Online Appendix

Do Disasters Affect Policy Priorities? Evidence from the 2010 Chilean Earthquake

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Contents

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1 Appendix A: Ordinal outcome

Table A1 reports the results when using an ordinal instead of a binary outcome. The ordinal variable takes the following values: 0 when the topic is not mentioned as a priority, 1 when it is mentioned as third priority, 2 when it is mentioned a second priority, and 3 when it is mentioned as a first priority. The results are the same regardless of how we construct the outcome.

Table A1: Difference-in-differences (using an ordinal outcome)

	Housing	Infrastructure and Transportation
	(1)	(2)
Exposure*CEP #57 (06/2008)	−0.025	0.035
	(0.080)	(0.067)
Exposure*CEP #58 (11/2008)	0.089	−0.026
	(0.081)	(0.050)
Exposure*CEP #59 (05/2009)	0.079	0.003
	(0.089)	(0.059)
Exposure*CEP #60 (08/2009)	0.003	0.032
	(0.079)	(0.046)
Exposure*CEP #62 (06/2010)	0.349**	−0.021
	(0.107)	(0.044)
Exposure*CEP #63 (11/2010)	0.172	−0.011
	(0.119)	(0.051)
Exposure*CEP #64 (06/2011)	0.203*	−0.027
	(0.095)	(0.047)
Exposure*CEP #65 (11/2011)	0.161*	−0.013
	(0.082)	(0.049)
Exposure*CEP #66 (04/2012)	0.072	−0.050
	(0.091)	(0.054)
Placebo covariates	Yes	Yes
Observations	5946	5946

CEP #61 (10/2009) is the reference category.
Variables not shown: exposure, CEP surveys, and placebo covariates.
*p<0.05; **p<0.01; ***p<0.001

2 Appendix B: Education and health as outcomes

In addition to infrastructure and transportation, I also check whether the disaster affects concerns about education and health since these two topics might be indirectly connected with the consequences of the disaster (i.e., the reconstruction of schools and hospitals). I do not find evidence to claim that the earthquake makes people care more about education or health.

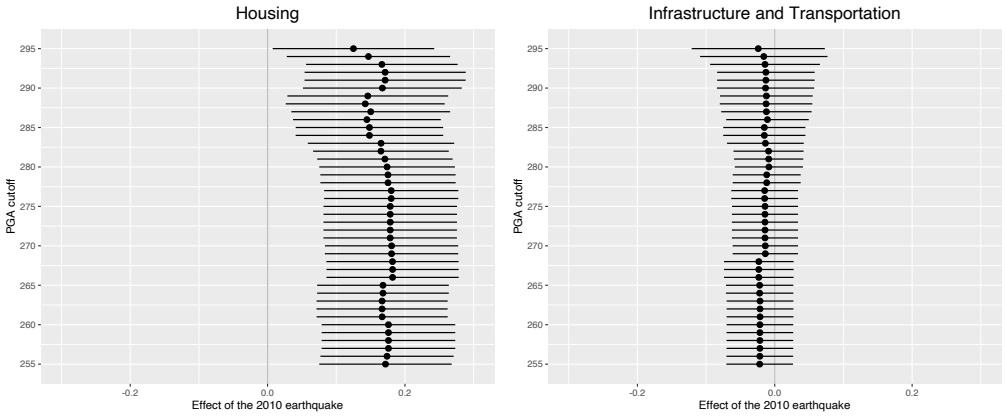
Table A2: Difference-in-differences

	Education	Health
	(1)	(2)
Exposure*CEP #57 (06/2008)	−0.009	−0.033
	(0.049)	(0.055)
Exposure*CEP #58 (11/2008)	0.028	0.072
	(0.056)	(0.061)
Exposure*CEP #59 (05/2009)	−0.012	0.018
	(0.057)	(0.055)
Exposure*CEP #60 (08/2009)	0.064	0.062
	(0.058)	(0.066)
Exposure*CEP #62 (06/2010)	0.071	−0.042
	(0.061)	(0.067)
Exposure*CEP #63 (11/2010)	−0.040	0.002
	(0.057)	(0.057)
Exposure*CEP #64 (06/2011)	−0.001	0.023
	(0.061)	(0.060)
Exposure*CEP #65 (11/2011)	0.021	0.023
	(0.058)	(0.063)
Exposure*CEP #66 (04/2012)	0.029	0.036
	(0.058)	(0.057)
Placebo covariates	Yes	Yes
Observations	5946	5946

