Stranger Danger: Parental Attitudes, Child Development and the Fear of Kidnapping

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Abstract

This paper studies the long-term effects of growing up with more or less protective parents. To induce quasi-experimental variation in parental attitudes, we focus on rare but shocking events: nearby child kidnappings. Using geo-localized information from the PSID and a matching strategy (of U.S. counties), we find that the occurrence of a kidnapping causes a decrease in children's cognitive skills and lowers the probability of finishing high school. Turning to mechanisms, we find no evidence that kidnappings make parents or children more neurotic. However, they change parenting style, limiting the time children spend unsupervised and decreasing parental involvement.

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"*[The kidnapping of Adam Walsh] created a nation of petrified kids and paranoid parents. Kids used to be able to go out and organize a stickball game, and now all playdates and the social lives of children are arranged and controlled by the parents.*"
Richard Moran, criminologist at Mount Holyoke College

1 Introduction

There is ample anecdotal evidence that parenting styles have evolved over the past decades (Haidt and Lukianoff, 2018). Kids are now "bubble-wrap children" since they supposedly grow up with less freedom. Their parents are "helicopter parents" who always watch over their shoulder, ready to intervene whenever a problem occurs. Little is known on how these changes in parenting style have impacted children's outcomes later in life. It is challenging to assess these effects because (i) few datasets combine information on parenting during children and outcomes at adulthood and (ii) it is hard to observe variation in parenting style that is arguably as good as random.[1]

We isolate a series of extremely rare, but shocking events, which are likely to have long-lasting effects on parents and to change the environment one grows up in. More precisely, we use child kidnappings (of children younger than eight years old not abducted by family members) in the United States. The idea behind this strategy is that in the late seventies and eighties in the U.S., missing child cases garnered a great deal of news media attention. It developed into a type of moral panic called the "Stranger Danger", which lead to new legislation[2], the creation of the National Center for Missing and Exploited Children, and the Missing Children Milk Carton Program, among others.

Although infrequent, those events may not be random. For instance, child kidnappings could be more likely to occur in places with fewer police patrols, in richer neighbourhoods, or in places where the share of young children in the population is high. This is why we rely on a matching strategy, whereby we match counties which experienced a kidnapping to others that did not. Since kidnappings affect parents' behaviour, this paper provides causal evidence of the effects of growing up in more or less protective environments on child development and outcomes in early adulthood.

We rely on the PSID, which provides a long panel, linking parents and children during childhood and at adulthood. We rely mostly on the Child Development Supplement (CDS) and the Transition into Adulthood Supplement (TAS) to categorize parenting styles and quantify children's cognitive skills and personality traits. We use a restricted version of the PSID with geo-localized information on where families live.

Our main finding is that living in a county where a kidnapping took place significantly

[1] Baumrind (1967) established a classification of parenting styles based on how they score in two dimensions: how responsive they are to children's needs and how demanding they are towards their children. This generates four types of parenting, 'authoritarian' who are not responsive and demanding, 'authoritative' who are responsive and demanding, 'permissive' who are responsive but not demanding and 'uninvolved' who are not responsive and not demanding.

[2] Between 1982 and 1990, the Congress enacted the Missing Children's Act and the National Child Search Assistance Act.

decreases cognitive skills in childhood and lowers the probability of finishing high school by 7.8 percentage points. This effect is concentrated among households in which the head has no college education. When turning to the mechanisms, we find that the effect is not driven by a change in children or parents' personality, but rather by changes in parenting towards a more 'autoritarian' style. Specifically, we find evidence of less parental involvement in their children's schooling, and in children having less time unsupervised (measured through time diaries) for children with lower educated household head (group for which the baseline effects on cognitive skills and probability to finish high school are strongest).

We primarily contribute to the literature on parenting in economics (Mulligan, 1997; Doepke and Zilibotti, 2017) which studies the decisions to adopt a certain parenting style and its consequences on children. Our main contribution is to provide an exogenous variation in the environment one grows up in and the parenting style one will experience. Our second contribution is to provide a long term perspective by linking parenting style in childhood to outcomes in early adulthood.

We also contribute to the literature on human capital formation and development (Heckman, Pinto and Savelyev, 2013; Attanasio et al., 2020; Attanasio, Meghir and Nix, 2020). Following Cobb-Clark, Salamanca and Zhu (2019), we include parenting into the production function of human capital, and rely on the methodology developed by Attanasio et al. (2020); Attanasio, Meghir and Nix (2020) to extract factors from various measurements of parenting. Compared to the literature that has focused on targeted programs for parents, the quasi-experimental design that we use induces changes in the environment in which children grow up, in a more general sense. This paper also contributes to the general discussion on parental transmission of attitudes. Similar to Fernández, Fogli and Olivetti (2004), we identify a shock to the environment in which one grows up. We do not correlate preferences of parents and children, as Dohmen et al. (2012) do for risk preference, but study how certain forms of parenting cause higher or lower achievements.

The rest of the paper is organized as follows: Section 2 describes our different data sources. Section 3 presents the theoretical framework we use to think about how parenting styles and child development relate. Section 4 explains the empirical strategy and provides evidence on its validity. Section 5 presents the method used to extract the factors of interest and provides descriptive statistics on the samples of analysis. Section 6 shows the baseline results, and section 7 gives insights on the mechanisms at play. Section 8 provides some robustness checks, and section 9 shows the results of our placebo tests. Finally, section 10 concludes.

2 Data

2.1 The Panel Study of Income Dynamics

We use the Panel Study of Income Dynamics (PSID) as a source of information on American families. It is composed of a Main Study (MS), itself composed of an individual-level survey and a family-level survey. Additionally, it contains several supplements which occurred at different points in time, among which the Child Development Supplement (CDS) and the Transition into Adulthood Supplement (TAS). The PSID's complex structure yields one of its most valuable characteristics: it interviews parents as well as their children when they are young, and then again when they are older. It allows to observe how parenting, child development and transition into adulthood are intertwined. We present the MS, CDS and TAS below, and provide details on the PSID's structure in Appendix A.

2.1.1 The Main Survey

The PSID Main Study started in 1968 with roughly 5,000 families, selected so as to form a nationally representative sample and an oversample of low-income families. Nowadays, it interviews more than 10,000 families due to adult children joining it when forming their own households.

It is a very large survey which gathers extensive individual and household-level characteristics, including information on educational levels and employment status, as well as on rules in the household, parents' involment in their child's life, child care arrangements, etc.

2.1.2 The Child Development Supplement

The CDS sample was constructed in two steps. First, all the PSID families with children under twelve years of age during the calendar year of 1997 were sampled. Second, up to two children were selected per family. They were then inverviewed first in 1997, and again in 2002.[3]. In 2007, children who were under eighteen were asked to be interviewed again as part of the CDS, and children who were eighteen or older joined the TAS

The CDS gathers a very large amount of information related to the children of the household, such as their cognitive (achievement at school, standardized test scores' results) and non-cognitive (socio-emotional behaviour) skills, and their relationships with their parents and with other children. It also provides very detailed information on their schedule, both on a representative weekday and on a representative day of the weekend, in the Time Diary part of the CDS.

2.1.3 The Transition into Adulthood Supplement

The eligibility criteria for the TAS interviews are (i) to be a member of a PSID family during the year of data collection, (ii) to have responded to the first wave of the CDS, (iii) to be at least eighteen years old and (iv) to no longer be attending high school.

The TAS is conducted every other year since 2005, and gathers information on a very large set of domains which are relevant during one's transition into adulthood. The questions we are mainly interested in relate to cognitive and non-cognitive skills, educational attainment, responsibilities, and relationships.

[3] 84% of the 1997 sample's children responded in 2002.

2.1.4 Geo-Localized Restricted Data

The PSID is geo-localised, the thinest information being at the census-tract level.[4] This is a crucial characteristic, as our analysis relies on the identification of the area in which a family lives and a child grows up. Not only do we need to associate each family to its environment, we also need to know whether a family is potentially affected by a kidnapping event. This is done by matching the place and date of each kidnapping event to the PSID families living in the vicinity of that place at that date.[5]

2.1.5 Sample Selection

We first select all the CDS and TAS individuals as the core of our sample. However, when the CDS was launched in 1997, it interviewed children aged 0 to 12, therefore born after 1985. Hence, restricting the analysis to children of the CDS would exclude from the analysis children born before 1985. Yet, as described in Appendix B, the fear of kidnappings became a reality during the late seventies and early eighties in the U.S. We believe that children born before 1985 might be a high impact group, having been raised during that period when the moral panic called "stranger danger" was most present. As a result, we decide to include in our sample not only all the CDS and TAS individuals, but also all the individuals from the MS who were born from 1975 onwards. Keeping the MS individuals will allow us to perform the analysis on a larger, and potentially high impact sample, at the cost of being restricted in terms of outcomes, given the fact that the MS does not focus on interviewing children, while the CDS does. We describe the final sample on which we perform the analysis after constructing the treatment and the latent factors in section 5.

2.2 Kidnapping Cases

Given the lack of reliable data on kidnapping cases which happened in the United States over our period of interest, we assemble one from CharleyProject.org, NewsLibrary.com, Wikipedia, and the National Center for Missing & Exploited Children online database. We combine these sources and retain the cases for which we have reliable information on place and date of kidnapping, age of the victim(s) and type of kidnapping.

