The Impact of Natural Disasters on Education: Evidence from Standardized Testing

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May 31, 2022

Abstract

Few studies have investigated the impact of natural disasters on academic performance. We use an event-study design to estimate dynamic treatment effects of natural disasters on students' performance in standardized tests. We find a significant effect on the performance in mathematics in the period of the disaster. For the performance in Reading Language Arts we find no significant effect.

1 Introduction

Natural disasters are responsible for massive economic damage and due to climate change the frequency of such disasters will increase in most regions (Intergovernmental Panel on Climate Change (IPCC), 2021). Therefore, it is essential to have a good understanding of the consequences. While much research has been done on the macroeconomic consequences of natural disasters, few studies have focused on the impact on education. This study investigates the effect of natural disasters on academic performance.

A causal effect of natural disasters may be driven by school closures (Grewenig et al., 2021) or lowered attendance (Spencer et al., 2016), destroyed infrastructure, and emotional stress (Vogel and Schwabe, 2016). Furthermore, some forms of disasters, e.g. extreme heat, may directly impair cognitive performance (Ramsey, 1995).

To identify dynamic causal effects of experiencing natural disasters, this paper uses an event-study design. In particular, we estimate dynamic treatment effects for up to eight years after initial treatment. As a result, not only short-term but also medium to long-term effects can be found. Since treatment effects are likely very heterogenous, we use the estimator by Sun and Abraham (2021).

This article uses standardized test data on a US county level for grades 3 through 8 in mathematics and reading language arts (RLA) to measure academic performance, covering the school years 2008/2009 to 2018/2019. This measure is very attractive as the test scores are standardized relative to a national reference cohort. Therefore, the outcomes are nationally comparable. For the same period, we obtain data on natural disasters from declarations by the Federal Emergency Management Agency and data on storms from the National Weather Service.

We find strong evidence of a negative effect of disasters on mathematics performance in the same school year. Evidence of a significant effect on RLA, as well as for medium and long term effects is rather weak.

Previous Work: This article contributes to a rich literature on the economic effects of natural disasters. More specifically, this paper contributes to the literature on the impact of natural disasters on education. Previous work has produced mixed results. Some authors find significant effects of natural disasters on the education system, while others find no or only small effects.

Holmes (2002) was among the first to study the effect of extreme weather events on academic achievement. Using a difference-in-differences approach, he finds a significantly negative effect on the performance of North Carolina students. Baggerly and Ferretti (2008) find a statistically significant, but negligibly small effect of the 2004 hurricanes on the performance of students' test scores in Florida. Lamb et al. (2013) study the effects of hurricane Katrina and find a significant impact on mathematics achievement in Mississippi with the greatest effects in nonpoor and nonrural schools. Park et al. (2020) find that cumulative heat exposure negatively impacts PSAT scores. At the same time, they find air conditioning to be very successful at mitigating the negative effect of heat exposure.

Many authors have focused on the role of student mobility as a consequence of natural disasters. Pane et al. (2008) focus on students who switch schools following hurricanes Katrina and Rita and find small negative effects of displacement on test scores. Similarly, Sacerdote (2012) finds sharp declines in test scores one year after the hurricanes, but a substantial improvement three to four years after. This is largely driven by the students' tendency to switch to higher quality schools.

The rest of this paper is organized as follows: Section 2 introduces the data used and presents some descriptive statistics. Section 3 explains the empirical strategy. Section 4 discusses the results

and section 5 concludes.

2 Data

2.1 Natural Disasters

Natural disasters are declared as such by the president, usually upon request by the affected state's governor. Once a disaster is federally declared, states or local governments can receive federal assistance. The Federal Emergency Management Agency (FEMA) provides data on all federally declared natural disasters, beginning in 1953. The data is easily accessible via their API (Turner, 2022). Figure 1 shows the number of declared disasters between 2009 and 2018 across the US. Table 1 shows the types of disasters and their proportion in the FEMA data. Storms make up the largest share of disaster events. Fire and floods also form a substantial part.

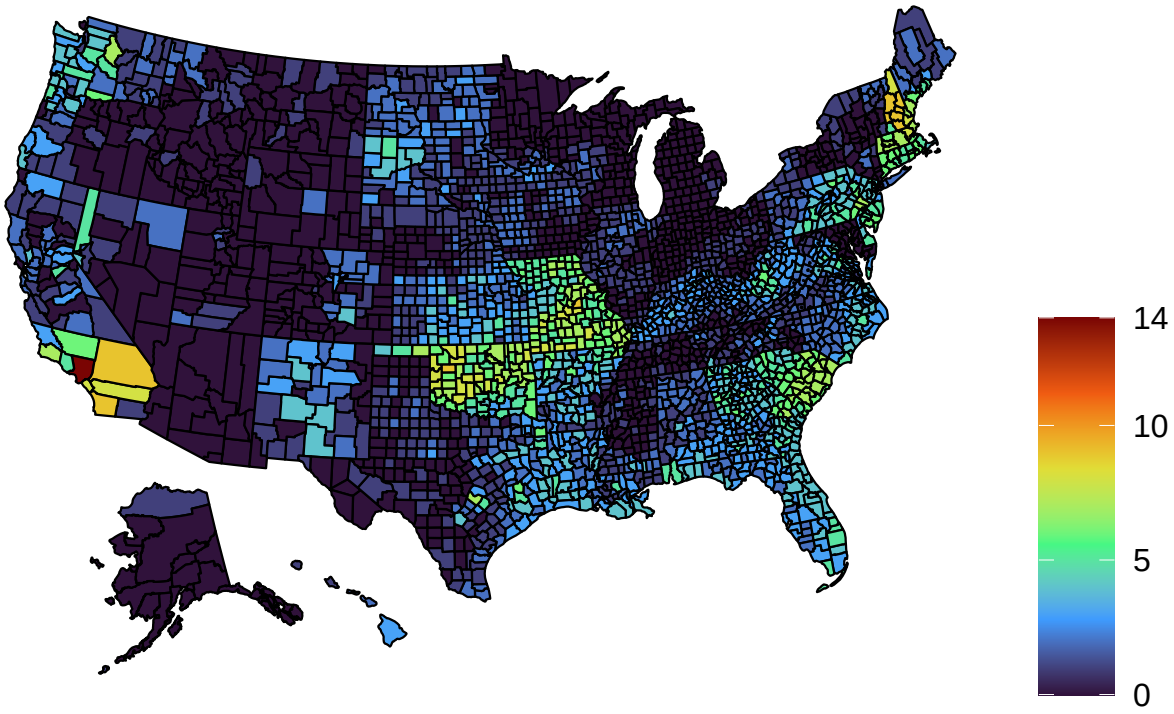

Figure 1: Number of declared natural disasters from 2009 to 2018

Nevertheless, we repeat the analysis on a second disaster dataset from an alternative source as a robustness check. The National Weather Service (NWS) provides data on storm events. In particular, this covers hurricanes, mainly affecting southern coastal regions, tornadoes, and other severe storms. These make up a very large part of all natural disasters experienced in the United States (see table 1). Combined they account for more than 80% of all disaster damage in the FEMA Public Assistance Applicants Program Deliveries database.

We only consider severe storms which are likely to cause substantial damage. Tornadoes can be classified based on estimated peak wind speeds on the Enhanced Fujita (EF) scale (for more details see Mcdonald et al., 2004). Tornadoes with an EF scale of 0 or 1 (wind speeds of up to 110 mph) are characterized as weak. Therefore, we exclude those and only keep tornadoes with an EF scale of at least 2 (wind speeds of at least 111 mph). Unfortunately, the hurricane data does not include a similar measure, but it does include an estimated amount of property damage. We exclude all hurricanes with an estimated property damage of zero. Storm exposure by county is shown in figure

Table 1: Disasters from 2009 to 2018 by type

Variable	N	Percent
Disaster Type	13226	
... Chemical	9	0.1%
... Coastal Storm	12	0.1%
... Dam/Levee Break	3	0%
... Earthquake	19	0.1%
... Fire	886	6.7%
... Flood	2006	15.2%
... Freezing	1	0%
... Hurricane	3094	23.4%
... Mud/Landslide	28	0.2%
... Other	7	0.1%
... Severe Ice Storm	803	6.1%
... Severe Storm(s)	5644	42.7%
... Snow	577	4.4%
... Tornado	114	0.9%
... Toxic Substances	1	0%
... Tsunami	9	0.1%
... Typhoon	11	0.1%
... Volcano	2	0%

2.

