

SUPPLEMENT TO “VALUING PEACE: THE EFFECTS OF FINANCIAL  
MARKET EXPOSURE ON VOTES AND POLITICAL ATTITUDES”  
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APPENDIX A

*Note:* Appendices denoted A appear both on the journal’s and on the authors’ websites. Appendices denoted B appear only on the authors’ websites.

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*A.1. Effects of Paper versus Realized Losses*

Recall from Section 6.1 that the treatment effect on those divested before the election is not smaller than the effect on those who had experimentally assigned *skin in the game* on election day. This is inconsistent with direct material incentives explaining the effect. However, it remains an intriguing question why individuals who were divested before the elections actually appear to respond *more* in their voting decisions (column 2). One possibility is that knowing that they were committing to a shorter duration made early divesters more likely to take up the treatment to begin with. It may have also made them more engaged in trading and in other parts of the study during the period prior to elections, increasing the treatment intensity. However, early divesters are only 0.011

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(standard error  $se = 0.026$ ) more likely to take up the treatment, and do not appear to engage in more trades, have more accurate knowledge of their stock's performance, spend more time on the survey, or be otherwise more engaged prior to the elections (Table B16).

Instead, we unpack the results in light of a distinction highlighted by Imas (2016): that differences in risk-related behavior across settings can be reconciled by the differential effects of realized losses versus paper losses. In particular, Imas shows that individuals who experience realized losses tend to become more averse to risks, whereas those who experience paper losses become more risk-seeking. If this is true and if the treatment operates in part through exposing individuals to broader economic risks, then the effects should be greater for those with realized losses relative to paper losses. We examine this in Table A5. The first three columns replicate the results from Table IV in the paper. Column 4 examines whether the treatment effect differs for early and late divesters according to whether the price of their assigned asset rose or fell prior to the early group's divestment. The results appear to confirm Imas's interpretation: while those whose assets did well show similar effects among both early and late divesters, among those whose prices fell, the effect is 0.084 ( $se = 0.029$ ) for those who divested before the elections, while it is 0.005 ( $se = 0.024$ ) for those who did not realize these falls in price. Column 5 uses the price change to instrument for realized versus paper portfolio gains and losses, showing a consistent picture: those with realized losses by election change their vote, while those with paper losses are less sensitive.

Finally, columns 6 and 7 in Table A5 repeat this exercise for the subset of individuals who reported (pretreatment) a willingness to take risks that is at or below the sample median. Consistent with the risk sensitivity interpretation, the difference between those with realized and paper losses is further amplified for the risk-averse. As we show in Section 6.3, the risk-averse appear to respond more to the treatment in their attitudes toward the peace process as well.

### A.2. Testing for Effects due to Wealth and Affect

One possibility is that receiving a financial portfolio worth \$50 or \$100 might have some form of wealth effect that could change policy preferences directly. It could also affect well-being or increase stress. It is worth observing, however, that the initial amounts we provide are unlikely to change an individual's overall wealth meaningfully enough to influence voting a month later. Further, as we just saw, economic policy preferences move, if at all, slightly to the *right* rather than to the left.

However, we can test whether the effects of asset exposure are larger for the poor, as one might expect with a direct wealth effect. Table A6 (columns 1, 3, and 5) estimates the interaction of the treatment with an indicator for below average pretreatment income on the vote choice, peace index, and economic policy index. As expected, poorer individuals do support more left-leaning economic policies in our sample (column 5). However, the interaction term shows no significant difference in the treatment effect for this group for any of these outcomes.

A related test of a potential wealth effect is to see if the effects are greater for those who received the high allocation. As column 2 suggests, while the effect of being assigned \$50 of financial assets is 0.044 on the ordered vote choice, the effect of being assigned \$100 is only 0.016 larger (a statistically insignificant difference).

Another possibility is that the provision of financial assets causes meaningful changes in individuals' well-being, mood, or affective states of mind, potentially associated with

winning a lottery or with having to make financial decisions. In other settings, the positive effect of such chance events has tended to favor incumbent parties, which should, if anything, attenuate our results (see, e.g., Healy, Malhotra, and Mo (2010)). To examine this directly, we asked individuals immediately after the elections not only about their overall life satisfaction, but also a battery comprising the top predictors of well-being based on Benjamin, Heffetz, Kimball, and Szembrot (2014), Table 2. As we show in Table A7, however, the treatment did not significantly change *any* individual indicator of subjective well-being or a combined index of all indicators. Taken together, our treatment effects do not appear to be due to a wealth effect or to a change in mood or affective state.