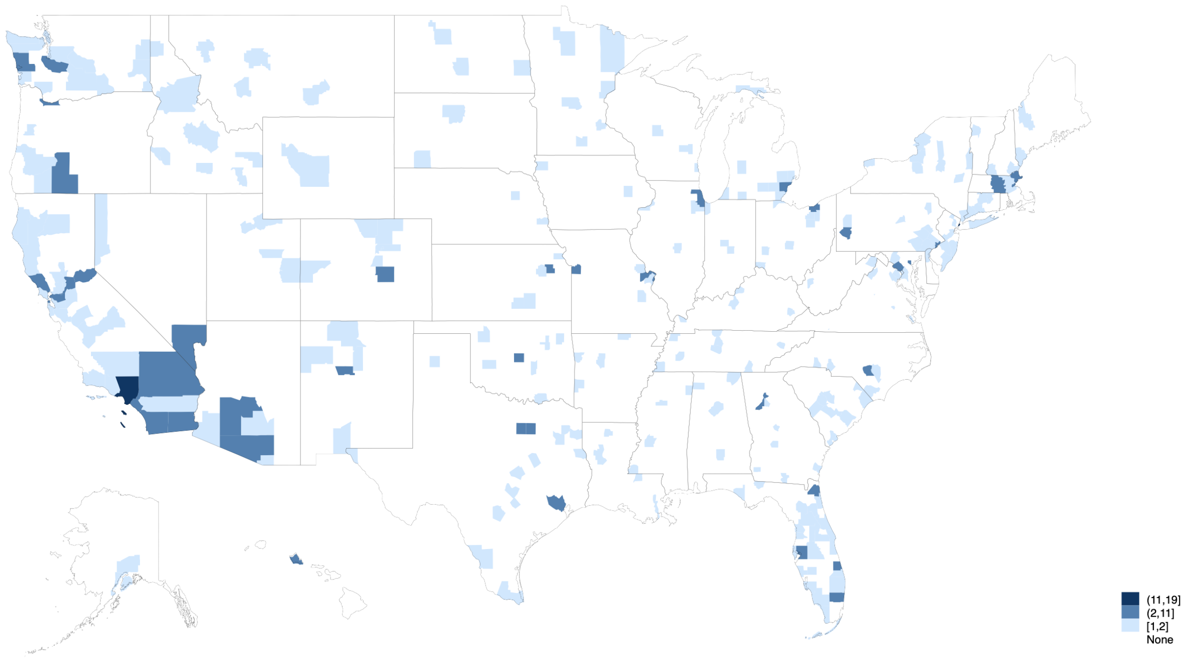
The U.S. Department of Justice's Office of Juvenile Justice and Delinquency Prevention defines stereotypical kidnappings as "abductions in which a slight acquaintance or stranger moves a child at least 20 feet or holds the child at least 1 hour, and in which the child is detained overnight, transported at least 50 miles, held for ransom, abducted with the intent to keep permanently, or killed" (Wolak, Finkelhor and Sedlak, 2016). Relying on this definition, we focus on cases involving victims aged 0 to 12 years old, assuming that these are the cases which are likely to affect parental behaviours the most, and we exclude abductions perpetrated by a family member.

[4] A census tract is defined as a small statistical subdivision of a county or statistically equivalent entity, and it is designed to be relatively homogeneous with respect to population characteristics, economic status and living conditions. Census Tracts generally contain between 1,000 and 8,000 individuals.

[5] The treatment is precisely defined in Section 4.2.

These selection criteria yield a total of 549 kidnapping cases over the period 1970-2010.[6] We map each kidnapping to its county of occurrence, and find that a total of 296 counties experienced a kidnapping over the period, as shown in Figure 1.[7]

Figure 1: Distribution of kidnapping cases across counties.



Notes: This map shows the 549 kidnapping cases of our sample, that is steorotypical kidnappings perpetrated against children aged 0-12 years old between 1970 and 2010. These 549 cases are distributed across 296 counties.

2.3 Counties-Level Characteristics

The first step of our empirical strategy consists in matching treated counties to non-treated counties with similar characteristics in order to define our county-level control group.[8] It requires augmenting our data with county-level characteristics.

We use the 1970 to 2010 NHGIS Decennial Censuses, provided by Manson et al. (2018), and containing information collected at the beginning of each decade on ethnicity, educational level,

[6] The lack of statistics on the number of cases which actually happened over the period makes it difficult to assess whether our database is far from reality or not. However, Wolak, Finkelhor and Sedlak (2016) state that "An estimated 105 children were victims of stereotypical kidnappings in 2011, virtually the same as the 1997 estimate". First, it suggests that we are not capturing the entirety of cases which happen every year. Considering that our database contains an average of 36 cases perpetrated on children aged 0-18 per year, we are capturing around 1/3 of all the cases. This will potentially lead to a misclassification of some treated counties as non-treated, and therefore to an underestimation of the extent to which kidnappings have an impact on people's behaviours. Second, it suggests that the number of kidnappings occurring every year has not increased over time.

[7] Figure 1 shows that kidnapping events are very rare. Our paper therefore speak to the literature on extremely rare events with enormous consequences. Specifically, this literature shows that people who experienced a disaster report unrealistically high probabilities that another disaster will occur again in the near future (see for instance Cameron and Shah (2015) on the effects of natural disasters).

[8] The second part consists in going from a county-level treatment to an individual-level treatment, and then comparing individuals with different treatment status.

labour force status, income and poverty status, housing characteristics, population density, and geographical territories (rural/urban).

Moveover, we add yearly county-level arrest and offense data provided by the Uniform Crime Reporting Program (Kaplan, 2019) to the decennial county-level data. It includes information on the occurrence of a variety of crimes, ranging from burglary to murder.

3 Theoretical Framework

In this section, we first introduce the production function framework we use to think about how parenting styles and personality relate to child development. We then provide a strategy to efficiently perform our analysis in a way that overcomes the challenges to the estimation of this production function.

3.1 Production Function of Skills

We follow a production function approach to child development and skill formation, in which parenting styles, own personality and parents' personality are the main inputs. Precisely, we are interested in a production function of the form:

$$y_i = f\left(P_i, B_i, B_i^p, X_i, \epsilon_i\right) \tag{1}$$

where y_i is, in turn, children' or young adults' socio-emotional and cognitive development, P_i refers to the parenting style that individual i has experienced during his childhood (Cobb-Clark, Salamanca and Zhu, 2019), B_i refers to own personality and behaviour (Todd and Zhang, 2020), B_i^p to parental behaviour, X_i contains individual and household characteristics, and ϵ_i are random shocks to human capital formation.[9]

Our empirical analysis consists in analysing how a random shock − namely a kidnapping event happening in the county of residence during childhood − affects both the inputs and outputs of the production function. Specifically, our baseline analysis estimates the impact of kidnapping events on children' and young adults' cognitive and non-cognitive skills. Then, we seek for insights in terms of mechanisms by estimating the impact of kidnapping events on the different inputs. However, we do not specify the functional form $f(.)$, or decompose the effect of the shock on the outcomes between the different inputs. Note that kidnappings are assumed not to have a direct impact on child development, but only to affect it through the different channels that we explicitly model. We claim that this assumption is fair, given that one of the channels at play is one's own personality.

The main challenge we face when estimating this production function is that cognitive and non-cognitive skills, as well as parenting styles and personality traits are inherently unobservable. The PSID provides a large number of measures relating to the latent factors we are interested in, however, there are two main reasons why we can't simply use these measures as our outcome variables. First, there are too many of them. Second, they are likely to be error-ridden, and

[9]Note that unlike (Heckman, Pinto and Savelyev, 2013; Attanasio et al., 2020), we use skills during childhood as an output and not an input, since we are interested in explaining both skills at childhood and skills at adulthood.

to only imperfectly capture the latent factors that we wish to analyse. As a result, we use factor analysis, which allows to reduce the dimensionality of the model while also accounting for measurement error. It consists in two steps: first, we define a measurement system linking the observable potentially relevant measures to the latent factors, and perform an exploratory factor analysis to select the relevant measures and allocate them to factors. The second step, which is described in details in section 5, consists in estimating the distribution of the latent factors. In that second step, we follow the approach developed by Heckman, Pinto and Savelyev (2013); Attanasio, Meghir and Nix (2020); Attanasio et al. (2020).

3.2 Exploratory Factor Analysis

The basic idea behind Exploratory Factor Analysis (thereafter EFA) is to uncover the underlying structure of a relatively large set of variables from the correlation matrix of that set of variables. It consists in associating elements of a set M of measures to elements of a set F of factors. Following the literature, and for interpretability's sake, we build a dedicated measurement system, that is a system in which each measure m is associated to one factor f only. Formally, the relationship between the measures associated to the j^{th} factor in F and the factor is of the form:

$$m_j = a^j_{m_j} + \lambda^j_{m_j} f^j + \nu^j_{m_j},\ f^j \in F,\ m_j \in M^j \tag{2}$$

where $a^j_{m_j}$ is a constant, f^j is the factor, $\lambda^j_{m_j}$ denotes the loading of measure m_j on factor f^j, and $\nu^j_{m_j}$ are mean-zero error terms.

In this section, we identify the potentially relevant measures in our data (set M), and we group them according to which underlying factor they relate to, e.g. cognitive skills at childhood, socio-emotional skills at adulthood, parental warmth, etc. (sets M_j). Then, we run EFA in order to select the number of factors that should be extracted from the data, given the measures that we observe. Finally, we estimate factor loadings and allocate measures to factors, discarding the ones which do not load on any factor, and the ones which load on multiple factors.