2.2 Standardized Testing Data

Data on academic achievement is available from the Stanford Education Data Archive (Reardon et al., 2021). They provide mean test results from standardized tests by county, year, grade and subject among all students and various subgroups (including race, gender, and economically disadvantaged). The most recent version 4.1 covers grades 3 through 8 in mathematics and Reading Language Arts (RLA)[1] over the 2008-09 through 2017-18 school years.

Test scores are cohort-standardized, meaning they can be interpreted relatively to an average national reference cohort in the same grade. This makes the dataset very attractive, as test scores are nationally comparable. For instance, a county mean of 0.5 indicates that the average student in the county scored approximately one half of a standard deviation higher than the average national student in the same grade.

In addition to overall mean test scores, the data includes mean test scores for various subgroups, e.g. by ethnicity. In particular, we consider mean test scores for black, hispanic, female, and economically disadvantaged students to investiagte whether the effects differ by ethnicity, gender, or socioeconomic position. These are only reported if the subgroups' sample sizes are large enough. Thus, the number of observations for some of them is substantially smaller.

The outcomes of interest are overall mean test scores by county, and mean test scores for black, hispanic, female, and economically disadvantaged students. Figure 3 shows boxplots for the five

[1]RLA assesses students' ability to understand what they read and to write clearly.

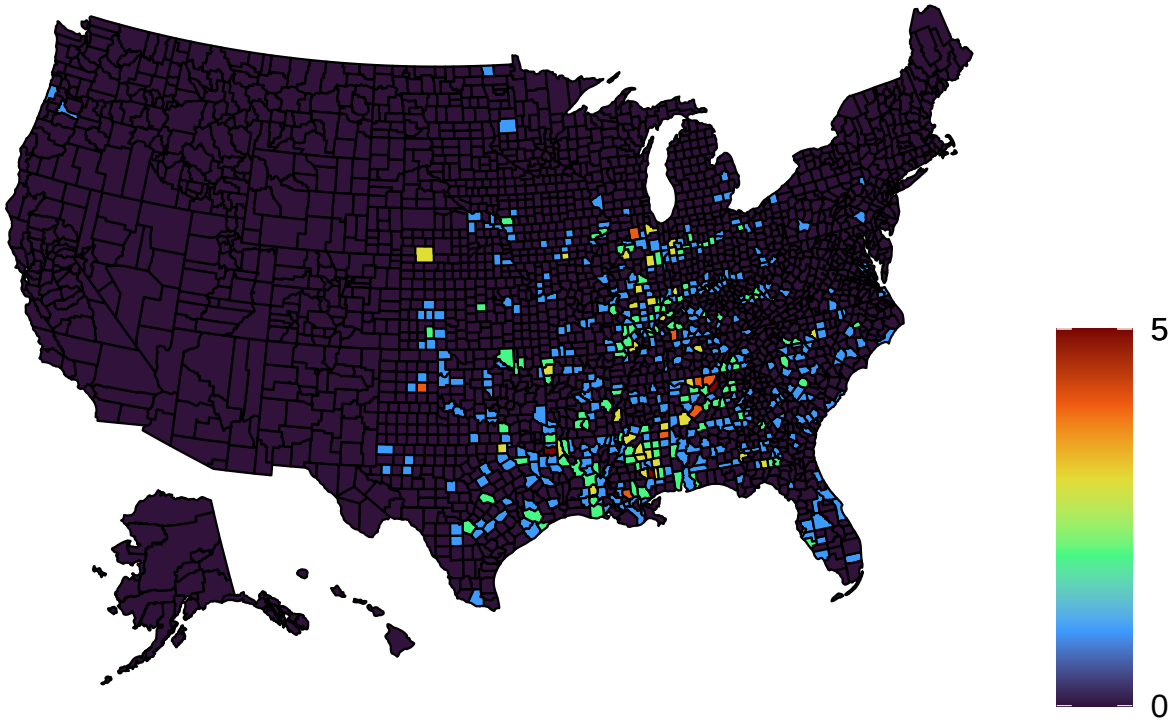

Figure 2: Number of storms from 2009 to 2018

outcomes of interest. All five mean test scores are measured on the cohort standardized scale, that is a given observation measures the distance in standard deviations from the national reference cohort.

Due to the way the scale is constructed, overall test scores are distributed symmetrically around zero, except for a few outliers. The mean scores for black, hispanic, and economically disadvantaged students are shifted downwards by -0.48, -0.281, and -0.283 standard deviations respectively. Female mean scores are slightly above overall mean scores, meaning that female students perform slightly better than male students on average.

Furthermore, the Stanford Education Data Archive maintains a large set of covariates for each county and year. They include variables like the county's median income, unemployment and poverty rate.

Natural disasters should only have an effect on test scores if they occur before the test. Standardized tests are generally administered during spring. We will use March 1st as a cut-off point. Thus, any disaster happening within the same school year before the 1st of March will be considered. School years tend to start in late August or early September with some variation across states. We will use September 1st, meaning any disaster happening between September 1st and March 1st will be counted for a given school year. Disasters occuring in the summer or in the spring after the exams should have much less influence on performance. Thus, we do not consider disasters that occur between March 1st and September 1st.

Each disaster is assigned to a school year as described above. Then, disaster and test score data can be merged by school year and county. This yields a panel data set with six grades and two subjects for each county-year combination.

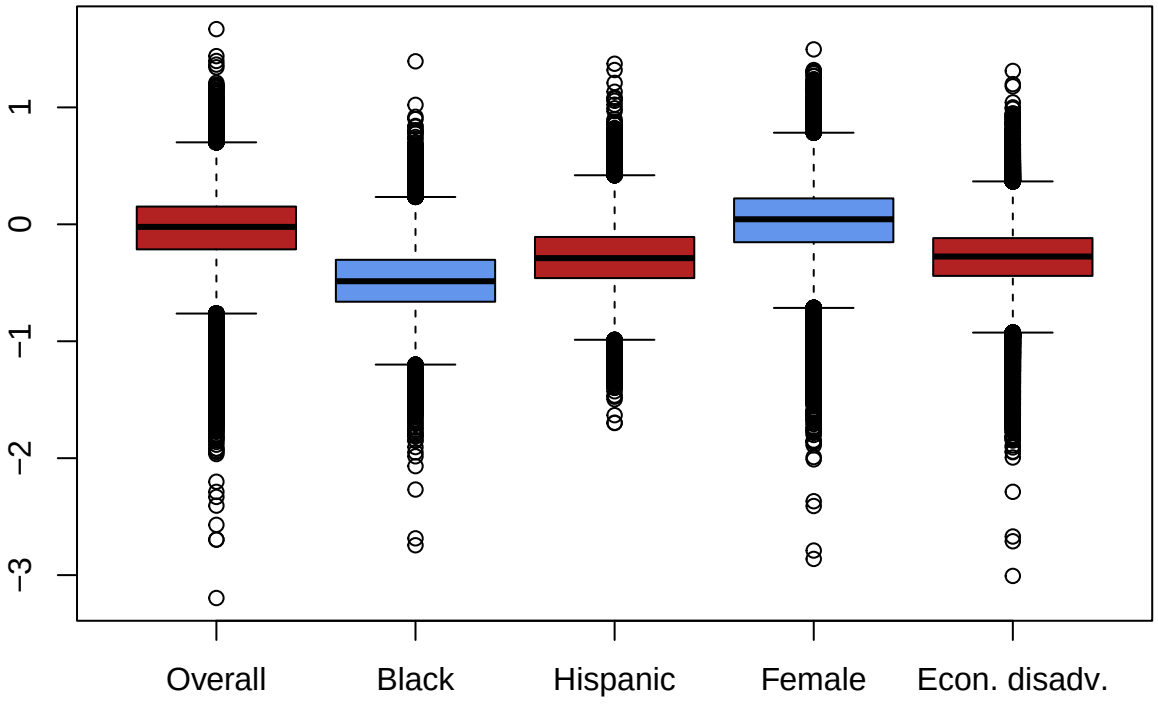

Figure 3: Boxplots of the outcomes of interest

2.3 Assistance Applications

FEMA also provides a dataset on their Public Assistance Applicants Program Deliveries. This contains information on applicants and their recovery priorities, including the amount of damage caused and amount of federal disaster assistance granted. Unfortunately, this data is only available starting in October 2016. Figure 4 shows the total federal assistance awarded to counties.