CEP #61 (10/2009) is the reference category.
Variables not shown: exposure, CEP surveys, and placebo covariates.
*p<0.05; **p<0.01; ***p<0.001

3 Appendix C: Multiple cutoffs

Figure A1 shows 30 point estimates and 95% confidence intervals obtained when changing the cutoff used to identify exposed counties. In the manuscript I use a peak ground accelaration of 0.275 g (Zubizarreta et al., 2013; Visconti and Zubizarreta, 2018). As robustness check, I use 30 cutoffs going from 0.255 to 0.295. The results are consistent across all the different ways of defining exposure to the earthquake. This disaster only increased concerns about housing and has no effect on infrastructure and transportation.



(a) Housing (b) Infrastructure and Transportation

Figure A1: Effect of the earthquake using 30 different cutoffs for the PGA

4 Appendix D: Sample characteristics

Tables A3 and A4 compare the descriptive statistics of three placebo covariates (i.e., individual characteristics not affected by exposure to the earthquake): age,[1] education,[2] and gender,[3] across two groups: all the subjects available in the survey (without excluding subjects living in partially exposed counties),[4] and exposed and control subjects (the sample used for the DID).

Table A3: Descriptive statistics entire sample

Statistic	Mean	St. Dev.	N
Female	0.54	0.50	15,003
Education	3.68	2.26	15,003
Age	2.56	1.08	15,003

Table A4: Descriptive statistics DID sample

Statistic	Mean	St. Dev.	N
Female	0.55	0.50	5,946
Education	3.38	2.19	5,946
Age	2.56	1.07	5,946

Table A5 provides the standardized differences between both groups to illustrate that they are similar to each other. Standardized differences express the difference in means in standard deviation units and a common rule of thumb to provide evidence of balance (or similarity between groups) is a pooled standard deviation below 0.2 (Silber et al., 2013). I do not find evidence to claim that both groups are different in terms of these key observed characteristics. Thus, the main analysis was not conducted on a group of people that look substantially different from the entire

[1] 1: Less than or equal to 29 years old, 2: 30–44 years old, 3: 45–59 years old, 4: greater than or equal to 60 years old. There are no missing values.

[2] 1: no education or primary education incomplete, 2: primary education complete or secondary education incomplete, 3: secondary education complete, 4: higher education, no college, 5: higher education, college. There are 17 missing values, which are excluded from the sample.

[3] 1: female, 0: male. There are no missing values.

[4] Remember that partially exposed counties were removed to reduce sensitivity to hidden biases (Rosenbaum, 2004; Zubizarreta et al., 2013), the main goal in the design of an observational study.

sample.

Table A5: Comparison between the entire and the DID sample

Statistic	Mean entire	Mean DID sample	Stand. Diff.
Female	0.54	0.55	0.01
Education	3.68	3.38	0.13
Age	2.56	2.56	0.00

5 Appendix E: Partially exposed

The main analysis is based on the idea of extreme-response relationship. I analyze a subpopulation in which the exposure effect is larger to better identify the association between exposure and the outcome (Rosenbaum, 2004; Zubizarreta et al., 2013). Here I use the subjects that were partially exposed as the exposed group. Table A6 shows that, as expected, this group is indistinguishable from the control group: citizens partially exposed to the disaster experienced limited material damage that would drive policy preferences. Therefore, it is a reasonable decision to exclude partially exposed units from the main analysis.

Table A6: Difference-in-differences (partially exposed and control areas)

	Housing	Infrastructure and Transportation
	(1)	(2)
Exposure*CEP #57 (06/2008)	−0.019	0.017
	(0.027)	(0.016)
Exposure*CEP #58 (11/2008)	0.015	0.032
	(0.027)	(0.022)
Exposure*CEP #59 (05/2009)	0.020	0.057**
	(0.031)	(0.019)
Exposure*CEP #60 (08/2009)	−0.014	0.022
	(0.036)	(0.015)
Exposure*CEP #62 (06/2010)	0.011	0.024
	(0.039)	(0.018)
Exposure*CEP #63 (11/2010)	0.013	0.026
	(0.034)	(0.018)
Exposure*CEP #64 (06/2011)	0.029	0.004
	(0.035)	(0.017)
Exposure*CEP #65 (11/2011)	0.041	0.015
	(0.026)	(0.018)
Exposure*CEP #66 (04/2012)	−0.004	0.016
	(0.036)	(0.020)
Placebo covariates	Yes	Yes
Observations	12152	12152

CEP #61 (10/2009) is the reference category.
Variables not shown: exposure, CEP surveys, and placebo covariates.
*p<0.05; **p<0.01; ***p<0.001

6 Appendix F: Continuous indicator of exposure

Table A7 provides the results when using a continuous indicator of exposure (peak ground acceleration) instead of a binary indicator. In this analysis, I do not rely on extreme doses, so no county is excluded from the analysis. The results are the same as when using the main empirical strategy. The earthquake has only made people care more about housing but not infrastructure and transportation.