3.2.1 Selection of Measures

To select the measures relating to non-cognitive and personality traits (both for children and young adults), we rely on the Big Five classification.[10] We retain measures on sociability, psychological and emotional well-being, and tendency to experience feelings such as anger, loneliness, and anxiety. For outcomes related to a child's cognitive skills, we use the scores obtained in the Woodcock-Johnson Tests of Cognitive Abilities performed as a part of the CDS interviews, and designed to provide normative scores showing each child's abilities in comparison to a national average for the child's age.[11]

[10] In the psychology literature, the Big Five model is a grouping for personality traits which was first introduced by Goldberg (1981) and then developed by McCrae and Costa (1990). The five personality traits are: openness to experience, conscientiousness, extraversion, agreeableness, and neuroticism.

[11] Note that we will not extract any factor on young adults' cognitive skills. Instead, we decide to focus on outcomes which do not require transformation, such as the probability of graduating high school, of going to college, etc.

To define parenting styles, we rely on Baumrind's classification, which, depending on how responsive and demanding parents are, classifies styles into authoritarian ("too harsh"), permissive ("too soft"), and authoritative ("just right"). Responsiveness mostly captures the extent to which parents are involved in their children' life − by guiding them in making decisions and helping them with their struggles − while letting them explore and decide for themselves. Demandingness focuses on how controlling and expectant of their children the parents are. We retain information on the amount and type of time which parents spend with their children, on whether parents and children discuss the child's interests and future, on the rules which are enforced in the household, and on parental involvement in a child's school life. While the two first sets of measures should provide indications on parental responsiveness, the two other sets should help us characterize the degree of parental demandingness. Note that we also retain a variable capturing parental socio-emotional state. Even though it does not obviously relate to the other selected measures, we think that it relates to parents' personality traits, which can be a relevant dimension to consider.

To account for the fact that the raw measures described above are likely to be correlated with age, we follow Attanasio et al. (2020) and standardize all the measures to remove the effect of age. Details on the standardization procedure can be found in Appendix C.

All in all, we have (i) measures on children' cognitive skills, (ii) measures on children' non-cognitive skills (iii) measures on young adults' non-cognitive skills, and (iv) measures on two main dimensions of parenting styles. We therefore expect to extract one factor for (i), (ii) and (iii), and at least two factors for (iv).

3.2.2 Selection of the Number of Factors to Extract

After gathering measures into groups relating to child development, young adults' outcomes, and parenting styles, and before moving on to estimating the factor loadings and interpreting the factors, we need to decide how many factors to extract from the data. There are several methods available to do so, and we implement four of the most commonly used ones (Kaiser's eigenvalue rule (Kaiser, 1960), Cattell's scree plot test (Cattell, 1966), Horn's parallel analysis (Horn, 1965), and Velicer's minimum average partial correlation rule (Velicer, 1976)). Intuitively, these methods all aim at retaining the factors which explain more variance than a single observed variable.[12] We report the results in Table 1.

While all the methods suggest to extract two factors from the CDS measures on children outcomes, the results are less neat when it comes to parents' and young adults' outcomes. They suggest that the data is rich enough to work with two to six factors for parenting styles, and one or two factors for young adults. We decide to extract one single factor capturing the socio-emotional state of the young adults of the sample, as suggested in section 3.2.1. Moreover, we decide to keep six factors for parenting styles, with the idea in mind that the more factors, the larger the number of dimensions of parenting styles we are able to capture.

[12] The first three methods all rely in different ways on the fact that a factor's eigenvalue is a measure of how much of the variance of the observed measures this factor explains, so as to retain the factors which explain most of the observed variance. The fourth method relies on the partial correlation matrix between the measures of interest so as to retain only the factors consisting primarily of common variance.

Table 1: Exploratory factor analysis to determine the number of factors to retain.

	Number of factors, by method:			
	Kaiser's Eigenvalue Rule	Cattell's Scree Plot Test	Horn's Parallel Analysis	Velicer's MAP Rule
Children	2	2	2	2
Parents	6	4	6	2
Young Adults	2	1	2	1

Note: Kaiser's eigenvalue rule suggests to keep any latent factor with eigenvalue greater or equal to 1. Cattell's scree plot test states that one should keep all the factors which appear before the "elbow" on the scree plot. Horn's parallel analysis compares the eigenvalues generated from the data to the eigenvalues generated from simulated random data of the same size. It consists in ordering factors according to their eigenvalues and, going down the list, retain them as long as the i^{th} eigenvalue from the actual data is greater than the i^{th} eigenvalue from the random data. Velicer's Minimum Average Partial Correlation rule first performs factor analysis and then computes the partial correlation matrix between the variables of interest, removing one factor at a time, and then retain as many factors as the number of steps necessary to get the lowest average squared partial correlation.

3.2.3 The Dedicated Measurement System

With the number of factors to extract from the data in mind, we now turn to identifying the dedicated measures for each factor and perform the exploratory factor analysis.

EFA first requires to compute the matrix of correlations between the selected measures. Given that standard methods performing EFA assume that the measures are continuous whereas most of our measures are ordinal, we use the polychoric correlation technique, which allows to estimate the factor loadings by assuming that measures have a joint (normal) continuous distribution. Then, we estimate the factor loadings in equation (2). Given that unrotated factor loadings are not easily interpretable, we re-weight them in a way to make measures only load heavily on one factor, when it is possible. This procedure is called loading rotation. There are two main types of factor rotation, one allowing for the latent factors to be correlated (orthogonal rotation), the other one permitting correlation between factors (oblique rotation). In our setting, factors are very likely to be correlated, and we therefore use an oblique rotation technique. Specifically, we use direct quartimin rotation (Jennrich and Sampson, 1966).

Tables 2 to 4 display the rotated factor loadings for each measure. Based on the results from the previous section, we have assumed two factors for children, one factor for young adults and six factors for parenting styles.

The rotated factor loadings on children' measures suggest a clear way of allocating the measures to the two factors (Table 2). The standardized test scores all load heavily on the first factor, while the behavioural indices load heavily on the second factor. We call the first factor *cognitive skills* and the second factor *socio-emotional skills*. The sign of the loadings tell us that the higher the score in the first factor, the better a child's cognitive skills, and that the higher the score in the second factor, the more disturbed the child.

Table 3 shows that the rotated factor loadings on measures on parenting styles support six

Table 2: Estimated rotated factor loadings on children' measures.

Measures	Factor 1	Factor 2
Applied Problems standardized score	**0.85**	-0.17
Letter Word standardized score	**0.87**	-0.15
Passage Comprehension standardized score	**0.89**	-0.15
Behavior Problem Index - Externalizing score	-0.21	**0.88**
Behavior Problem Index - Internalizing score	-0.15	**0.87**
Positive Behavior Scale score	0.09	**-0.82**

Note: This table shows the estimated factor loadings on children' measures after quartimin rotation and standardization of the measures. We impose that there be two factors, as explained in section 3.2.2. All the highlighted loadings are large, and therefore all the measures are used in the remaining of the factor analysis.

Table 3: Estimated rotated factor loadings on parenting measures.

Measures	Factor 1	Factor 2	Factor 3	Factor 4	Factor 5	Factor 6
Rules: after-school activities	0.143	0.043	0.068	**0.665**	0.069	0.075
Rules: kind of TV	0.092	0.084	0.067	**0.713**	0.034	-0.111
Rules: amount of TV	0.147	0.196	0.128	**0.666**	0.043	-0.015
Rules: with whom interacts	0.168	0.043	-0.022	**0.693**	0.045	0.050
Do together: arts	0.314	**0.698**	0.204	0.148	0.040	0.015
Do together: board games	0.256	**0.669**	0.125	0.089	0.018	0.107
Do together: reading	0.347	**0.536**	0.447	0.161	0.114	0.084
Do together: building	0.253	**0.678**	-0.001	0.055	0.068	0.009
Do together: sports	0.268	**0.630**	0.089	0.048	0.112	-0.136
Do together: cleaning	**0.762**	0.309	0.087	0.214	0.001	0.158
Do together: dishes	**0.664**	0.305	0.164	0.028	0.056	-0.186
Do together: food	**0.665**	0.445	0.259	0.045	0.051	-0.016
Do together: shopping	**0.581**	0.275	0.111	0.127	0.035	0.031
Do together: washing	**0.754**	0.207	0.155	0.1872	0.023	0.031
Discuss interests	0.107	0.088	**0.728**	0.005	0.215	-0.112
Discuss school	0.160	0.137	**0.822**	0.040	0.151	-0.036
Discuss studies	0.176	0.159	**0.827**	0.117	0.077	-0.057
School: attend PTA meeting	0.1468	0.171	0.09	0.186	**0.447**	-0.215
School: have conference with principal	0.106	0.056	0.003	0.103	**0.553**	0.462
School: have conference with teacher	0.091	0.053	0.107	0.0519	**0.574**	0.241
School: talk to principal	0.024	0.053	0.146	0.053	**0.758**	-0.046
School: talk to teacher	-0.041	0.068	0.192	-0.003	**0.725**	-0.177
Family psychological distress score	0.005	0.009	-0.075	-0.040	-0.057	**0.803**

Note: This table shows the estimated factor loadings on parenting measures after quartimin rotation and standardization of the measures. We impose that there be six factors, as explained in section 3.2.2. All the highlighted loadings are large, and therefore all the measures are used in the remaining of the factor analysis.

groupings of measures. The measures which load highly on the first factor are all related to the time which parents and children spend doing chores together. The higher the factor, the more time is spent doing chores together. We call it *time invested in chores*. Similarly, the measures which load on the second factor have to do with the time which parents and children spend doing entertaining activities together, and again, the higher the factor, the more time is spent doing activities together. We call it *time invested in entertainment*. The third factor gathers the measures related to the discussions that parents and children have about the child's future, interests and studies. The higher the factor, the more common discussions are. We call it *advising*. The measures which load heavily on the fourth factor focus on the restrictions that parents put on children' activities and interactions, and we call it *home monitoring*. Then, the measures which load on the fifth factor have to do with the parents' involvement in their children' life at school. We call it *school monitoring*. Both factors are defined such that the higher the score, the more parents monitor their children. Finally, the measure on parental psychological distress does not enter any of the factors above. Given that we need at least three measures with non-zero loadings per factor in order to have an identified measurement system (Anderson and Rubin, 1956), we exclude it from the remaining of the factor analysis, but keep the variable for the analysis on mechanisms that we perform in section 7.