Based on the overlap between this dataset and our main dataset, it is possible to analyze which counties are affected by a disaster, but do not receive assistance. To do this, we simply check which counties experienced disasters after October 2016, but do not appear in the Public Assistance Applicants Program Deliveries database. In fact, about 62.15% of counties that experienced a disaster in that period did not apply for federal assistance. Table 2 shows the number of disasters and the share of counties that applied for assistance following such a disaster by disaster type.

It may be interesting to see how these counties differ from the ones that did apply. Figure 5 shows boxplots by county application status. Counties that did apply for federal disaster assistance tend to have lower median income, higher poverty rates, and higher shares of single motherhood. Thus, it seems that counties that had to apply for federal disaster assistance were more socially vulnerable in the first place. However, the direction of causality is not clear. Possibly these counties are more vulnerable to natural disasters and are also poorer or more socially vulnerable because of it. Alternatively, counties that are poorer could be more likely to apply for public disaster aid as they have fewer private resources.

It is also interesting whether variation in the federal assistance procedure may be driven by political factors. Visually, democratic votes in the 2016 election (almost coincides with the start of the Public Assistance Applicants Program Deliveries dataset) tend to be lower in counties that applied. That is, counties that applied for federal assistance tend to vote more Republican. Logistic

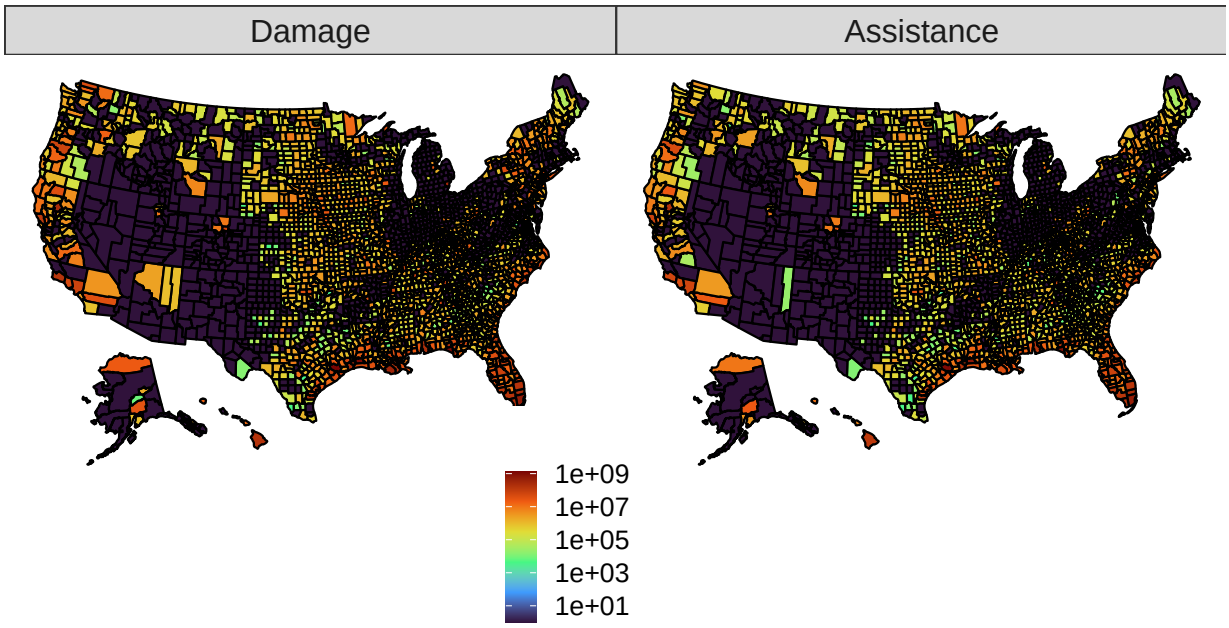

Figure 4: Amount of disaster damage reported by and federal disaster assistance awarded to counties since October 2016 (both in USD)

regression results confirm the visual impression (see Appendix A). While this is not necessarily a causal effect, it could be an indication that a Republican president may be more hesitant awarding disaster assistance to Democratic counties.

Table 2: Share of counties that applied for federal assistance following a disaster by disaster type

	Number of Cases	Applied for Assistance (in %)
Coastal Storm	1	0.00
Dam/Levee Break	3	0.00
Fire	83	2.41
Flood	110	0.00
Hurricane	1115	20.36
Mud/Landslide	1	0.00
Severe Ice Storm	41	0.00
Severe Storm(s)	220	15.00
Tornado	29	79.31
Total	1603	17.78

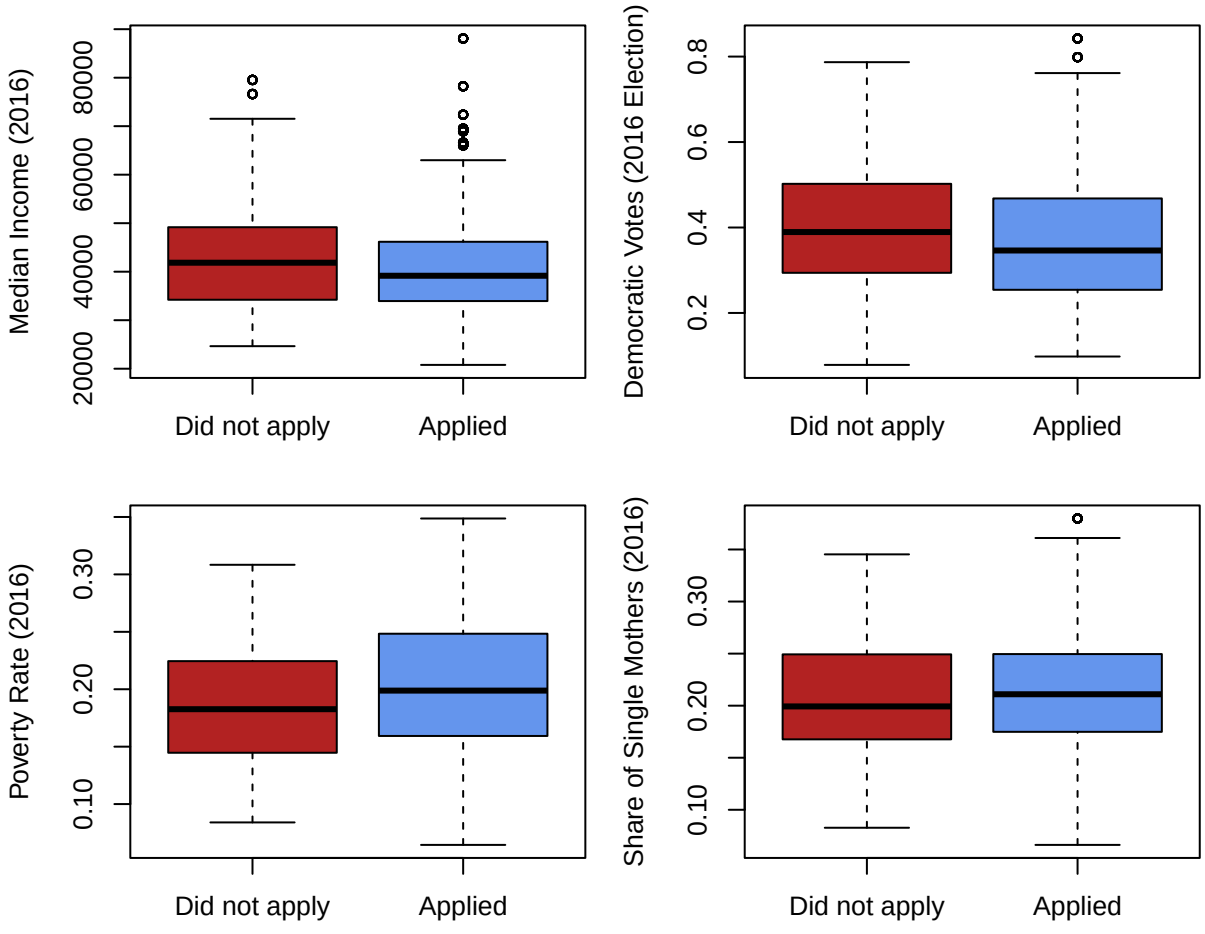

Figure 5: Boxplots by application status

3 Empirical Strategy

3.1 Setting

We employ an event study design to measure the effect of natural disasters on standardized test outcomes. An event study design is a staggered adoption design where units are first-treated at different points in time, and there may or may not be never-treated units (Sun and Abraham, 2021).