Table A7: Difference-in-differences (using a continuous indicator of exposure)

	Housing	Infrastructure and Transportation
	(1)	(2)
Exposure*CEP #57 (06/2008)	−0.086	0.046
	(0.140)	(0.097)
Exposure*CEP #58 (11/2008)	0.099	−0.003
	(0.139)	(0.108)
Exposure*CEP #59 (05/2009)	0.244	0.096
	(0.158)	(0.111)
Exposure*CEP #60 (08/2009)	0.075	0.053
	(0.171)	(0.085)
Exposure*CEP #62 (06/2010)	0.538**	0.015
	(0.207)	(0.096)
Exposure*CEP #63 (11/2010)	0.288	0.039
	(0.201)	(0.094)
Exposure*CEP #64 (06/2011)	0.341	−0.055
	(0.175)	(0.091)
Exposure*CEP #65 (11/2011)	0.302*	−0.028
	(0.141)	(0.093)
Exposure*CEP #66 (04/2012)	0.180	−0.050
	(0.179)	(0.105)
Placebo covariates	Yes	Yes
Observations	15003	15003

CEP #61 (10/2009) is the reference category.
Variables not shown: exposure, CEP surveys, and placebo covariates.
*p<0.05; **p<0.01; ***p<0.001

7 Appendix G: Robustness checks

I expand equation 1 presented in the paper to run two extra robustness checks. Table A8 reports the results when adding county fixed effects. Table A9 does not incorporate placebo covariates nor fixed effects. The results are consistent across both specifications. Exposure to the earthquake has only affected concerns about housing.

Table A8: Difference-in-differences (with county fixed effects)

	Housing	Infrastructure and Transportation
	(1)	(2)
Exposure*CEP #57 (06/2008)	−0.028	−0.0003
	(0.037)	(0.026)
Exposure*CEP #58 (11/2008)	0.041	−0.023
	(0.039)	(0.026)
Exposure*CEP #59 (05/2009)	0.076	−0.006
	(0.044)	(0.031)
Exposure*CEP #60 (08/2009)	0.022	0.016
	(0.042)	(0.021)
Exposure*CEP #62 (06/2010)	0.166**	−0.016
	(0.052)	(0.023)
Exposure*CEP #63 (11/2010)	0.125*	−0.004
	(0.059)	(0.026)
Exposure*CEP #64 (06/2011)	0.137**	−0.022
	(0.044)	(0.024)
Exposure*CEP #65 (11/2011)	0.082*	−0.007
	(0.039)	(0.024)
Exposure*CEP #66 (04/2012)	0.085	−0.027
	(0.049)	(0.028)
County fixed effects	Yes	Yes
Placebo covariates	Yes	Yes
Observations	5946	5946

CEP #61 (10/2009) is the reference category.
Variables not shown: exposure, CEP surveys, and placebo covariates.
*p<0.05; **p<0.01; ***p<0.001

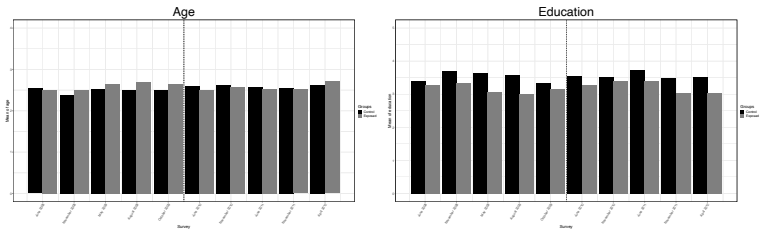
Table A9: Difference-in-differences (no placebo covariates and no fixed effects)