Table 4: Estimated rotated factor loadings on young adults' measures.

Measures	Factor 1
Non-specific psychological distress scale	**-0.715**
Well-being scale: emotional	**0.777**
Well-being scale: psychological	**0.743**
Well-being scale: social	**0.648**
Mental health scale: worry	**-0.645**
Mental health scale: anxiety	**-0.563**

Note: This table shows the estimated factor loadings on young adults' measures after quartimin rotation and standardization of the measures. We impose that there be only one factor, as explained in section 3.2.2. All the highlighted loadings are large, and therefore all the measures are used in the remaining of the factor analysis.

Table 4 shows the rotated factor loadings on young adults' measures, and suggests that all the measures load heavily on the single factor. The sign of the loadings show that the higher the factor, the higher the young adults' well-being. In the analysis, we reverse this factor so that it is interpreted as a young adult's *socio-emotional distress*.

The second step of factor analysis is to estimate the measurement system described above. Given that it is done separately for the treated and control groups, we first describe the matching strategy which leads to the final sample used in the analysis as well as the definition of our treatment, and then go back to estimating the factor scores in section 5.

4 Matching Strategy and Balancing Tests

One of the reasons that make kidnappings so scary to parents is that *it could happen to anyone*. But can we claim that kidnappings are actually random? One might argue that they are not, because the probability of a kidnapping is related to, for instance, the number of police patrols deployed in the area, the occurrence of other types of crimes, the number of kids playing in the street, the quality of supervision in schools, etc. In other words, counties which have experienced kidnapping events may systematically differ from counties which have not. Yet, failing to account for these differences could confound the results.[13]

4.1 The Matching Procedure

In order to address such concerns, we employ a Mahalanobis matching procedure which aims at finding non-treated matches for every treated county. Specifically, it requires to define a set of variables on which to perform the matching, and minimizes the Mahalanobis distance[14] of all matching variables between treated and non-treated counties. It then selects the closest non-treated counties according to the calculated distance measure.

We perform the matching on (i) the following county-decade variables: share of Whites, share of Blacks or African Americans, share of foreign borns, population density, share of the population living in an urban area, share of families living with less than $\$10,000$ / between $\$10,000$ and $\$14,999$ / between $\$15,000$ and $\$24,999$ / between $\$25,000$ and $\$49,999$ / more than $\$50,000$ per year, and (ii) a series of county-year crime rates (murder, rape, robbery, assault, larceny, burglary).

Importantly, we make the following restrictions. First, assume that counties c^1 and c^2 are located in state s, and that county c^1 is treated in year t. Then, in order to avoid spillover effects, we impose that county c^2 cannot be selected as a control for any treated county in years t and $t+1$. Second, we select three control counties per treated county, and we perform the matching by year.

This strategy defines the treatment at the county level and, starting from a sample of $115,014$ county-year observations among which 410 saw a kidnapping event occur, it yields a sample composed of 408 treated county-year observations and $1,072$ control county-year observations.

4.2 Definition of Treatment

The idea behind our identification strategy is to compare children who grew up in an environment in which a kidnapping happened, to children who grew up in an environment in which no kidnapping happened. It requires to define an age under which a child is considered as young enough to be treated (or control). We choose the age of eight as that threshold, and check that our results are robust to that choice in section 8.1.

Hence, after merging the county-level treatment to the PSID, we define as treated an individual who lived in a treated county when he was eight years old or younger. Similarly, we define as control an individual who lived in a control county when he was eight years old or younger.

Then, we impose that an individual who was treated as a child is treated forever. However, an individual who is selected as a control can be treated later on, in which case he is considered as treated from treatment onwards. Finally, in order to avoid spillover effects, we impose that an individual who lived in a non-treated county located in a state in which another county was treated (see the example in Section 4.1) cannot be selected as a control.

[13] Including county fixed effects may not solve the problem if the confounders vary with time within a county.

[14] We describe the Mahalanobis matching procedure in details in Appendix E.

We end up with a sample of 778 treated and 656 control children over the years, which amounts to $3,547$ treated observations and $2,827$ control observations. We show that the treatment and control groups are balanced in terms of a set of characteristics in section 4.3, and provide descriptive statistics on our sample of analysis in section 5.

4.3 Balancing Tests

The identification strategy relies on the fact that all individuals are as likely to be treated, after controlling for county and year fixed effects. To test this assumption, we perform balancing tests at the individual level. Specifically, we focus on the year of treatment and we estimate the following equation using Ordinary Least Squares (OLS):

$$Y_{ict} = \alpha + \beta T_{ict} + \mu_c + \nu_t + \epsilon_{ict} \tag{3}$$

where Y_{ict} is the characteristics which is being tested, T_{ict} is a dummy variable indicating whether individual i living in county c in year t is treated, μ_c and ν_t are respectively county and year fixed effects, and ϵ_{ict} is the error term. We test $H_0 : \beta = 0$ against $H_1 : \beta \neq 0$. We also run balancing tests at the county level and report the results in Appendix D.

Results of the individual-level balancing tests are reported in Table 5. Columns (1) to (5) investigate the differences in household-level characteristics between the treated and control groups, and columns (6) to (8) focus on differences in individual-level characteristics across treatment status (Treated = 1 versus Treated = 0).

Table 5: Individual-level balancing.

	(1) Age HH Head	(2) Education HH Head	(3) Education HH Wife	(4) Employment HH Head	(5) Nb. HH Members	(6) Gender	(7) Age	(8) Race
Treated	0.115	-0.0473	-0.0493	0.0197	0.0633	0.0260	-0.338	0.0573
	(0.705)	(0.0403)	(0.0364)	(0.0269)	(0.0725)	(0.0402)	(0.296)	(0.0375)
Observations	3,797	3,836	3,845	3,786	3,862	3,862	3,862	3,734
R-squared	0.211	0.254	0.276	0.397	0.279	0.145	0.270	0.408
Mean dep var	34.81	0.757	0.617	0.786	2.199	0.489	3.796	0.196
P-value	0.871	0.241	0.177	0.466	0.383	0.519	0.255	0.128

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table reports the results obtained from regressing a set of household-level (columns (1) to (5)) and individual-level (columns (6) to (8)) characteristics on treatment, controlling for county and year fixed effects. The selected characteristics are measured at treatment year. In the table, 'HH' stands for household. 'Education HH Head' and 'Education HH Wife' are dummy variables equal to 1 if the head/wife of the household holds a high school degree or higher, and to 0 otherwise. 'Employment HH Head' is a dummy variable indicating whether the household head works at the time of the survey. 'Gender' indicates that the individual is a woman. 'Race' is a dummy variable indicating that the individual is black.

Table 5 shows that there exist no observable and statistically significant pre-treatment difference between treated and control individuals, which suggests that treatment is conditionally random.

5 Factor Analysis and Descriptive Statistics

To estimate, the measurement system, we follow the methodology laid out in Attanasio et al. (2020) and Attanasio, Meghir and Nix (2020).[15] Compared to equation (2), the factors are now indexed by treatment status $d \in \{T, C\}$. This allows the distribution of factors to differ between treated and control observations.

$$m_{j,d} = a^j_{m_j} + \lambda^j_{m_j} f^j_d + \nu^j_{m_j},\ f^j \in F,\ m_j \in M^j,\ d \in \{T, C\} \tag{4}$$

This model is based on the following assumptions, the factor loadings and the intercepts $a^j_{m_j}$ and $\lambda^j_{m_j}$ are the same across treatment groups. The error terms $\nu^j_{m_j}$ have mean zero and are independent of the factors and of each other. We set a scale restriction by constraining one loading for each factor to equal 1 (Anderson and Rubin, 1956). We set the location of all factors by setting the mean of the latent factor to 0 in the control group.

We assume that the joint distributions of factors (F_F) follow a mixture of normal distributions.[16]

$$F_F = \tau\Phi(\mu_A, \Omega_A) + (1 - \tau)\Phi(\mu_B, \Omega_B) \tag{5}$$

where A and B index the two distributions and the parameter τ gives the probability for an observation to be sampled from each of them (or mixture weights). The distribution of factors is not observed, while that of the measurements is. In matrix notation, equation (2) can be represented as:

$$M = A + \Lambda F + \Sigma\epsilon \tag{6}$$

where Σ is a diagonal matrix and the matrix Λ has entries zeros for the factors on which measurements do not load. This follows that the distribution of measures (F_M) has the following form:

$$F_M = \tau\Phi(\pi_A, \Psi_A) + (1 - \tau)\Phi(\pi_B, \Psi_B) \tag{7}$$

where $\Psi_i = \Lambda'\Omega_i\Lambda + \Sigma$ and $\pi_i = A + \Lambda\mu_i$ for $i \in \{A, B\}$. The estimation of the system proceeds in three steps.

1. In the first step, we use the modified E-M algorithm of Arcidiacono and Jones (2003) to estimate the parameters of the mixture of normal distributions of the measurements, namely $\tau, \pi_A, \pi_B, \Psi_A, \Psi_B$.