Note that treatment must be absorbing, meaning the sequence of treatment indicators $(D_{i,t})_{t=1}^{T}$ must be a non-decreasing sequence of 0s and 1s. In other words, after being treated for the first time a county stays treated. In the present application this means treatment refers to having experienced a disaster rather than experiencing a disaster in that year. This is common practice and does not cause bias due to the conditionally random nature of natural disasters (Deryugina, 2017). Thus, the emphasis lies on cumulative long-term effects rather than instantaneous short-term effects.

In order to identify a causal effect, unobservable determinants of a county's test scores must be unrelated to natural disasters conditional on observable characteristics of that county. The occurrence of natural disasters is plausibly random conditional on location. Furthermore conditioning on the year should account for an increasing trend in natural disasters due to climate change. Thus, independence of mean test scores and natural disasters is plausible conditional on county and year fixed effects.

Consequently, the baseline specification is

$$y_{i,t,g} = \sum_{l=-9}^{8} \beta_l D_{i,t-l} + \alpha_i + \lambda_t + \zeta_g + \varepsilon_{i,t,g} \ , \tag{1}$$

where $y_{i,t,g}$ is the outcome of interest for county i, year t, and grade g. County, year, and grade fixed-effects are given by α_i, λ_t, and ζ_g respectively. $D_{i,t-l}$ is a treatment indicator for county i in year $t-l$. That is, $D_{i,t-l} = 1$ if the county had already experienced a disaster l years ago at time t.

Since we consider the time period 2009-2018, $-9 \leq l \leq 9$, but note that $l = 9$ would correspond to a unit that experienced a disaster in the first period and is therefore always treated. As recommended by Sun and Abraham (2021) and Callaway and Sant'Anna (2021), these units are dropped from estimation. Neither can treatment effects be identified for that group nor are they useful as a comparison group under standard parallel trends assumptions.

Also, we need to drop at least two leads or lags to avoid a multicollinearity problem. A complete set of treatment leads and lags is perfectly collinear with unit and time fixed-effects (for an extensive discussion of this issue see Borusyak et al., 2021, section 3.2). It is common to drop the first relative indicator prior to treatment (i.e. $\beta_{-1} = 0$). This acts as a normalization of treatment relative to the period before treatment. Furthermore, we bin the distant leads, that is we combine the treatment indicators for $l \leq -5$. Thus, equation (1) turns into

$$y_{i,t,g} = \beta_{-5} D_{i,t-5} + \sum_{l=-4, l\neq -1}^{8} \beta_l D_{i,t-l} + \alpha_i + \lambda_t + \zeta_g + \varepsilon_{i,t,g} \ , \tag{2}$$

where $D_{i,t-5}$ indicates treatment for any $l \leq 5$.

It is implausible that the treatment effects are constant in our setting. The extent of disasters varies substantially, and also the level of preparation for such disasters likely displays high variance across counties. Also, some counties may experience additional natural disasters after the first one, while others only experience one. Naturally, we would expect larger treatment effects for the former group.

With heterogenous treatment effects, standard two-way fixed-effects estimators are inadequate (de Chaisemartin and D'Haultfœuille, 2020; de Chaisemartin and D'Haultfoeuille, 2021; Sun and Abraham, 2021). Therefore, we use an alternative estimation procedure by Sun and Abraham (2021), which will be explained below. A similar estimator was introduced by Callaway and Sant'Anna (2021). However, the latter is unable to handle multiple observations for the same unit-period combination. Since we have multiple grades for each county-year combination this would be a severe restriction in our setting. That is why, Sun and Abraham (2021) is better suited.

Treatment adoption varies in time and is therefore assigned in clusters: Counties that are first treated in a given year form a cluster. Following the recommendation by Abadie et al. (2017), standard errors are therefore clustered at the cohort level.

3.2 Interaction-weighted estimator

We utilize the interaction-weighted (IW) estimator proposed by Sun and Abraham (2021) that is robust to treatment effects heterogeneity. The main interest lies on the cohort average treatment effect on the treated (CATT),

$$CATT_{e,l} := \mathbb{E}\left[Y_{i,t+l} - Y^{\infty}_{i,t+l} | E_i = e\right],$$

where $Y^{\infty}_{i,t+l}$ is the counterfactual of being never treated and E_i denotes the first treatment period. Thus, $CATT_{e,l}$ is the average treatment effect on the treated l years after being treated for the first time for the cohort that was first treated in year e.

The estimation procedure consists of three main steps:

1. Estimate $CATT_{e,l}$ using a linear fixed effects specification with interactions between relative period indicators and cohort indicators:

$$y_{i,t,g} = \sum_{e \notin C} \sum_{l \neq -1} \delta_{e,l}(\mathbb{1}\{E_i = e\} D_{i,t-l}) + \alpha_i + \lambda_t + \zeta_g + \varepsilon_{i,t,g}\ , \tag{3}$$

   where C is the set of comparison cohorts. In our case C is the never treated cohort, i.e. $C = \infty$. If there is a cohort that is always treated, i.e. that already receives treatment in the first period, then we need to exclude this cohort. The coeffiecient estimator $\widehat{\delta}_{e,l}$ that we obtain from (3) estimates $CATT_{e,l}$.

2. Weight the estimators by the share of the respective cohort in the sample in that period. Let $\hat{W}^l$ be a weight matrix with element (t, e)

$$[\widehat{W}^l]_{t,e} := \frac{\mathbb{1}\{t - e = l\} \sum_{i=1}^{N} \mathbb{1}\{E_i = e\}}{\sum_{e \in h^l} \sum_{i=1}^{N} \mathbb{1}\{E_i = e\}},$$

   where $h^l := \{e : 1 - l \leq e \leq \max(E_i) - 1 - l\}$ is the set of cohorts that experience at least l periods of treatment.

3. Take the average over all $CATT_{e,l}$ estimates weighted by the cohort shares in the weight matrices. Let $vec(A)$ be the vectorize operator that vectorizes matrix A by stacking its columns and let $\widehat{\delta}$ be the vector that collects $\widehat{\delta}_{e,l}$ for all e and l. Then, the IW estimator $\widehat{v}_g$ for bin g can be written as

$$\widehat{v}_g := \frac{1}{|g|} \sum_{l \in g} [vec(\widehat{W}^l)]^{\intercal} \widehat{\delta}. \tag{4}$$

For a singleton bin $g = \{l\}$, this simplifies to

$$\widehat{v}_g := [vec(\widehat{W}^l)]^\intercal \widehat{\delta}.$$

Under some standard assumptions, $\widehat{v}_g$ is asymptotically normal (for a proof and a detailed description of said assumptions see Sun and Abraham, 2021, Appendix C). Under the additional assumptions of parallel trends and no anticipatory behavior, $\widehat{v}_g$ is consistent, that is it converges in probability to

$$\widehat{v}_g \xrightarrow{p} [vec(W^l)]^\intercal \delta = \sum_{e \in h^l} \mathbb{P}(E_i = e | E_i \in h^l) CATT_{e,l} \ ,$$

where W^l is the probability limit of the weight matrix $\widehat{W}^l$.

We use $\widehat{v}_g$ as an estimator for β_g in equation (2) and we exploit the existing implementation in the **fixest** R package (Bergé, 2018).

3.3 Identifying assumptions

Below we discuss the identifying assumptions.

Parallel Trends: Parallel trends in the sense of Sun and Abraham (2021) refers to the following: $\mathbb{E}[Y^\infty_{i,t} - Y^\infty_{i,s} | E_i = e]$ does not depend on e for any $s \neq t$. That is, the expected temporal difference, i.e. the trend, in the potential outcomes of being never-treated is the same for all treatment timings. A conditional version of the assumption, as in Callaway and Sant'Anna (2021), should definitely hold, as test scores and natural disasters are plausibly independent given location. However, we cannot be sure about the unconditional version required by Sun and Abraham (2021).

