement: (correlational) We hypothesise that the treatment effects will mediated by involvement, as measured by the proportion of weekly survey completed.

Social and Political Responses.

The main hypothesis is that individuals assigned Palestinian stocks will be more likely to alter their attitudes towards the peace process and their voting preferences than those that were assigned cash or Israeli stocks.

1. **Voting:**
    a. We measure the following with a one sided test:
        i. that there will be a swing to the left in voting.
        ii. an increase in turnout for those on the left, relative to those on the right.
    b. There may also be a backlash, so we allow a two-sided test. The backlash effect may result in a swing to the right.

c. Shares may also allow individuals to hedge the political outcome (as in Matozzi 2008), and thus lead to lower turnout. Again, we allow a two-sided test.

2. **Peace deal:** We hypothesise that individuals with the Palestine treatment will be more likely to move their view to the left when it comes to their support for the following principles for a potential peace deal. We present both one and two sided tests and treat these as a single domain for multiple hypothesis testing.
    a. Two countries for two nations (this is the left-center position- they will be more likely to support)
    b. 1967 borders with land exchanges (this is the left-center position- they will be more likely to support)
    c. Splitting Jerusalem (this is the left position- they will be more likely to support. However this is the most controversial and we expect smaller effects.)
    d. Palestinians are mainly to blame for the conflict (they will be more likely to oppose)
    e. Israel should integrate with the West and only maintain necessary contacts with the Arabs (they will be more likely to oppose)

3. Exposure to Israeli and Palestinian stocks may also **change individuals' self-identification** with different groups-- i.e. make individuals feel more Israeli if they share in the Israeli stock market.
    a. We hypothesise that an individual's self-identification with Israel (measured by pride in Israel and preference for living in Israel) is higher if they are exposed to the Israeli stock market than if they are exposed to the Palestinian stock market, cash or none.
    b. Furthermore, if the Israeli stocks went up, the effect of Israeli stocks (but not of cash and no-assets) will be accentuated, but will be attenuated if the stocks lose value.
    c. We test this in two waves (depending on the price fluctuations): in the intermediate social survey and in the final survey.

4. **Attitudes:** We hypothesise that mere exposure to stocks will lead individuals to be more likely to answer yes on the following questions (we test this one-sided, as well as employing a multiple hypothesis test correction.) Furthermore, the effect should be stronger for exposure to Palestinian stocks.
    a. whether the two state solution will improve Israel's security and economy,
    b. whether the two state solution will improve their own economic situation.

5. **Arab-Jewish relations:** We hypothesise that all the following will increase with exposure to Arab stocks, particularly if the stocks rise in value (in a one sided test). Respondents are asked if they supported an environment in which the following would occur:
        a. Arabs attend Jewish high schools
        b. Arabs live in Jewish neighbourhoods

c. Arabs and Jews open joint businesses
d. Arabs manage Israeli companies.
e. Whether a joint political coalition with Arab parties exists.

We hypothesise that the joint business question will be disproportionately affected. There may also, however, be a backlash, so we will also present two-sided tests. We further treat a and b (social interactions), c and d (business interactions), respectively as domains for multiple hypothesis corrections.

6. We expect there to be **financial home biases** in common with the literature (eg Huberman 2001, Grinblatt and Keloharju 2001). The literature also shows that investors from `blue' and `red' states invest differently eg Hong and Kostovetsky 2012).

1. We differ from Hong and Kostovetsky by randomly assigning individuals the same stocks. We hypothesise that political home bias exists in our context with a comparison in the propensities to divest from Palestinian stock among those with ex ante right wing political preferences vs those with left wing political preferences with a one-sided test.
2. We will quantify the extent to which the **home bias** is driven by ex ante financial literacy, gender, information (as measured by individuals' accuracy in answering questions about the stock and its performance), ex ante risk and time preferences and ex ante political ideology in a multiple regression. We hypothesise that (and measure the extent to which) right wingers divest faster from Palestinian stock (relative to left-wingers) and the realized monetary penalty or gain from doing so (if any).
3. We will test and quantify whether randomly assigned exposure to particular Palestinian stocks that turn out to do well versus badly reduces such home bias week by week.