### A.3. *Differential Effects by Risk Aversion: Theoretical Intuition*

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

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

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

### A.4. *How Much of the Treatment Effect Can be Explained by Different Mechanisms?*

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

Figure A2 shows the estimated treatment effect on the ordered vote choice, after controlling for different outcome variables. The change in coefficients suggests a consistent pattern that highlights the relationship between asset exposure, attitudes toward peace, and a focus on the gains to the broader economy. In the postelection social survey (top left panel), individuals' attitudes toward peace stand out as a major factor that both is

influenced by the treatment and is correlated with the vote choice: holding individuals' posttreatment peace attitudes constant attenuates the treatment effect by 28.6%. Two other factors also stand out: the fact that, as we have seen, treated individuals are (somewhat) more likely to view socioeconomics as the main issue in the election and that they also increase their assessment of the potential gains to the Israeli economy from a peace agreement. Both these factors also correlate with a vote for parties supportive of the peace process, and controlling for them attenuates the treatment effect by 9.6% and 17.3%, respectively.

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

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

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

As one step in this direction, the bottom right panel of Figure A2 compares the extent to which controlling for different responses among the compliers augments or attenuates the treatment effect. First observe that controlling for those who traded outside the experiment actually strengthens the treatment effect. This suggests that these outside trades might indeed have played a small role in undoing the treatment.

TABLE A1  
COMPARISON OF THE SAMPLE AND THE ISRAELI POPULATION<sup>a</sup>

	Randomization Sample (N = 1345)	Observed vote (N = 1311)	Israeli Jewish Population	Israeli Population
1. Region: Population in District (%)				
Jerusalem District	9.4	9.2	11.1	12.5
Northern District	9.5	9.5	9.5	16.4
Haifa District	13.7	13.7	10.7	11.7
Central District	29.2	29.2	28.5	24.4
Tel Aviv District	19.8	19.8	20.2	16.3
Southern District	10.6	10.7	14.2	14.4
West Bank	7.8	7.8	5.8	4.5
2. % Female in Pop., 18+	48.3	48.1	51.4	51.3
3. Age (Population above age 18 (%))				
Male 18–24				
25–34	10.1	9.5	14.6	16.1
35–44	29.6	29.1	20.4	21.0
45–54	28.1	28.6	18.7	19.5
55–64	15.0	15.3	14.7	14.9
65+	9.6	9.8	15.1	13.9
Female 18–24				
25–34	7.6	7.6	16.5	14.5
35–44	14.2	14.1	13.3	14.6
45–54	29.7	29.0	19.2	19.9
55–64	26.3	26.3	17.9	19.0
65+	14.0	14.1	14.6	14.9
55–64	10.5	10.8	15.5	14.3
65+	5.4	5.6	19.5	17.3
4. Religiosity (Jewish Population aged 20 and over (%))				
Not religious/Secular	63.1	63.1	43.4	
Traditional	16.8	16.7	36.6	
Religious	11.9	12.0	10.6	
Ultra-orthodox	8.2	8.2	9.1	
5. Schooling (%)				
Less than high school grad (0 to 10 yrs.)	5.8	5.7	13.7	18.3
High school graduate (11 to 12 yrs.)	13.7	13.7	33.3	33.9
Post-secondary/BA Student (13 to 15 yrs.)	38.2	37.9	24.1	22
College grad and above (16+ yrs.)	42.3	42.6	28.9	25.9
6. Net Monthly Income per Household (NIS)				
Mean	10,978	11,035		14,622
Median	12,000	12,000		13,122

<sup>a</sup>Sources for Israeli population data (last two columns): 1: Statistical Abstract of Israel 2015, Table 2.15, 2014 Totals. 2, 3, 5: Statistical Abstract of Israel 2015, Table 8.72, 2014 Totals. 4: Statistical Abstract of Israel 2015, Table 7.6, 2013 Totals. These religiosity categories are available for the Jewish population only. Survey data for religiosity includes all observations age 20 or over (8 excluded). 6: Statistical Abstract of Israel 2015, Table 5.27, 2013 Total (mean). Median is midpoint between 5th and 6th deciles. Survey data represents midpoint of SES categories.

TABLE A2  
BALANCE BY SUBTREATMENT<sup>a</sup>

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

<sup>a</sup>Includes only individuals for whom we have the 2015 vote outcome. Standard deviations in brackets in Col 1. Standard errors in parentheses in Cols 2-11. Each entry in Cols 2-11 is derived from a separate OLS regression where the explanatory variable is an indicator for treatment. +: mid-point of SES income categories.