Testing for parallel trends is problematic for two reasons: These tests tend to have very low power and they introduce selective inference type issues if inference is conditional on passing a parallel trends test (Rambachan and Roth, 2019). A purely visual inspection of pre-treatment trends does not indicate a violation of the parallel trends assumption (see appendix B). In fact, the trends look almost identical for treated and control (never-treated) units for most cohorts.

No Anticipatory Behavior: There is no treatment effect prior to treatment, that is $\mathbb{E}[Y_{i,e+l} - Y^\infty_{i,e+l}] = 0$ for all e and all $l < 0$. This assumption is plausible as the treatment path is not known. Natural disasters are quasi-random and cannot be reliably forecast more than a few days in advance. Thus, anticipatory behavior is implausible.

Both identifying assumptions should be fulfilled and the IW-Estimator consistently estimates a weighted average of the cohort average treatment effects on the treated.

4 Results

Figure 6 shows estimated dynamic treatment effects and 95% confidence intervals for all students and the four subgroups of interest.

For the period of treatment there is a significant[2] effect of natural disasters on the performance in mathematics. The effect size is between just above zero and -0.01 standard deviations. For all subsequent periods the effect is not significant. There are some point estimates well below zero, but the uncertainty around those is relatively large. For performance in RLA, there are no significant effects.

Note that the number of observed units decreases with the distance in time from treatment. The reason for this is that in order to experience eight treated years, the county has to experience its first disaster very early. Similarly, it has to receive treatment very late to experience more than five years before treatment. As a result, the uncertainty increases with the distance in time from treatment.

For the subgroups we find some surprising results. Black students seem to perform better in RLA in the medium term after a disaster. That is, there are significantly positive results one to seven years after treatment. The effect sizes are substantial: Seven years after treatment the increase in RLA performance goes up to 0.1 standard deviations. In other words, the average black student sees an increase in performance of up to 0.1 standard deviations of the national reference cohort. Also, hispanic students score significantly lower in mathematics in the year following a disaster.

Positive effects of disasters on performance are not unheard of in the literature. In fact, this is somewhat consistent with the findings by Sacerdote (2012). Many students have to switch schools and some may even benefit from attending a higher quality school after the disaster. Black students may disproportionally attend lower quality schools and are therefore more likely to benefit from having to switch schools.

Figures 7 shows the same graphs based on the storm treatment. The results look very similar. In the period of the storm there is a significant decrease in mathematics scores of up to -0.015 standard deviations. For the years following treatment there are no significant effects.

For female students there is a significant decrease in both subjects in the period of the storm. For RLA we even find a significantly negative effect one year after the storm. Similarly, economically disadvantaged students perform worse in the period of treatment and in RLA one year after treatment. The effect sizes range from barely above zero up to -0.015 or even -0.02 standard deviations. For black and hispanic students we do not find any significant effects of the storm treatment.

It could be the case that medium- and long-term effects are largely driven by migration. That's why it may be interesting to have a look at the ethnic composition of the counties relative to initial treatment. Figure 8 shows ethnic shares for the treated counties in relative time.

In the left panel we see that the share of black students decreases in counties that experienced a disaster. This supports the hypothesis that black students disproportionately switch schools after disasters and may explain the positive long term effect as described above. However, the share of black students already decreases before treatment, so this may not be a causal effect of the disaster.

The same plot based on the NWS storm data shows a different picture. Here, all three shares remain somewhat constant until five years fter treatment. Then the share of black students increases, while the share of white students decreases. This suggests that the migration response to storms may be qualitatively different than the response to other forms of disasters. At least for the storms data this does not seem to be a major driver of the results.

[2]Significant is used here in the sense that a confidence interval with nominal coverage of 95% does not include zero, that is a corresponding t-test would reject the null hypothesis of a zero effect at the 5% level.