	Housing	Infrastructure and Transportation
	(1)	(2)
Exposure*CEP #57 (06/2008)	−0.014	0.016
	(0.039)	(0.029)
Exposure*CEP #58 (11/2008)	0.038	−0.015
	(0.038)	(0.028)
Exposure*CEP #59 (05/2009)	0.079	0.008
	(0.042)	(0.031)
Exposure*CEP #60 (08/2009)	0.018	0.030
	(0.042)	(0.024)
Exposure*CEP #62 (06/2010)	0.186***	−0.015
	(0.051)	(0.025)
Exposure*CEP #63 (11/2010)	0.124*	0.002
	(0.058)	(0.027)
Exposure*CEP #64 (06/2011)	0.124**	−0.009
	(0.044)	(0.025)
Exposure*CEP #65 (11/2011)	0.102**	−0.0003
	(0.038)	(0.026)
Exposure*CEP #66 (04/2012)	0.069	−0.020
	(0.047)	(0.028)
County fixed effects	No	No
Placebo covariates	No	No
Observations	5946	5946

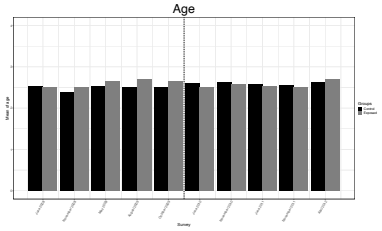
CEP #61 (10/2009) is the reference category.
Variables not shown: exposure, CEP surveys, and placebo covariates.
*p<0.05; **p<0.01; ***p<0.001

8 Appendix H: Covariates across time

In a difference-in-difference design (DID), groups should have a similar composition across time or if they change, it should happen in the same direction for both groups, to respect the parallel trend assumption. Otherwise, these imbalances can translate into bias when estimating the DID since it will not be possible to know whether the estimate is a result of exposure to the disaster or a consequence of group composition. Figure A2 shows the mean for the three placebo covariates for the exposed and control group across the ten surveys.



(a) Female (b) Education



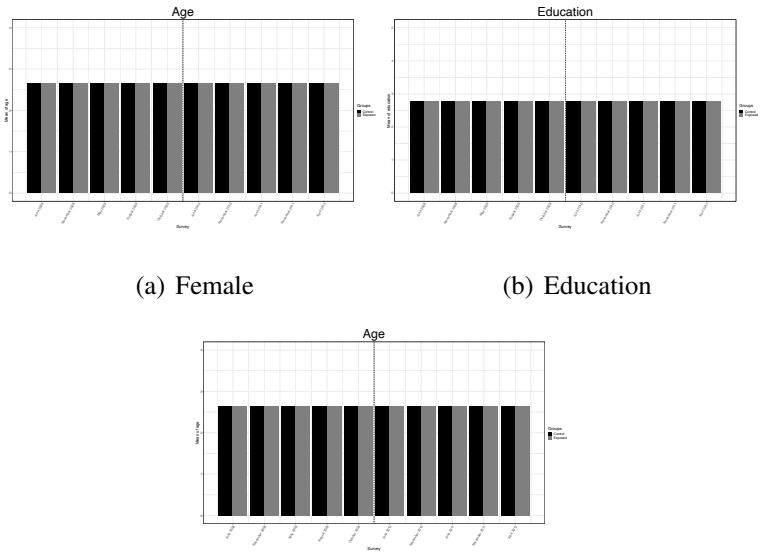
(c) Age

Figure A2: Balance before matching

The results illustrate that groups have pretty stable characteristics across time. In the next section I use matching to achieve an even better covariate balance and to check whether the results are the same.

9 Appendix I: DID and matching

I use matching to construct a synthetic panel that guarantees covariate balance for the control and exposed groups across time, and implement a DID strategy in this matched sample. To achieve covariate balance, and consequently to construct the synthetic panel, I use the designmatch package in *R* (Zubizarreta and Kilcioglu, 2016). In this case, I use fine balance, which focuses on balancing the marginal distributions of the exposed and control groups exactly in aggregate but does not constrain who is paired with whom as exact matching does (Rosenbaum et al., 2007). Put simply, if in the exposed group there are five women and ten men, after using fine balance, in the matched control group there will be five women and ten men, but a woman does not have to be paired to a woman.[5] Figure A3 shows that after matching the groups are balanced across time (and between each other).



(a) Female (b) Education

(c) Age

Figure A3: Balance after matching

Table A10 provides the main results when implementing equation 1 from the main manuscript in the matched sample. The conclusions are the same as those obtained in the main analysis: the

[5] This assumes that there has been no pruning of observations in the exposed group to achieve covariate balance.

earthquake only changed citizens' concerns about housing.