TABLE A3  
BALANCE ACROSS SUBTREATMENTS<sup>a</sup>

		Assigned to treatment		Complied with treatment	
		Treatment vs. Control (1)	Treatment vs. Other Subtreatments (2)	Treatment vs. Control (3)	Treatment vs. Other Subtreatments (4)
Asset treatment	F	0.91		1.55	
	<i>p</i> -value	0.591		0.044	
	N	1286		1113	
Late Divest	F	0.97	0.83	1.44	0.75
	<i>p</i> -value	0.499	0.702	0.081	0.798
	N	960	990	843	817
High Allocation	F	1	0.87	1.41	0.66
	<i>p</i> -value	0.465	0.643	0.092	0.893
	N	795	990	720	817
Voucher	F	1.29	1	1.64	0.89
	<i>p</i> -value	0.162	0.464	0.03	0.617
	N	489	990	464	817
Palestinian Stock	F	0.76	0.64	1.22	0.7
	<i>p</i> -value	0.784	0.907	0.215	0.857
	N	697	990	614	817
Israeli Stock	F	0.76	0.79	1.07	0.74
	<i>p</i> -value	0.783	0.754	0.375	0.813
	N	692	990	627	817

<sup>a</sup>Each cell is derived from a separate OLS regression where the dependent variable is an indicator for the subtreatment (indicated in the row name) and the explanatory variables include the full list of pre-treatment variables in Table 2. The table reports the F-statistic and *p*-value for the hypothesis that all of the coefficients are 0. Column 1 includes individuals assigned to the relevant treatment group or to the control. Column 2 includes individuals assigned to the relevant treatment group or to other treatment groups. Columns 3–4 repeat these exercises but includes only the (selected) sample of individuals who complied with the treatment (or the control in col 3). The samples includes only the individuals for whom we have the 2015 vote outcome.

TABLE A4  
ATTRITION

	Treatment	Control	Total
Initial assignment	1036	309	1345
Observed vote in March 2015 elections	1009	302	1311
Proportion observed	0.974	0.977	0.975
Observed peace deal attitudes, March 2015	985	292	1277
Proportion observed	0.951	0.945	0.949
Observed economic attitudes, July 2015	854	257	1111
Proportion observed	0.824	0.832	0.826
Observed vote intention, April 2016	735	208	943
Proportion observed	0.709	0.673	0.701

TABLE A5  
EFFECTS OF PAPER VERSUS REALIZED LOSSES<sup>a</sup>

	Full Sample					Risk Averse	
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	2SLS (5)	OLS (6)	2SLS (7)
Treatment	0.052 (0.019)	0.038 (0.020)	0.045 (0.019)				
Divest Before Election		0.039 (0.019)					
Voucher Treatment			0.033 (0.022)	0.020 (0.024)	0.025 (0.021)	0.028 (0.031)	0.037 (0.027)
Divest Before × 1(Price Gain by Mar. 12)				0.067 (0.027)		0.088 (0.033)	
Divest Before × 1(Price Loss by Mar. 12)				0.084 (0.029)		0.126 (0.039)	
Divest After × 1(Price Gain by Mar. 12)				0.055 (0.023)		0.073 (0.030)	
Divest After × 1(Price Loss by Mar. 12)				0.005 (0.024)		0.006 (0.032)	
1(Realized Gain before Election)					0.070 (0.025)		0.090 (0.030)
1 (Realized Loss before Election)					0.076 (0.028)		0.117 (0.036)
1 (Paper Gain before Election)					0.052 (0.022)		0.063 (0.028)
1 (Paper Loss before Election)					0.006 (0.023)		0.017 (0.030)
Strata FE	YES						
Demographic Controls	YES						
R-squared	0.549	0.550	0.550	0.553	0.553	0.574	0.572
Observations	1311	1311	1311	1311	1311	817	817

<sup>a</sup>Dependent variable is vote choice, ordered from Right (0), Center/Other (0.5) to Left (1). Col 4 estimates separate effects according to whether early or late divesters experienced price gains or losses. Col 5 uses the price variables in Col 4 as instruments for whether an agent experienced realized or paper portfolio gains or losses. Cols 6–7 repeat the estimates in Col 5–6 for the sub-sample reporting ex ante median or below willingness to take risks. All regressions include the full set of controls from Table 3, Col 2. Robust standard errors in parentheses.