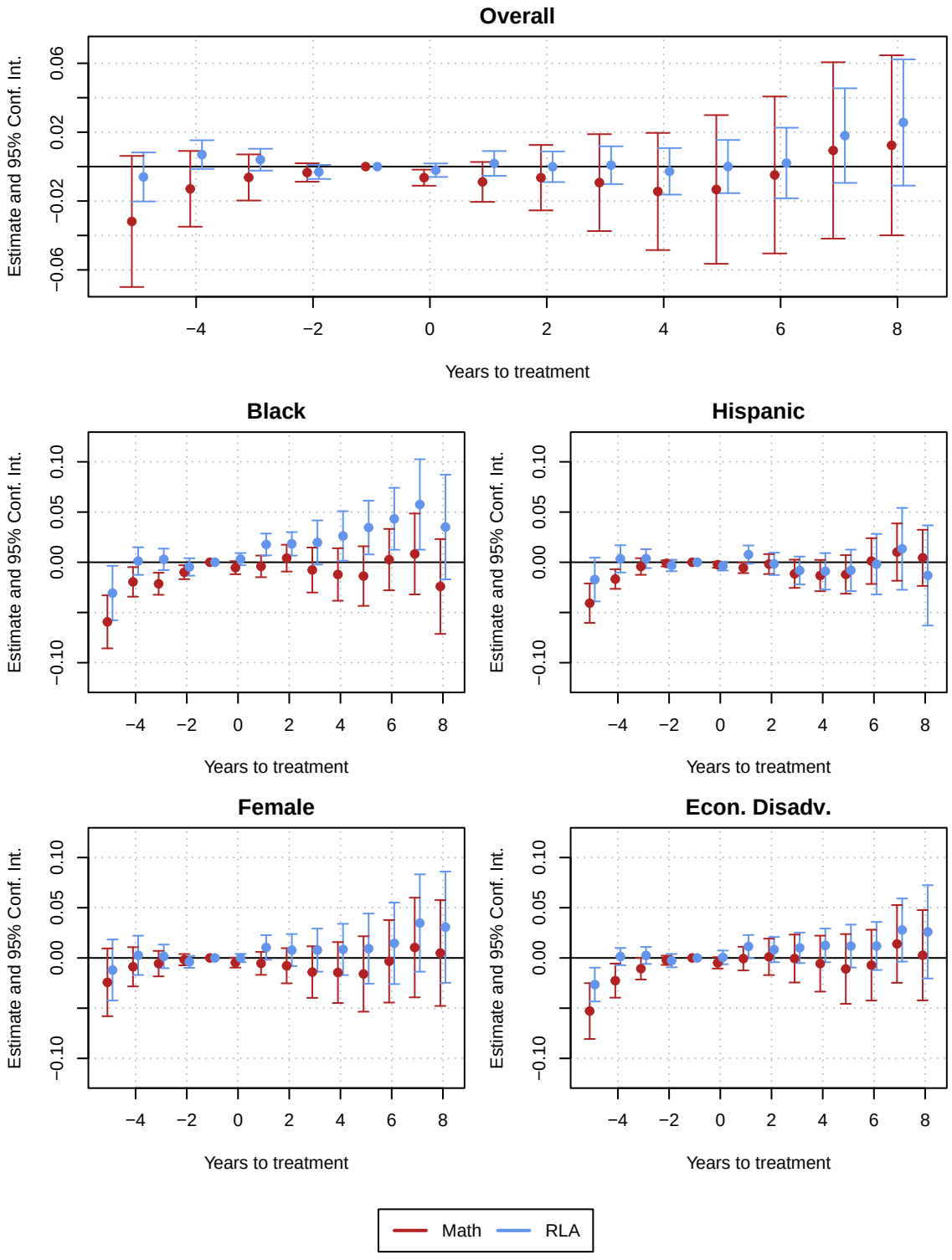

Figure 6: Dynamic Treatment effects in relative time: FEMA disaster data

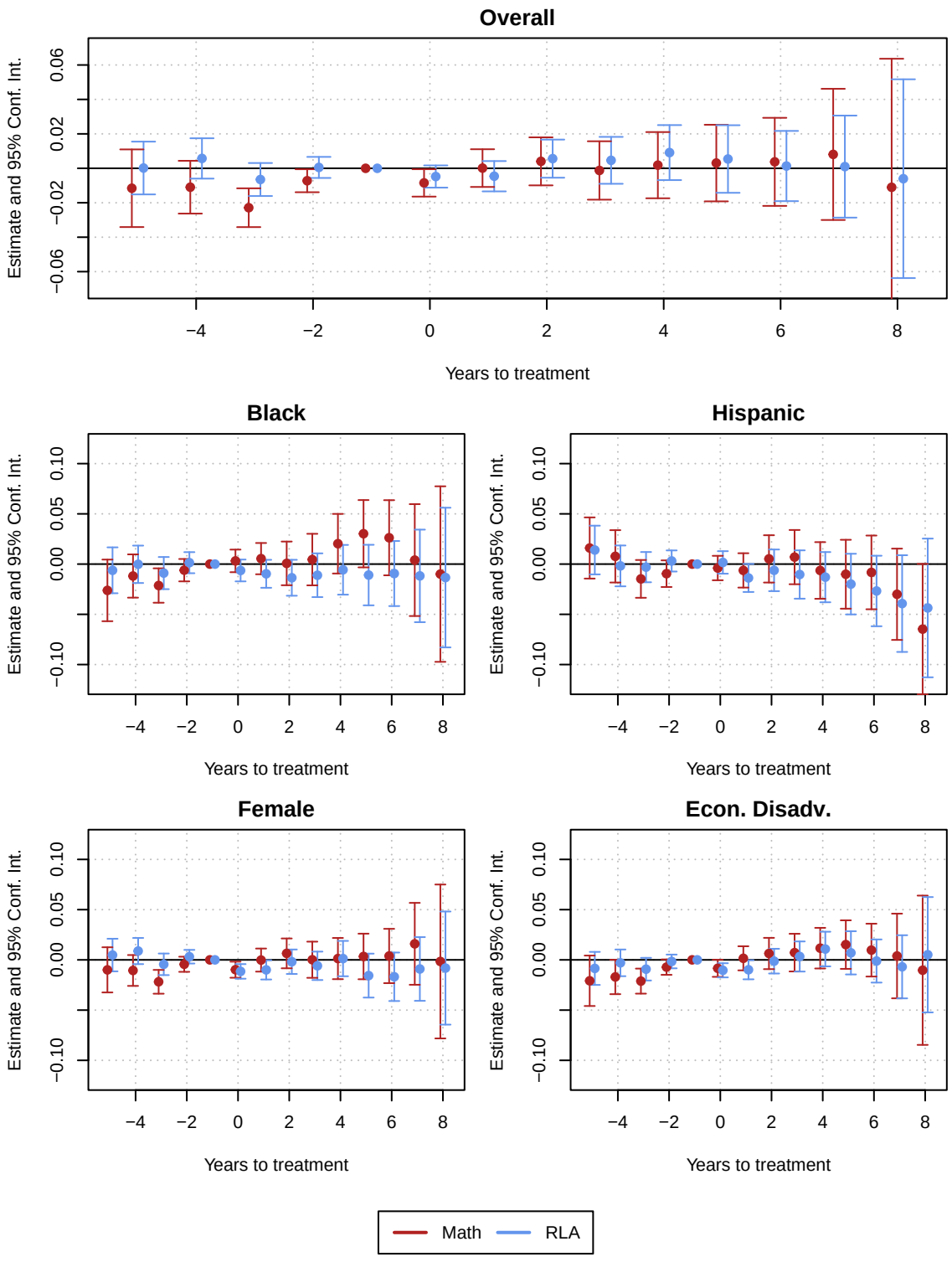

Figure 7: Dynamic Treatment effects in relative time: NWS storm data

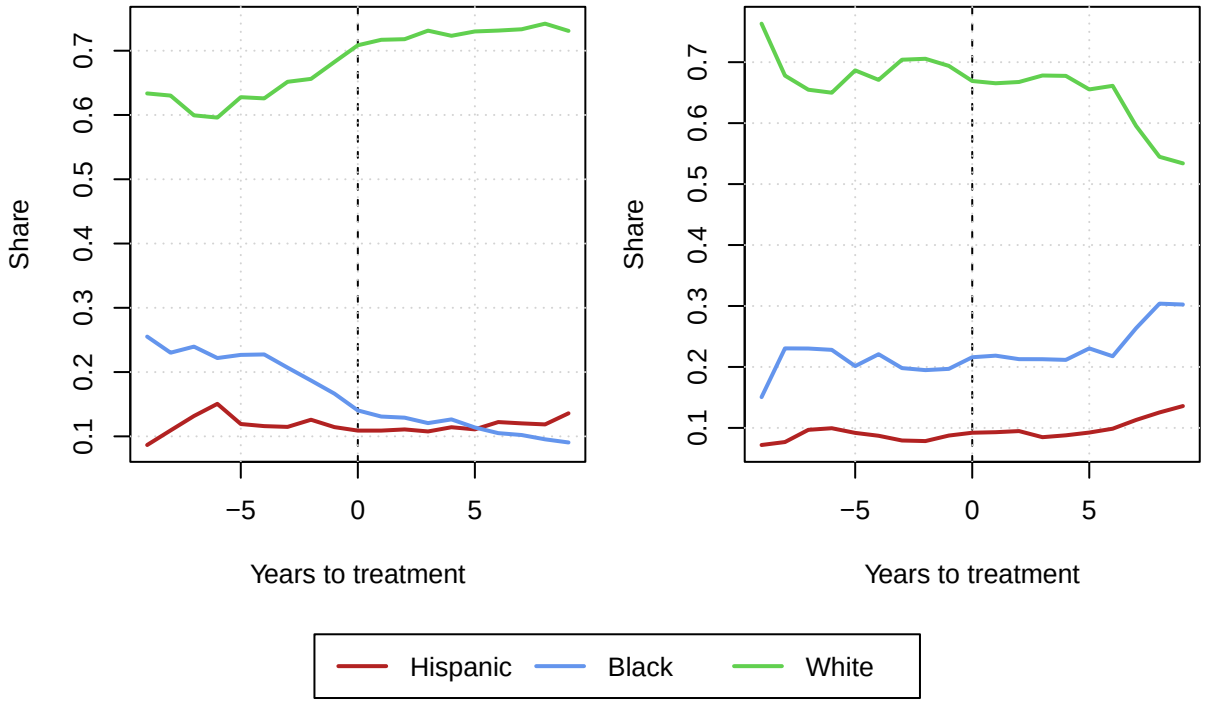

Figure 8: Aggregated ethnic shares by treatment timing based on FEMA disasters (left) and on NWS storms (right)

5 Conclusion

This study estimates dynamic effects of natural disasters on academic performance measured by standardized test results in mathematics and Reading Language Arts (RLA). For both datasets we find a negative effect on the performance in mathematics in the year the disaster occured. The effect reaches up to 0.01 or even 0.015 standard deviations of the national reference cohort. For RLA we find no significant effects on the overall mean score.

Based on FEMA natural disaster data we find that the performance in RLA among black students increases substantially in the years following a natural disaster. The reason could be that black students may disproportionately benefit from having to switch schools after a disaster (Sacerdote, 2012). However, the same model estimated on the NWS storm data does not confirm these findings.

In total, there is strong evidence for a negative effect of disasters on performance in mathematics in the same school year. For RLA, on the other hand, there is no significant effect. Evidence for medium and long term effects is weak. There are some significant effects among minority students, but they do not seem to be very robust, that is they only appear in one of the datasets used.

Mitigating such negative effects should be a concern for policymakers. Even if effect sizes are small, such negative effects can quickly compound in regions that are frequently affected by disasters.

A Additional Results

A.1 Logistic regression for assistance applications

Below we report logistic regression results for the applicant status, that is whether a county applied for federal disaster assistance based on the Public Assistance Applicants Program Deliveries data. More specifically, the applicant variable is 1 if the county experienced a disaster and applied for dederal assistance, that is itappears in the Public Assistance Applicants Program Deliveries database, and 0 otherwise. This is regressed on a few covariates, including the share of democratic votes in the 2016 election. All independent variables are as of 2016.