7. **Incentives versus Social Identification:** The differences in **redemption date** allow us to check the mechanism underlying any differential effect of being assigned Palestinian vs Israeli stock.

- If individuals' political behavior responds to financial *incentives*, the effect will be greater for those who redeem after the election, since the election itself is likely to affect the stock value of Palestinian stocks more positively with greater prospects for peace.
- However, the effect of stock may be mainly about *social identification*: holding the stock may change the way you identify with groups (see 3 above). This mechanism would be largely invariant to the redemption date.

These hypotheses suggest one-sided tests of positive versus no effect on $Late_i$. We will also allow two-sided tests.

8. **Gender Interactions:** As noted above, Bucher- Koenen et al (2014) provide cross- country evidence for gender differences in financial literacy across countries, with women lagging men,

even in environments with favourable economic conditions. Women also tend to be more risk-averse. At the same time, Barber and Odean (2001) find that men trade more and are more over-confident.

1. We will test whether there is a heterogeneous effect on women's political and social attitudes versus men. Women may divest more due to greater ex ante risk aversion.
2. However, greater propensities to trade may induce a lowered *incentive* effect on political preferences (due to the likely correlation between Palestinian stock performance and the peace process) if men divest more from stock as a result. This will, we hypothesise, accentuate a greater endowment effect for men, as greater propensities to trade imply that they are more likely to invest in stock if they are assigned cash and more likely to divest from stock if assigned stock. We thus employ a two sided test, and interact gender with the assignment to stocks and cash.
3. Further, separate from the *incentive* effect, the act of trading stock may lead to greater involvement and a greater feeling of `ownership'. This may lead to a greater effect on political and social attitudes for men who trade more.

9. **Geographical and time series variation:** there may be differences in unanticipated events that shock different areas of Israel differently during the study period.

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Appendix: Block Randomization Procedure:

a.First we sort the data lexicographically by:

1. vote in the 2013 elections (order the parties from right to left). [D]
2. gender [D]
3. financial history (traded stocks in the past 6 months: yes/no) [F]
4. discrepancies between reported vote choice in the 2013 between panel survey and our baseline survey
5. risk aversion (subjective willingness to take risks) [F]

b. Next we divide the data into consecutive sets of 13. Note that if a particular set overlaps categories, they will still be as similar as possible on this dimension and possibly identical on the previous variables.

c. Within each 13 – we randomize to 7 primary treatments

1. P-high (NIS 400, Pal. stock, N=2)
2. P-low (NIS 200, Pal. stock, N=2)
3. I-high (NIS 400, Isr. stock, N=2)
4. I-low (NIS 200, Isr. stock, N=2)
5. CASH-high (NIS 400, N=1)
6. CASH-low (NIS 200, N=1)
7. NA (No-Assets, N=3)

d. Now we take ONLY the P treatments (~400).

1. Sort by:

1. primary treatment (high/low)
2. the above 5 variables

2. We divide these once more into consecutive sets of 3 and within each 3, randomize to the three Palestinian assets:

. (A) Bank of Palestine **(BOP)**
(B) Palestine Telecommunications **(PALTEL)**
(C) Palestine Stock Exchange Index Fund **(PLE)**

e. Repeat for the I treatments (~400):

1. Sort by:
a. primary treatment (high/low)
b. the above 5 variables

2. Divide to consecutive sets of 3 and within each 3, randomize to the three Israeli assets:
(A) Bank Leumi **(LUMI)**
(B) Bezeq **(BEZQ)**
(C) Tel Aviv 25 **(TA25)**

This generates the exact asset (SYMBOL*high/low) for each individual in the asset treatments.

f. Now take all the asset treatments (P,I,Cash) (~1000)
sort by:
1. primary treatment (P/I.cash*high/low)
2. the above vars
divide to consecutive sets of 3 and within each 3, randomize to redemption dates:

i. **March 12, N=1**
ii. April 2, N=2