TABLE A6  
WEALTH EFFECTS<sup>a</sup>

	Ordered Vote Choice		Peace Index		Econ. Policy Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.053 (0.025)	0.044 (0.021)	0.104 (0.058)	0.083 (0.049)	-0.017 (0.052)	-0.003 (0.047)
Below Avg Income	0.001 (0.035)		-0.052 (0.089)		0.175 (0.081)	
Treatment × Below Avg Income	-0.004 (0.039)		0.014 (0.094)		-0.028 (0.089)	
High Allocation		0.016 (0.018)		0.055 (0.042)		-0.045 (0.040)
Strata FE	YES	YES	YES	YES	YES	YES
Demographic Controls	YES	YES	YES	YES	YES	YES
Observations	1311	1311	1277	1277	1111	1111
R-squared	0.547	0.549	0.454	0.455	0.207	0.211

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

TABLE A7  
SUBJECTIVE WELL-BEING AND AFFECT<sup>a</sup>

Sample	All				Inexperienced	
	Mean	SD	Treatment Effect	SE	Treatment Effect	SE
Subjective Well Being Index (OLS)	0.026	[0.727]	0.011	(0.047)	-0.030	(0.060)
Specific Outcomes (Ordered Probits):						
Overall, how satisfied are you with your life? [1-4]	3.057	[0.661]	-0.023	(0.079)	-0.061	(0.101)
On a scale from 0 to 10, how would you rate...						
The overall well-being of you and your family	6.492	[2.100]	0.048	(0.072)	0.026	(0.091)
The happiness of your family	7.618	[1.885]	-0.010	(0.072)	-0.034	(0.094)
Your health	7.777	[1.895]	-0.021	(0.070)	-0.006	(0.093)
The extent to which you are a good, moral person and living according to your personal values	8.558	[1.379]	0.052	(0.071)	0.043	(0.092)
The quality of your family relationships	8.115	[1.765]	0.064	(0.070)	0.012	(0.092)
Your financial security	6.281	[2.304]	0.057	(0.071)	0.053	(0.088)
Your sense of security about life and the future in general	6.564	[2.229]	-0.017	(0.069)	-0.106	(0.089)
The extent to which you have many options and possibilities in your life and the freedom to choose among them	6.795	[2.238]	-0.033	(0.071)	-0.138	(0.090)
Your sense that your life is meaningful and has value	7.724	[2.053]	0.021	(0.071)	-0.096	(0.090)
Observations			1276		818	

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

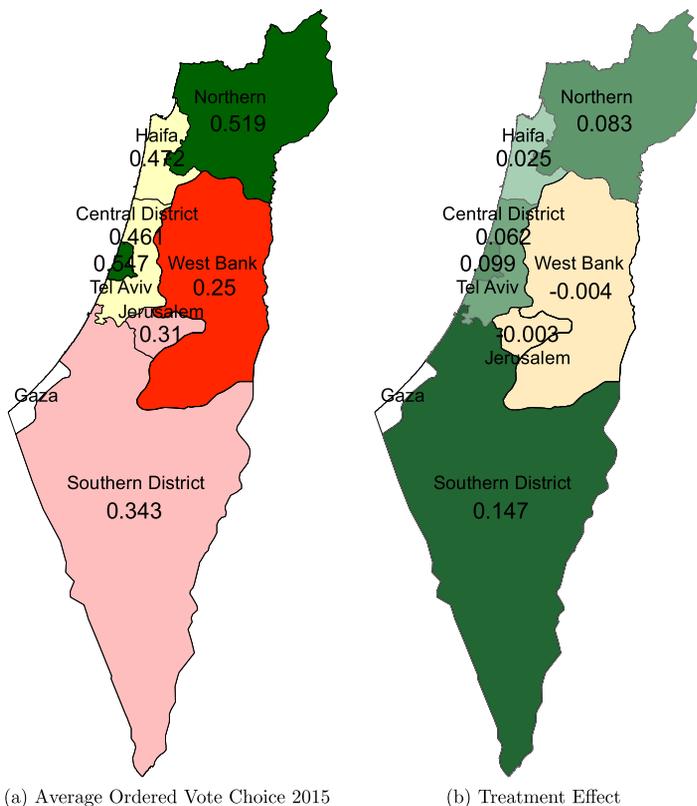


FIGURE A1.—Treatment Effects on the Ordered Vote Choice by Region, 2015 Elections. The ordered vote choice is defined as 0 = Right, 0.5 = Center and 1 = Left.