Table 3: Determinants of Assistance Application

Dependent Variable:	Applied
Model:	(1)
Variables	
(Intercept)	-17.57***
	(2.109)
Share of democratic voters (2016)	-1.258***
	(0.1815)
Median Income (logs)	1.393***
	(0.1876)
Poverty Rate	9.684***
	(0.8676)
Share of single mothers	4.658***
	(0.6640)
Fit statistics	
Observations	9,265
Squared Correlation	0.05654
Pseudo R^2	0.04306
BIC	11,835.2

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

B Pre-Treatment Trends

Here we show plots of aggregated pre-treatment trends to justify the parallel trends assumption. Mean test scores are aggregated by cohort (year of first treatment) and relative time to treatment, and never treated units act as the control group. We only display these plots for overall test scores for both datasets, but not for subgroups. However, the plots for the subgroups look very similar.

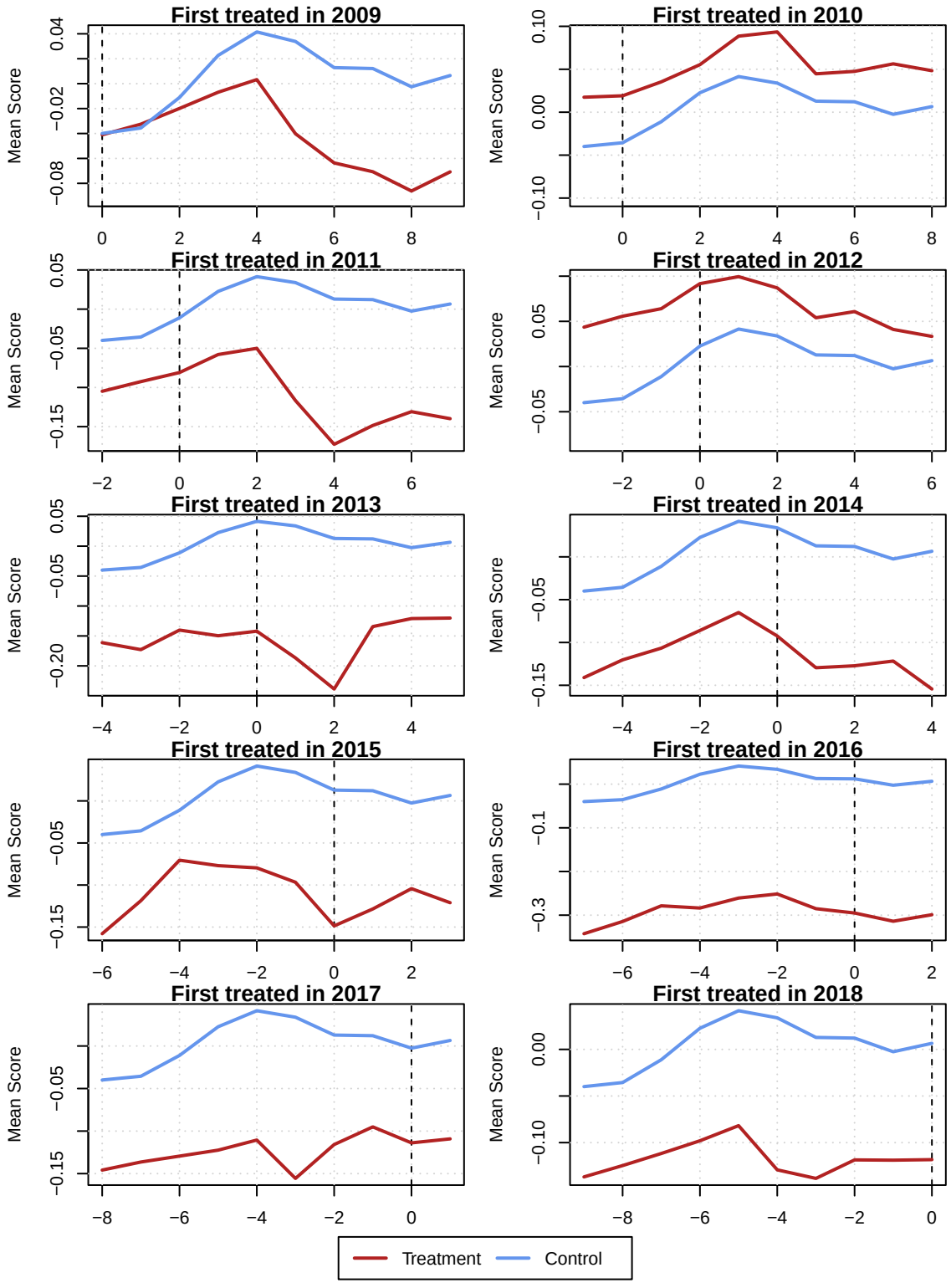

Figure 9: Aggregated mean scores in mathematics based on FEMA data in relative time to treatment

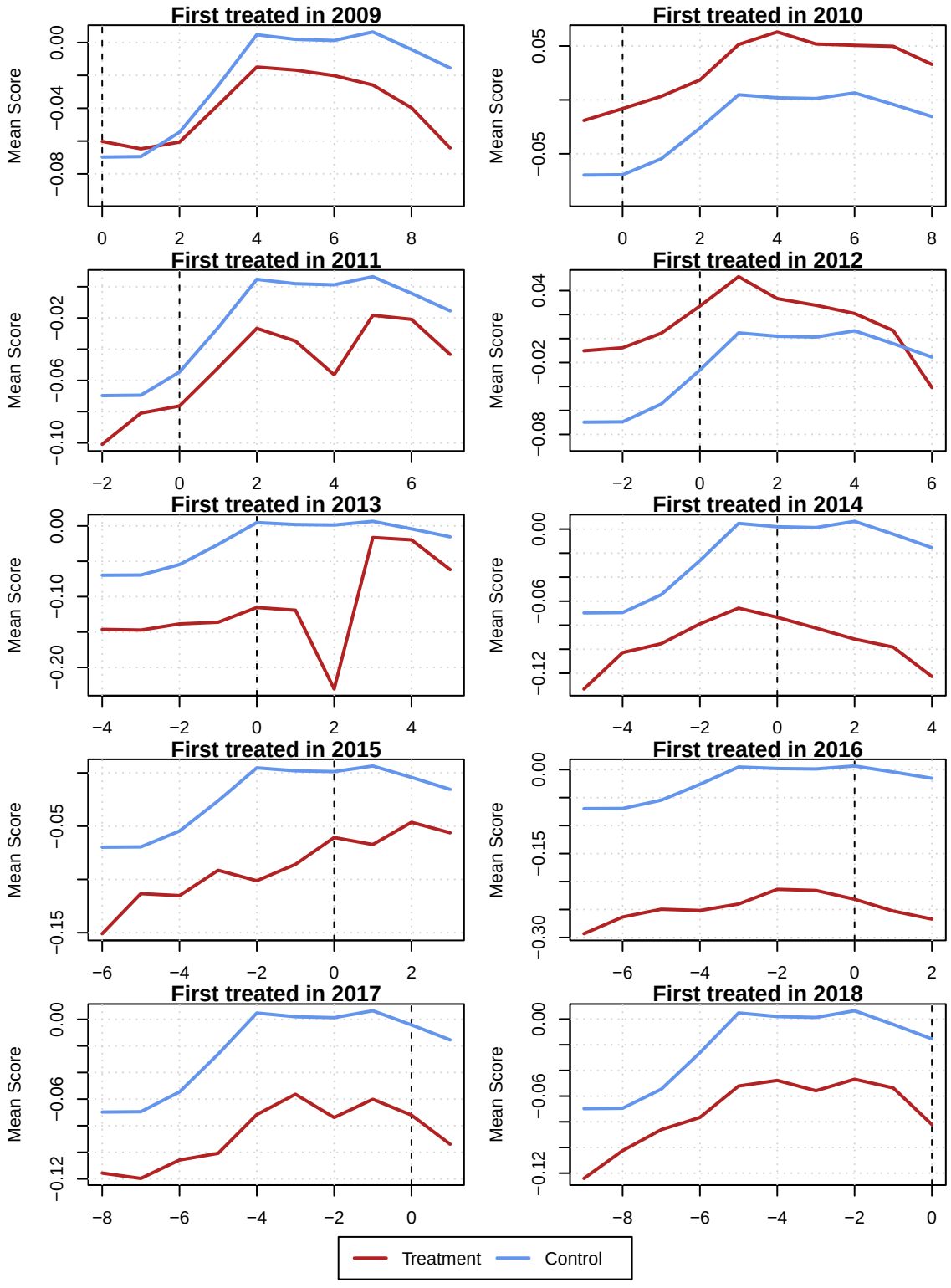

Figure 10: Aggregated mean scores in RLA based on FEMA data in relative time to treatment