[15] A key difference between our setting and those studied in (Attanasio et al., 2020; Attanasio, Meghir and Nix, 2020) is that we rely on panel data. In particular, we use unbalanced panel as individuals from older cohorts are likely to appear more often than individuals from younger cohorts. We assume that the system is the same for different ages. Since some individuals appear more often, although at different ages, they will be given more weight in the estimations.

[16] Assuming a mixture of normal distributions is much less restrictive than assuming a single distributional form while still being tractable. This ensures that our representation is a better approximation of the underlying distribution.

2. The parameters defining the factors' distribution can be expressed as a function of the parameters defining the measurements' distributions. Relying on the estimates produced in step 1, we estimate a minimum distance estimator of $\Lambda, A, \Sigma, \mu_A, \mu_B, \Omega_A, \Omega_B$.
3. In a third step, we recover the factor values (at the individual level) by minimizing the distance between the actual measurements and the product of the loadings (estimated in step 2) and the factor (being estimated).

Table 6: Estimates of the mixture weights of the joint distribution of the latent factors.

	CDS		**TAS**	
	Control	Treated	Control	Treated
Mixture 1	0.488	0.428	0.657	0.629
Mixture 2	0.512	0.572	0.343	0.371

Note: This table reports for the two samples the weights assigned to each normal distribution in the joint distribution of latent factors (τ in equations (5) and (7)). The number 0.488 should be read as follows: 48.8 percent of the observations used to estimate the factors associated with CDS measures were sampled from mixture 1.

Some results of this estimation procedure are summarized in Tables 6, 7 and 8. Table 6 provides the estimated mixtures for the control and treated groups for both samples (considering that all measures and related factors from CDS and TAS are estimated together). Tables 7 and 8 report the factor loadings for the CDS and TAS samples respectively.

We also report in Table 9 descriptive statistics of the samples used in this paper, namely the MS, the CDS and the TAS. For the relevant samples, we report the mean (together for treated and control observations) of the factors estimated above. We also report the mean age and the share of women for all samples together with the number of children in household, age at the end of school and number of years of education for the main sample.

6 Empirical Results

In this section, we describe our baseline specifications, and report the main results.

6.1 Empirical Specifications

We want to estimate:

$$y_{ict} = \alpha + \beta T_{ict} + X'_{ict}\gamma + \epsilon_{ict} \tag{8}$$

where y_{ict} is the outcome under study, T_{ict} is the individual-level treatment defined above, X'_{ict} is a vector of individual characteristics, and ϵ_{ict} is a standard error term. One threat to achieving a high-quality inference while estimating equation (8) is the relatively high number of variables contained in our database compared to its sample size. Precisely, we perform the analysis on

Table 7: Estimated factor loadings on the CDS sample: children outcomes and parenting styles.

Measures	(1) Cognitive Skills	(2) Non-cognitive Skills	(3) Time Invested in Chores	(4) Time Invested in Entertainment	(5) Home Monitoring	(6) Advising	(7) School Monitoring
LW std score	**1**	0	0	0	0	0	0
PC std score	**1.080**	0	0	0	0	0	0
AP std score	**1.029**	0	0	0	0	0	0
BPI externalizing std score	0	**1**	0	0	0	0	0
BPI internalizing std score	0	**0.877**	0	0	0	0	0
PBS scale	0	**0.852**	0	0	0	0	0
Do together: cleaning	0	0	**1**	0	0	0	0
Do together: dishes	0	0	**0.828**	0	0	0	0
Do together: food	0	0	**0.984**	0	0	0	0
Do together: shopping	0	0	**0.697**	0	0	0	0
Do together: washing	0	0	**0.885**	0	0	0	0
Do together: arts	0	0	0	**1**	0	0	0
Do together: boardgames	0	0	0	**0.833**	0	0	0
Do together: reading	0	0	0	**0.941**	0	0	0
Do together: building	0	0	0	**0.834**	0	0	0
Do together: sports	0	0	0	**0.830**	0	0	0
Rules: after-school activities	0	0	0	0	**1**	0	0
Rules: kind of TV	0	0	0	0	**1.160**	0	0
Rules: amount of TV	0	0	0	0	**1.261**	0	0
Rules: with whom interacts	0	0	0	0	**0.813**	0	0
Discuss interests	0	0	0	0	0	**1**	0
Discuss school	0	0	0	0	0	**1.168**	0
Discuss studies	0	0	0	0	0	**1.200**	0
School: attend PTA meeting	0	0	0	0	0	0	**1**
School: conf. with principal	0	0	0	0	0	0	**2.339**
School: conf. with teacher	0	0	0	0	0	0	**1.304**
School: talk with principal	0	0	0	0	0	0	**2.373**
School: talk with teacher	0	0	0	0	0	0	**1.634**

Note: This table shows the factor loadings estimated on the CDS sample as described in section 5. Columns (1) and (2) display the factors relating to children outcomes, and columns (3) to (7) show the loadings on factors relating to parenting styles.

Table 8: Estimated factor loadings on the TAS sample.

Measures	Socio-Emotional Distress
Psychological distress	**1**
Well-being: emotional	**-1.232**
Well-being: psychological	**-1.007**
Well-being: social	**-0.725**
Mental health: worry	**0.686**
Mental health: anxiety	**0.532**

a sample of 2,959 CDS observations and 3,493 TAS observations, and want to control for a large number of characteristics (county of treatment fixed effects, year of treatment fixed effects, year of interview fixed effects, age at treatment, father's education, mother's education, father's employment status, number of children in the household, gender, race). As a result, we follow Belloni, Chernozhukov and Hansen (2014) and use the Double LASSO algorithm to select for each outcome variable the control variables which should be included in the analysis from a set of 524 dummy variables created based on the control variables described above.[17] It allows to

[17] We describe the algorithm in details in Appendix F.

Table 9: Descriptive statistics.

	Mean	Std. Dev.	N
		MS	
Age	11.907	8.135	67,312
Female	0.501	0.500	67,312
Age end school	19.115	2.685	39,751
Years of education	12.275	2.168	17,253
Nb children in household	2.068	1.478	67,312
		CDS	
Children:			
Age	9.532	4.183	2,959
Female	0.484	0.500	2,959
Years since treatment	8.270	3.963	1,104
Cognitive Skills	0.009	0.846	2,032
Non-cognitive Skills	-0.012	0.924	2,032
Parenting:			
Time invested in Chores	-0.011	0.764	2,032
Time invested in Entertainment	-0.011	0.709	2,032
Home Monitoring	-0.004	0.652	2,032
Advising	-0.028	0.737	2,032
School Monitoring	0.018	0.341	2,032
		TAS	
Age	21.262	2.625	3,493
Female	0.499	0.501	3,493
Years since treatment	18.435	3.660	1,967
Socio-Emotional Distress	-0.002	0.745	3,493

Note: This table reports descriptive statistics from the three main data sources: the main PSID sample, the CDS and the TAS. For all of them, it reports the average age and the share of women of the children in the treated and control group. It also reports statistics for the factors and the main outcome variables.

split the vector of control variables in two and estimate:

$$y_{ict} = \alpha + \beta T_{ict} + X_{ict}^{1'}\gamma + X_{ict}^{2'}\delta + \epsilon_{ict} \tag{9}$$

where $X_{ict}^{1'}$ contains the individual characteristics which are always included as control variables (age, gender, and race), $X_{ict}^{2'}$ contains the characteristics among which the double LASSO algorithm picks additional control variables for the outcome under study, and the rest is defined as above.

We then look at heterogeneous effects of the treatment, focusing on three main dimensions.

First, we look at the differential impact of recent kidnapping events and older kidnapping events, with the idea that the effect might be concentrated during the late seventies and eighties, when the fear of the Stranger Danger was most present in American households. Second, we allow the effect to vary according to a child's family background, first defined by whether the father holds a degree or not, and then defined by whether the mother works or not. Specifically, we estimate:

$$y_{ict} = \alpha + \beta_0 T_{ict} * \mathbb{1}\{M_{ict} = 0\} + \beta_1 T_{ict} * \mathbb{1}\{M_{ict} = 1\} + X_{ict}^{1'}\gamma + X_{ict}^{2'}\delta + \epsilon_{ict} \tag{10}$$

where M_{ict} is the heterogeneity dimension at play (recent date, educated father, working mother), and the rest is defined as above. Testing $H_0 : \beta_0 = \beta_1$ against $H_1 : \beta_0 \neq \beta_1$ will tell whether the effect differs across groups.

6.2 Results on Skills at Childhood

Table 10 reports the results following the estimation of equation (9) on the sample of children. Column (1) shows that treated children perform poorly at the Woodcock-Johnson Tests of Cognitive Abilities compared to their non-treated counterparts. Precisely, treatment is associated with a 0.178 points lower score in cognitive skills. However, column (2) shows that treatment has no significant effect on our measure of non-cognitive skills.

Table 10: Baseline effect of treatment on CDS outcomes.

	(1) Cognitive Skills	(2) Non-cognitive Skills
Treated	-0.178**	0.00971
	(0.0726)	(0.0978)
Constant	-0.106	-0.000594
	(0.180)	(0.0707)
Observations	1,968	2,032
R-squared	0.378	0.061
Mean dep var	0.230	-0.0308

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table reports the baseline results on children' outcomes. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double Lasso for each outcome.

6.3 Results on Outcomes at Adulthood

Table 11 displays the results following the estimation of equation (9) on the sample of young adults observed in the TAS. Column (1) shows that treatment during childhood lowers the probability of ever graduating high school or getting a GED by 7.8 percentage points, and that this difference is statistically significant at the 5% level. However, column (2) does not exhibit any statistically significant impact of the treatment on these individuals' probability of ever attending college, even though the point estimate is negative.