Table A10: Difference-in-differences (using matched sample)

	Housing	Infrastructure and Transportation
	(1)	(2)
Exposure*CEP #57 (06/2008)	0.030	−0.010
	(0.074)	(0.038)
Exposure*CEP #58 (11/2008)	0.070	−0.020
	(0.079)	(0.040)
Exposure*CEP #59 (05/2009)	0.080	0.020
	(0.076)	(0.046)
Exposure*CEP #60 (08/2009)	0.020	0.020
	(0.089)	(0.036)
Exposure*CEP #62 (06/2010)	0.250**	−0.010
	(0.085)	(0.035)
Exposure*CEP #63 (11/2010)	0.110	0.000
	(0.099)	(0.038)
Exposure*CEP #64 (06/2011)	0.150	−0.020
	(0.095)	(0.034)
Exposure*CEP #65 (11/2011)	0.170*	−0.060
	(0.080)	(0.041)
Exposure*CEP #66 (04/2012)	0.050	−0.030
	(0.077)	(0.038)
Placebo covariates	Yes	Yes
Observations	2000	2000

CEP #61 (10/2009) is the reference category.
Variables not shown: exposure, CEP surveys, and placebo covariates.
*p<0.05; **p<0.01; ***p<0.001

10 Appendix J: Alternative explanations

There are other explanations that might sound plausible but that do not have strong support to explain the main findings. First, individuals' decision-making processes might be based on a trade-off between private and public goods: victims will always mention housing first, and only then they will pay attention to infrastructure and transportation. Nevertheless, this explanation does not seem to hold, because the outcome is constructed using a question where respondents select three priorities and not just one. Therefore, they can mention both private and public goods but in a different order. Affected citizens, however, are not more likely to mention public goods at all.

Second, the survey might be providing other options that are more relevant for respondents than infrastructure and transportation. In appendix B and K, I show the effects of the earthquake on all the other issues included as options in the survey, and there is no systematic evidence that this disaster is increasing concerns about any of these other issues. Therefore, there is no support to the statement that respondents are not prioritizing public goods because there are other things that they care more about.

Third, the labels used in the survey might be too general to provide relevant heuristics that respondents can connect with the consequences of the disaster. However, if that were true, we should observe null effects for all of the topics, which is not the case for housing, also a broad label. On the contrary, general or vague issues can be understood as a reference to the effects of the catastrophe due to its devastating consequences. The earthquake primed people, and they were able to connect a broad label like housing to the things they need. Additionally, the reconstruction plan used very similar labels to those used in the survey to identify private and public goods.

Fourth, respondents might want to prioritize urgent issues such as housing and then turn to less pressing concerns. This hypothesis does not seem to hold. First, the disaster happened days before the beginning of the school year in March, and 1 out of 3 schools located in the exposed region were damaged. In addition, the disaster affected the crucial public infrastructure that allows people

to move around the city (e.g., bridges and roads) or get drinking water at home (e.g., drainage) (Government of Chile, 2010). Therefore, both private and public goods were urgently needed by victims, and the survey allowed them to mention both.

Fifth, infrastructure might have already been in disrepair before the disaster, leading respondents to fail to identify its reconstruction as a priority. However, Chile is one of the countries in Latin America with the best quality of overall infrastructure (Fay et al., 2017). Citizens were, in fact, used to high-quality infrastructure before the 2010 earthquake, so we would expect any sudden deterioration to be notable.

Finally, it might be that the government was doing a good job reconstructing infrastructure but a bad one providing housing. The evidence shows that people were frustrated with the overall performance of the government when distributing both private and public goods. In fact, victims labeled the reconstruction process as a "big lie" in 2012, two years after the disaster. In response to these critics, who were joined by opposition politicians, the government said that recovering from the earthquake would take (at least) four years.[6]

[6] Damnificados por terremoto acusan que la reconstrucción es "una gran mentira", El Mostrador, February 14th, 2012.

11 Appendix K: Other outcomes

Tables A11 and A12 report the results when implementing equation 1 from the main paper on the other topics included in the battery of options to prioritize: corruption, crime, drugs, employment, environment, human rights, inflation, the judiciary, poverty, elections, and salaries. I did not include them in the main analysis or in appendix B because they might not be related to the disasters (e.g., reforms to the judicial or electoral systems) or the effect of the disaster might be ambiguous (e.g., in the case of employment a disaster destroys jobs but also generates new ones). None of the issues show a consistent change after the earthquake, which tells us that people were not systematic about what topics they cared less about after the disaster.