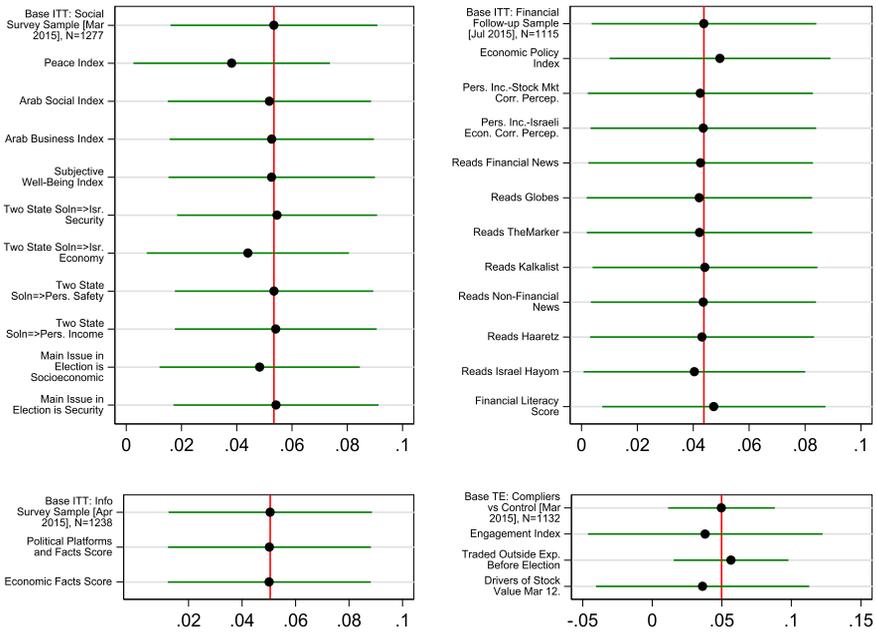


FIGURE A2.—How much of the treatment effect on the vote can be explained by different mechanisms? These figures show how the estimated treatment effect on the ordered vote choice moves when controlling for different potential channels. Each figure represents a different wave of the survey and, hence, a somewhat different sample. The top coefficient in each shows the ITT treatment effect (and 95% confidence interval) without controlling for other outcomes; the subsequent coefficients are after controlling for the indicated variable. All regressions control for the full set of controls and strata fixed effects from Table 3, column 2.

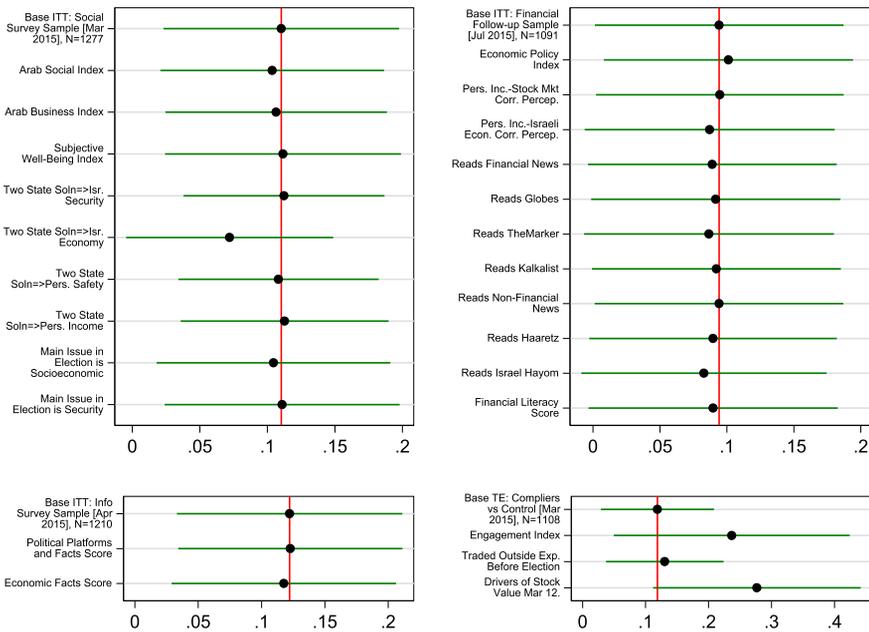


FIGURE A3.—How much of the treatment effect on support for peace can be explained by different mechanisms? These figures show how the estimated treatment effect on the peace index in 2015 moves when controlling for different potential channels. Each figure represents a different wave of the survey and, hence, a somewhat different sample. The top coefficient in each shows the ITT effect (and 95% confidence interval), without controlling for other outcomes. The subsequent coefficients are after controlling for the indicated variable. All regressions control for the full set of controls and strata fixed effects from Table 3, column 2.

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