Table 11: Baseline effect of treatment on TAS outcomes.

	(1) Ever graduate HS or get GED	(2) Ever attend college
Treated	-0.0778**	-0.0371
	(0.0318)	(0.0550)
Constant	0.973***	1.050
	(0.0146)	(0.894)
Observations	1,197	1,154
R-squared	0.179	0.558
Mean dep var	0.958	0.789

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table reports the baseline results on young adults' outcomes. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double Lasso for each outcome.

Then, Table 12 shows the baseline results estimated on the sample of young adults observed in the individual section of the MS. We find no statistically significant effect of treatment on any of these outcomes. This result is surprising to us, and goes against our intuition according to which most of the effect would come from events which happened during the seventies and eighties, as explained above.

6.4 Heterogeneity of Results

Results of the heterogeneity analysis are presented in Table 13. The top panel focuses on the outcomes measured at childhood (CDS sample), and the bottom panel on the outcomes measured at early adulthood (TAS sample). In each panel, the first row presents $\hat{\beta}_0$ and the second row $\hat{\beta}_1$ estimated from equation (10). The fourth row displays the p-value from testing the difference between $\hat{\beta}_0$ and $\hat{\beta}_1$.

The CDS panel first shows that the effect of treatment on children' cognitive skills is mostly concentrated on recent kidnapping events. Again, this result goes against our intuition according to which children born before 1985 would be a high-impact sample. However, we see from column (3) and column (5) that family background does not seem to impact the way treatment affects children: a child whose father does not hold any degree is not significantly more or less likely to be affected by the treatment than a child whose father is educated (p-value = 0.513). Similarly, one's mother working status does not affect the extent by which treatment affects cognitive skills

Table 12: Baseline effect of treatment on MS individual outcomes.

	(1) Ever graduate HS	(2) Years of education	(3) Reach higher education
Treated	-0.0158	-0.128	-0.00275
	(0.0209)	(0.113)	(0.0285)
Constant	-0.275***	5.054***	-1.047***
	(0.0807)	(0.353)	(0.0932)
Observations	2,872	2,770	2,771
R-squared	0.305	0.527	0.395
Mean dep var	0.845	13.43	0.560

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table reports the baseline results on outcomes from the individual Main Survey. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double Lasso for each outcome.

(p-value = 0.910). Moreover, we find that the absence of effect on children' non-cognitive skills is consistent across groups.

Turning to the TAS panel of Table 13, we first see in column (1) that the period at which a child is treated does not seem to affect whether or not there will be a long-lasting effect on his or her probability of graduating high school or getting a GED (p-value = 0.605). However, we observe important differences in the treatment effect on one's educational attainment depending on family background. First, column (3) shows that treated children with a low educated father are significantly less likely than treated children with an educated father to ever graduate from high school or get a GED (p-value = 0.0125). Young adults from a low-educated background are 12.2 percentage points less likely to graduate from high school or get a GED than their non-treated counterparts, and the impact of treatment on young adults from a higher educated background is 2.5 times lower (a 4.9 percentage points decrease). Similarly, column (5) shows that treated children with a stay-at-home mother are more likely to suffer from the treatment than treated children with a working mother (p-value = 0.0516): treatment decreases the probability of graduating high school or getting a GED by 9.2 percentage points for young adults with a stay-at-home mother, and by 4.3 for young adults with a working mother.

These results suggest that treatment affects all children in the same way in the relatively short-run: treated children consistently see their test scores worsen, irrespective of their background. However, our results suggest that the long-term impact of treatment depends on family background: children from lower socio-economic backgrounds seem to be more affected in the long-run by the shock than children from higher socio-economic backgrounds.

Table 13: Baseline heterogenous effects of treatment on CDS and TAS outcomes.

	(1)	(2)	(3)	(4)	(5)	(6)
	CDS					
Mechanism	Recent event		Educated Father		Working Mother	
Variable	Cognitive Skills	Non-cognitive Skills	Cognitive Skills	Non-cognitive Skills	Cognitive Skills	Non-cognitive Skills
Treated * $\mathbb{1}\{M=0\}$	-0.0393	0.106	-0.207**	0.0305	-0.177**	0.0313
	(0.0995)	(0.158)	(0.0915)	(0.115)	(0.0758)	(0.0954)
Treated * $\mathbb{1}\{M=1\}$	-0.227***	-0.0221	-0.153**	-0.00988	-0.186**	-0.0719
	(0.0722)	(0.106)	(0.0748)	(0.100)	(0.0917)	(0.132)
Constant	-0.0997	0.000276	-0.0871	-0.000355	-0.107	-0.000139
	(0.181)	(0.0709)	(0.185)	(0.0707)	(0.179)	(0.0709)
P-value	0.0280	0.452	0.513	0.654	0.910	0.273
Observations	1,968	2,032	1,968	2,032	1,968	2,032
R-squared	0.381	0.062	0.378	0.061	0.378	0.062
Mean dep var	0.230	-0.0308	0.230	-0.0308	0.230	-0.0308
	TAS					
Mechanism	Recent event		Educated Father		Working Mother	
Variable	Ever graduate HS or get GED	Ever attend college	Ever graduate HS or get GED	Ever attend college	Ever graduate HS or get GED	Ever attend college
Treated * $\mathbb{1}\{M=0\}$	-0.0649**	-0.0324	-0.122***	0.0653	-0.0923**	-0.0499
	(0.0254)	(0.0953)	(0.0458)	(0.0756)	(0.0378)	(0.0578)
Treated * $\mathbb{1}\{M=1\}$	-0.0832**	-0.0388	-0.0488**	-0.102*	-0.0428**	-0.00109
	(0.0387)	(0.0571)	(0.0822)	(0.176)	(0.0205)	(0.0754)
Constant	0.974***	1.041	0.979***	1.171	0.974***	0.375
	(0.0146)	(0.939)	(0.0143)	(0.873)	(0.0145)	(0.500)
P-value	0.605	0.946	0.0125	0.0392	0.0516	0.316
Observations	1,197	1,154	1,197	1,154	1,196	1,153
R-squared	0.179	0.558	0.190	0.564	0.185	0.559
Mean dep var	0.958	0.789	0.958	0.789	0.958	0.788

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table reports the baseline results when allowing for the effect to vary across different groups. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double Lasso for each outcome. Columns (1) and (2) report the effect of treatment separately by event date (recent vs. old). Columns (3) and (4) split the results according to the father's educational level (high-school graduate or higher vs. non high-school graduate). Columns (5) and (6) allow the estimates to vary depending on the mother's employment status (working vs. non-working mothers). Therefore, the results read as follows: treatment decreases the probability that young adults with non-educated fathers graduate high school or get a GED by 12.2 percentage points, while it decreases the probability that young adults with educated fathers graduate high school or get a GED by only 4.9 percentage points (c.f. column (3), TAS panel). The difference between these two estimates is significant at the 5% level.

7 Mechanisms

As laid out in section 3, our theoretical framework highlights three critical inputs in the production of children's skills: children's and parents' personality (as captured by their level of socio-emotional distress) and parenting styles (as captured by a series of factor variables which proxy key dimensions of parenting). Recall that all parents' measures come from the CDS meaning they were taken when children are young.

A necessary condition for each of these elements to act as a mechanism is that they are responsive to the treatment. Therefore, we replicate the specifications used to produce Tables 10 and 11 but change the outcome. Instead of using children's skills during childhood or educational attainment at adulthood, we use one of the potential channel as the independent variable.

7.1 Effects on Young Adults' and Parents' Psychological Distress

In Table 14, we investigate if kidnappings had a direct effect on children socio-emotional distress at adulthood. Results are reported in column (1), we cannot detect a direct effect on young adults. In column (2) we also fail to reject the hypothesis that kidnappings do not change parents' personality. Therefore we rule out these potential channels as explaining the baseline results on cognitive skills and educational attainment.

Table 14: Effect of the treatment on young adults' and parents' socio-emotional distress.

	(1) TAS Socio-Emotional Distress	(2) Family Socio-Emotional Distress
Treated	0.0173	0.0428
	(0.0828)	(0.127)
Constant	0.123*	0.191
	(0.0627)	(0.142)
Observations	3,490	1,767
R-squared	0.118	0.143
Mean dep var	-0.00377	-0.0605

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table reports the results on children' and parents' socio-emotional distress measured respectively from CDS and TAS. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double Lasso for each outcome.

7.2 Effects on Parenting Styles

We then turn to the potential effects of kidnappings on parenting styles. Results are reported in Table 15. There appears to be a clear negative effect of kidnappings on how much school monitoring is done by parents. The effect is large, almost -0.1 of a standard deviation and highly significant. There appears to be a marginally significant result on share of time.

Table 15: Effect of the treatment on parenting styles.

	(1) Time Invested in Chore	(2) Time Invested in Entertainment	(3) Home Monitoring	(4) Advising	(5) School Monitoring	(6) Share Week Spent Out	(7) Share Weekend Spent Out
Treated	0.0260	0.0508	0.0419	-0.125	-0.0982***	0.0191*	-0.0249
	(0.0636)	(0.0712)	(0.0691)	(0.0788)	(0.0302)	(0.0109)	(0.0169)
Constant	-0.330***	-0.0492	0.0281	-0.298***	0.0878***	0.112***	0.208***
	(0.0713)	(0.0554)	(0.0531)	(0.0707)	(0.0225)	(0.00614)	(0.0127)
Observations	1,972	2,032	1,972	1,972	2,032	2,245	1,751
R-squared	0.141	0.074	0.102	0.211	0.113	0.048	0.052
Mean dep var	-0.102	-0.0374	-0.0474	0.00130	0.00376	0.114	0.199

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table reports the results on parenting style. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double Lasso for each outcome.