Table A11: Difference-in-differences (other outcomes part I)

	Inflation	Corruption	Crime	Human rights	Employment
	(1)	(2)	(3)	(4)	(5)
Exposure*CEP #57 (06/2008)	0.035	−0.043	0.014	−0.026	−0.008
	(0.051)	(0.040)	(0.057)	(0.025)	(0.062)
Exposure*CEP #58 (11/2008)	0.004	−0.095*	0.017	−0.037	−0.049
	(0.050)	(0.044)	(0.073)	(0.024)	(0.063)
Exposure*CEP #59 (05/2009)	0.031	−0.043	−0.039	−0.037	−0.050
	(0.037)	(0.044)	(0.066)	(0.027)	(0.059)
Exposure*CEP #60 (08/2009)	−0.003	−0.048	−0.066	−0.008	0.006
	(0.037)	(0.049)	(0.061)	(0.026)	(0.057)
Exposure*CEP #62 (06/2010)	−0.022	−0.084	0.047	−0.053*	−0.024
	(0.033)	(0.044)	(0.058)	(0.024)	(0.055)
Exposure*CEP #63 (11/2010)	0.071	0.065	0.013	−0.021	−0.164**
	(0.050)	(0.052)	(0.065)	(0.021)	(0.059)
Exposure*CEP #64 (06/2011)	−0.027	−0.063	0.058	−0.050	0.024
	(0.043)	(0.048)	(0.063)	(0.027)	(0.056)
Exposure*CEP #65 (11/2011)	0.021	−0.071	−0.066	−0.048	−0.033
	(0.048)	(0.047)	(0.064)	(0.026)	(0.053)
Exposure*CEP #66 (04/2012)	−0.005	−0.023	−0.031	−0.046	−0.031
	(0.043)	(0.040)	(0.064)	(0.024)	(0.052)
Placebo covariates	Yes	Yes	Yes	Yes	Yes
Observations	5946	5946	5946	5946	5946

CEP #61 (10/2009) is the reference category.
Variables not shown: exposure, CEP surveys, and placebo covariates.
*p<0.05; **p<0.01; ***p<0.001

Table A12: Difference-in-differences (other outcomes part II)

	Poverty	Environment	Drugs	Salaries	Drugs	Judicial
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure*CEP #57 (06/2008)	0.011	0.012	0.039	−0.056	0.057	0.014
	(0.051)	(0.029)	(0.057)	(0.063)	(0.035)	(0.014)
Exposure*CEP #58 (11/2008)	−0.090	−0.012	0.021	0.031	0.087*	0.008
	(0.067)	(0.033)	(0.048)	(0.063)	(0.041)	(0.015)
Exposure*CEP #59 (05/2009)	−0.052	0.005	0.010	0.031	0.016	0.003
	(0.055)	(0.030)	(0.053)	(0.052)	(0.039)	(0.014)
Exposure*CEP #60 (08/2009)	−0.051	0.003	−0.001	−0.019	0.010	0.010
	(0.065)	(0.032)	(0.052)	(0.060)	(0.041)	(0.014)
Exposure*CEP #62 (06/2010)	−0.039	−0.040	0.033	−0.040	0.0001	0.021
	(0.062)	(0.037)	(0.050)	(0.057)	(0.033)	(0.012)
Exposure*CEP #63 (11/2010)	−0.062	0.003	0.014	−0.036	0.049	0.011
	(0.064)	(0.027)	(0.048)	(0.049)	(0.033)	(0.015)
Exposure*CEP #64 (06/2011)	−0.017	−0.036	−0.012	−0.060	0.037	0.025*
	(0.063)	(0.032)	(0.056)	(0.061)	(0.042)	(0.012)
Exposure*CEP #65 (11/2011)	−0.023	−0.012	0.054	0.043	0.002	0.00001
	(0.071)	(0.032)	(0.052)	(0.053)	(0.040)	(0.013)
Exposure*CEP #66 (04/2012)	−0.012	0.005	−0.002	0.002	0.022	0.010
	(0.060)	(0.033)	(0.059)	(0.067)	(0.042)	(0.015)
Placebo	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5946	5946	5946	5946	5946	5946

CEP #61 (10/2009) is the reference category.
Variables not shown: exposure, CEP surveys, and placebo covariates.
*p<0.05; **p<0.01; ***p<0.001

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