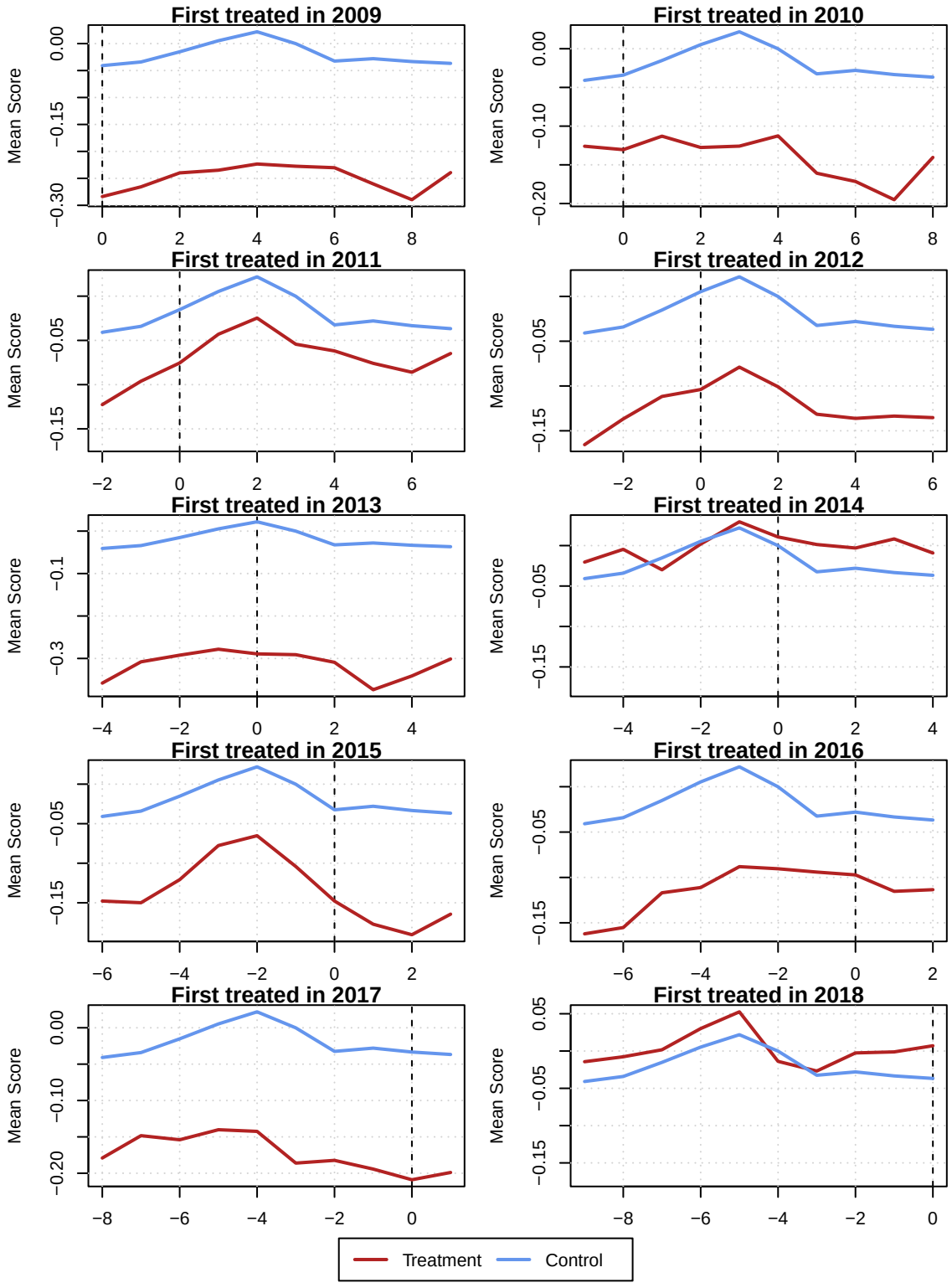

Figure 11: Aggregated mean scores in mathematics based on NWS storm data in relative time to treatment

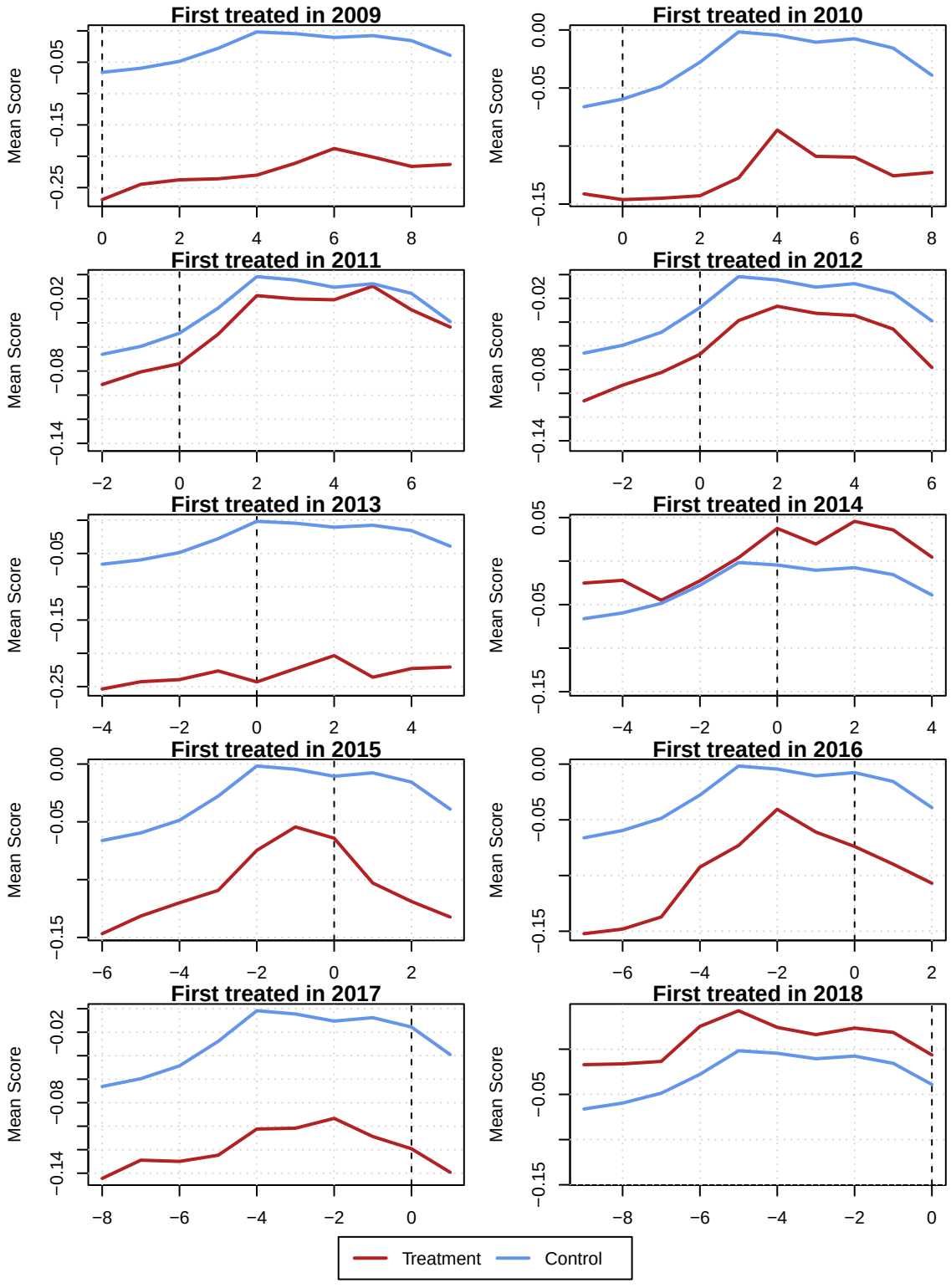

Figure 12: Aggregated mean scores in RLA based on NWS storm data in relative time to treatment

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