To check that the marginal significance indeed captures an effect and is not some idiosyncratic variation that randomly appears as significant, we reproduce the heterogeneity analysis for these outcomes. We check whether the effects on parenting styles are different depending on treatment date, on the education level of the father and on whether the mother was working when the child was young. We report these results in Table 16.[18] We look particularly at the effects on low educated fathers since this is the group for which the baseline effect is strongest.

What stands out is that several changes in parenting styles occur in households with low educated fathers. Not only do we observe a decrease in school monitoring but also in advising. There is also a large decrease in the share of time spent outside during the weekend. All these results are highly significant (at the 1% level).

When we relate those changes to the classifications of parenting style established in Baumrind (1967), kidnappings push parents to be less involved (for all parents) and to provide less freedom (particularly in the case of households with a low-educated father). This is characteristic of the so called authoritarian parenting style, or at least to an evolution towards such parenting style. Among the three possible mechanisms outlined in the theoretical framework, only one is responsive to the treatment, namely parenting style. It pushes parents (and more strongly parents for which the results to be explained is largest) to be more more authoritarian.

8 Robustness Checks

In this section, we perform a series of sensitivity tests in order to assess the robustness of our results. First, we show that the results are robust to alternative definitions of the treatment. Specifically, we show that the results are not affected by a change in the maximum age at which a child can be treated from eight to six or from eight to twelve. Second, we show that our results

[18]We also check that there is no heterogeneity for the outcomes on socio-emotional distress in Table G1. There is no such evidence.

Table 16: Heterogenous effect of the treatment on parenting styles.

	(1) Time Invested in Chore	(2) Time Invested in Entertainment	(3) Home Monitoring	(4) Advising	(5) School Monitoring	(6) Share Week Spent Out	(7) Share Weekend Spent Out
				Recent date			
Treated * $\mathbb{1}\{M=0\}$	0.000327	-0.0546	-0.146	-0.0925	-0.103***	0.0310*	-0.0216
	(0.0861)	(0.0866)	(0.102)	(0.108)	(0.0384)	(0.0169)	(0.0251)
Treated * $\mathbb{1}\{M=1\}$	0.0349	0.0858	0.110	-0.136	-0.0967***	0.00699	-0.0317*
	(0.0709)	(0.0707)	(0.0697)	(0.0849)	(0.0327)	(0.0112)	(0.0177)
Constant	-0.330***	-0.0502	0.0224	-0.297***	0.0877***	0.114***	0.209***
	(0.0715)	(0.0555)	(0.0533)	(0.0704)	(0.0225)	(0.00615)	(0.0124)
R-squared	0.141	0.077	0.110	0.211	0.113	0.049	0.051
Mean dep var	-0.102	-0.0374	-0.0474	0.00130	0.00376	0.115	0.200
P-value	0.707	0.0496	0.0157	0.682	0.870	0.162	0.671
				Educated Father			
Treated * $\mathbb{1}\{M=0\}$	0.0394	0.0566	0.0517	-0.259***	-0.118***	0.0128	-0.0447***
	(0.0874)	(0.0773)	(0.0781)	(0.0894)	(0.0319)	(0.0123)	(0.0171)
Treated * $\mathbb{1}\{M=1\}$	0.0142	0.0453	0.0332	-0.00661	-0.0795**	0.0132	-0.0156
	(0.0800)	(0.0743)	(0.0779)	(0.0763)	(0.0320)	(0.0115)	(0.0212)
Constant	-0.334***	-0.0491	0.0254	-0.247***	0.0875***	0.114***	0.209***
	(0.0745)	(0.0554)	(0.0517)	(0.0741)	(0.0225)	(0.00615)	(0.0124)
R-squared	0.141	0.074	0.102	0.222	0.114	0.047	0.053
Mean dep var	-0.102	-0.0374	-0.0474	0.00130	0.00376	0.115	0.200
P-value	0.817	0.834	0.798	0.000238	0.0898	0.971	0.135
				Working Mother			
Treated * $\mathbb{1}\{M=0\}$	0.0157	0.0679	0.0608	-0.125	-0.101***	0.0126	-0.0251
	(0.0643)	(0.0741)	(0.0703)	(0.0767)	(0.0305)	(0.0101)	(0.0178)
Treated * $\mathbb{1}\{M=1\}$	0.0692	-0.0139	-0.0428	-0.124	-0.0877*	0.0149	-0.0515*
	(0.106)	(0.0988)	(0.112)	(0.122)	(0.0497)	(0.0207)	(0.0283)
Constant	-0.328***	-0.0488	0.0227	-0.298***	0.0877***	0.114***	0.209***
	(0.0711)	(0.0553)	(0.0530)	(0.0717)	(0.0225)	(0.00615)	(0.0124)
R-squared	0.141	0.075	0.104	0.211	0.113	0.047	0.051
Mean dep var	-0.102	-0.0374	-0.0474	0.00130	0.00376	0.115	0.200
P-value	0.586	0.340	0.278	0.993	0.770	0.900	0.358
Observations	1,972	2,032	1,972	1,972	2,032	2,303	1,809

Notes: This table reports results on mechanisms when allowing for the effect to vary across different groups. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double Lasso for each outcome. The first panel reports the effect of treatment separately by event date (recent vs. old). The second panel splits the results according to the father's educational level (high-school graduate or higher vs. non high-school graduate). The third panel allows the estimates to vary depending on the mother's employment status (working vs. non-working mothers). Therefore, the results read as follows: treatment decreases the amount of advising received by children with non-educated fathers by 0.26 of a standard deviation.

are not affected by the choice of the empirical specification. Precisely, we show that the results are similar when using a difference-in-differences strategy instead of the strategy explained in section 6.1.

8.1 Changing the Definition of Treatment

In this section, we consider alternative treatment definitions. In turn, we alter the definition exposed in section 4.2 and use six ("Young if $\leq$ 6") and twelve ("Young if $\leq$ 12") as the threshold age under which a child can be selected as a treated or control.

The results are shown in Table 17. The top panel shows the results on CDS outcomes, and the bottom panel shows the results on TAS. Columns (1) and (2) focus on the version of the treatment using six years old as the threshold, while columns (3) and (4) use twelve years old. We can see that the results are largely unchanged. First, focusing on the top panel, we find that treatment leads to a significant decrease in children' cognitive skills with both versions of the treatment, and has no statistically significant impact on non-cognitive skills.

Moreover, when looking at the size of the estimates, we observe that the younger the group of children, the larger the effect: the effect of treatment on children who are six years old or younger is almost twice as large as the effect of treatment on children who are treated before twelve years old, and the baseline estimate lies between the two.

Second, the bottom panel shows that the long-term effect of the treatment are slightly affected by the change in definition. When using the "Young if $\leq$ 6" definition, we find that treatment is associated with a 3.6 percentage points lower probability of graduating high school or getting a GED. When using the "Young if $\leq$ 12" treatment, the effect loses its significance.

As a result, we also perform the heterogeneity analysis on the TAS outcomes, and display the results in Table 18. The top panel shows the results when using the "Young if $\leq$ 6" definition, and the bottom panel focuses on the "Young if $\leq$ 12" definition. Similarly to section 6.4, we find that regardless of the definition, the effect of treatment on the educational attainment of our young adults is driven by families from lower socio-economic backgrounds.

8.2 Changing the Empirical Strategy: Difference-in-Differences

Finally, in this section, we use a different empirical strategy. Specifically, we keep the treatment group unchanged (using the eight years old threshold), and define the control group as individuals who are either eighteen years old or younger when their county is selected as a control by the matching procedure, or individuals who live in a treated county and are between eight and eighteen years old at the time of treatment. Precisely, we define a cohort indicator: C_{ict} is equal to 1 for individuals below eight living in county c at time t, and equal to 0 for individuals aged between eight and eighteen living in county c at time t. As a result, we estimate a difference-in-differences specification in which we remove potential cohort-specific confounders:

$$y_{ict} = \alpha + \beta T_{ict} + X_{ict}^{1'}\gamma + X_{ict}^{2'}\delta + \epsilon_{ict} \tag{11}$$

where $T_{ict} = C_{ict} * T_{ct}$ in which T_{ct} denotes county-level treatment, and the other variables are defined as above. In this specification, we compare individuals from different cohorts from counties with different treatment status.

We present the results in Tables 19 and 20. In Table 19, we see that the difference-in-differences strategy fails to confirm the CDS results. However, Table 20 shows a result which is similar to our baseline result: treatment is associated with a significant decrease in the probability of graduating high school or getting a GED. Moreover, Table G2 in the Appendix reports the heterogeneity results on parenting styles estimated by difference-in-differences. The results are similar to those displayed in Table 16, which suggests that our results are robust to the use of an alternative empirical strategy.

Table 17: Results on children' and young adults' outcomes using different treatment definition.

	(1)	(2)	(3)	(4)
	CDS			
	Young if ≤ 6		Young if ≤ 12	
	Cognitive Skills	Non-cognitive Skills	Cognitive Skills	Non-cognitive Skills
Treated	-0.252**	0.00259	-0.132*	0.104
	(0.114)	(0.0926)	(0.0673)	(0.0769)
Constant	0.175	-0.0368	-0.116	-0.0368
	(0.181)	(0.0724)	(0.161)	(0.0610)
Observations	1,699	1,750	2,335	2,426
R-squared	0.367	0.061	0.363	0.074
Mean dep var	0.236	-0.0403	0.223	-0.0212
	TAS			
	Young if ≤ 6		Young if ≤ 12	
	Ever graduate HS or get GED	Ever attend college	Ever graduate HS or get GED	Ever attend college
Treated	-0.0362*	-0.0115	-0.0244	-0.0190
	(0.0209)	(0.0451)	(0.0184)	(0.0378)
Constant	0.975***	0.576***	0.961***	0.602***
	(0.0181)	(0.0536)	(0.0172)	(0.0748)
Observations	1,001	972	1,539	1,472
R-squared	0.177	0.249	0.246	0.298
Mean dep var	0.955	0.791	0.953	0.784

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: This table reports the results of the robustness analysis consisting in using alternative definitions of the treatment. The top panel focuses on the CDS analysis, and the bottom panel reports the TAS analysis. In both panels, columns (1) and (2) focus on the definition of the treatment in which the threshold age is six, and columns (3) and (4) use the definition in which the threshold age is twelve. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables with is chosen by Double LASSO for each outcome.

9 Placebo Tests

In this section, we perform one main placebo test. The idea behind it is to show that kidnapping events are a specifically relevant shock to study the effect of parenting styles on children development. It aims at strengthening the idea according to which parenting styles play an important role in the persistence of the effect in the long-term, as we claim in section 7.2. To do so, we consider an alternative shock to the environment one grows up in: school shootings. This choice is motivated by two main reasons. First, even though it is undeniable that school shootings are traumatising both for children and parents, the fact that they happen at school leaves little

Table 18: Heterogenous results on young adults' outcomes using different treatment definition.

	(1)	(2)	(3)	(4)	(5)	(6)
Mechanism	Recent Date		Educated Father		Working Mother	
Variable	Ever graduate HS or get GED	Ever attend college	Ever graduate HS or get GED	Ever attend college	Ever graduate HS or get GED	Ever attend college
			Young if ≤ 6			
Treated * $\mathbb{1}\{M=0\}$	-0.0432*	0.0165	-0.0768**	0.0827	-0.0494*	-0.0548
	(0.0231)	(0.0499)	(0.0333)	(0.0792)	(0.0258)	(0.0497)
Treated * $\mathbb{1}\{M=1\}$	-0.0327	-0.0241	-0.0147	-0.0634	-0.00382	0.0761
	(0.0268)	(0.0523)	(0.0147)	(0.0474)	(0.0150)	(0.0484)
Observations	1,001	972	1,001	972	1,000	971
R-squared	0.178	0.250	0.185	0.256	0.182	0.260
Mean dep var	0.955	0.791	0.955	0.791	0.955	0.791
P-value	0.737	0.455	0.00884	0.0929	0.0413	0.00345
			Young if ≤ 12			
Treated * $\mathbb{1}\{M=0\}$	-0.0297	0.0430	-0.0558**	0.0634	-0.0373*	-0.0542
	(0.0249)	(0.0450)	(0.0283)	(0.0649)	(0.0206)	(0.0397)
Treated * $\mathbb{1}\{M=1\}$	-0.0223	-0.0449	-0.00750	-0.0610	0.00293	0.0421
	(0.0202)	(0.0418)	(0.0148)	(0.0382)	(0.0214)	(0.0393)
Observations	1,539	1,472	1,539	1,472	1,538	1,471
R-squared	0.246	0.301	0.250	0.303	0.250	0.305
Mean dep var	0.953	0.784	0.953	0.784	0.953	0.783
P-value	0.766	0.0518	0.0289	0.0620	0.0700	0.00134

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table reports the results of the robustness analysis consisting in using alternative definitions of the treatment. The top panel uses the definition of the treatment in which the threshold age is six, and the bottom panel uses the definition in which the threshold age is twelve. Columns (1) and (2) report the effect of treatment separately by event date (recent vs. old). Columns (3) and (4) split the results according to the father's educational level (high-school graduate or higher vs. non high-school graduate). Columns (5) and (6) allow the estimates to vary depending on the mother's employment status (working vs. non-working mothers). All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables with is chosen by Double LASSO for each outcome.

room for parents to react. Second, 77.3 percent of the shootings of our sample happen in high school or in college, therefore affecting children at a more advanced age. These two elements make it more likely that the lasting effect on children, if any, is direct, rather than driven by changes in parenting styles.

We use data from Pah et al. (2017)[19] to build a measure recording the number of school shootings occurring in a U.S. county in a given year, and follow the same strategy as described in section 4 using that measure as our treatment variable. We perform our main analysis using this new treatment, and report the results in tables 21 to 23.

First, we find that school shootings don't seem to affect cognitive and non-cognitive skills of children in the short-run. Second, the results show that the placebo treatment does not affect young adults' educational attainment in the long-run. Finally, treatment has a positive impact on advising: parents who are affected by a school shooting tend to be more inclined to discuss their child's interests and future aspirations. Unlike our main result (using kidnapping events), this effect does not correspond to a change towards a more 'autoritarian' approach to parenting

[19] Data available at `https://amaral.northwestern.edu/school_gun_violence/`.

Table 19: Difference-in-differences effects of treatment on children' outcomes.

	(1) Cognitive Skills	(2) Non-cognitive Skills
Treated	-0.113	0.102
	(0.0920)	(0.107)
Constant	-0.126	-0.0868
	(0.515)	(0.530)
Observations	2,250	2,324
R-squared	0.479	0.232
Mean dep var	0.240	-0.0302

Note: This table reports the CDS results of the robustness analysis consisting in using difference-in-differences as an alternative estimation strategy. All regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double LASSO for each outcome.

Table 20: Difference-in-differences effects of treatment on young adults' outcomes.

	(1) Ever graduate HS or get GED	(2) Ever attend college
Treated	-0.0638**	-0.0399
	(0.0248)	(0.0395)
Constant	0.816***	-0.0860
	(0.0856)	(0.138)
Observations	1,533	1,534
R-squared	0.263	0.405
Mean dep var	0.960	0.781

Note: This table reports the TAS results of the robustness analysis consisting in using difference-in-differences as an alternative estimation strategy. All regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double LASSO for each outcome.

styles. Moreover, it is not associated with a change in young adults' outcomes.

This placebo analysis suggests that kidnapping events are particularly relevant shocks to the environment one grows up in: they not only significantly affect parenting styles, but their impact seems to be long-lasting, and to affect children outcomes up until adulthood.

Table 21: Effect of school shootings on children' outcomes.

	(1) Cognitive Skills	(2) Non-cognitive Skills
Treated	0.0934	0.160
	(0.137)	(0.185)
Constant	-0.210	0.387**
	(0.171)	(0.173)
Observations	1,638	1,638
R-squared	0.359	0.120
Mean dep var	0.224	0.00123

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table reports the effect of school shootings on children' outcomes. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double Lasso for each outcome.

Table 22: Effect of school shootings on young adults' outcomes.

	(1) Ever graduate HS or get GED	(2) Ever attend college
Treated	-0.00607	0.0197
	(0.0372)	(0.0584)
Constant	0.962***	0.580***
	(0.0181)	(0.0517)
Observations	910	941
R-squared	0.203	0.305
Mean dep var	0.945	0.748

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table reports the effect of school shootings on young adults' outcomes. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double Lasso for each outcome.

10 Conclusion

In this paper, we study the long-term effects of growing up with more or less protective parents on child development. To induce quasi-experimental variation in parenting styles, we focus on extremely rare but shocking events: nearby child kidnappings. To account for the fact that

Table 23: Effect of school shootings on parenting styles.

	(1) Time Invested in Chore	(2) Time Invested in Entertainment	(3) Home Monitoring	(4) Advising	(5) School Monitoring	(6) Share Week Spent Out	(7) Share Weekend Spent Out
Treated	0.159	-0.0110	0.0261	0.380***	-0.0504	-0.00439	-0.0156
	(0.122)	(0.131)	(0.127)	(0.115)	(0.0491)	(0.0141)	(0.0324)
Constant	-0.391***	-0.279*	-0.0124	-1.135*	-0.0649	0.108***	0.193***
	(0.134)	(0.159)	(0.106)	(0.656)	(0.0644)	(0.00836)	(0.0157)
Observations	1,638	1,638	1,638	1,638	1,638	1,781	1,395
R-squared	0.168	0.109	0.099	0.439	0.150	0.096	0.077
Mean dep var	-0.0860	-0.000890	-0.0707	-0.0143	0.0379	0.109	0.185
P-value	0.193	0.933	0.838	0.00111	0.307	0.756	0.630

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table reports the effect of school shootings on parenting styles. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double Lasso for each outcome.

kidnapping events might not be random, and using geo-localized data from the Panel Study of Income Dynamics, we rely on a matching strategy whereby we match U.S. counties which experienced a kidnapping to other counties which did not.

We find that living in a county in which a kidnapping occurred significantly decreases cognitive skills during childhood, and lowers the probability of finishing high school or getting the GED. Moreover, we find that this latter effect is driven by families in which the head has no college education.

Turning to the mechanisms, we find that these effects are not driven by a change in parents' and children' attitudes, but rather by changes in parenting styles. Specifically, parents who experienced a kidnapping in the vicinity of their home are more likely to adopt a more 'autoritarian' parenting style than those who did not. Again, this result is driven by families with low-educated household heads.

These results suggest that the shock experienced during childhood affects all children negatively in the relatively short term, but that while it dissipates in the longer-run for children from educated families, it has persistent significant negative effects in lower-educated families.

Based on our results, an exciting path for future research would be to dig into the reasons why low-educated families are more affected by the shock than higher-educated families. One potential mechanism that we want to test is whether low-educated parents are more likely to be influenced by the media and therefore to develop the type of moral panic called the "stranger danger" than more educated parents.

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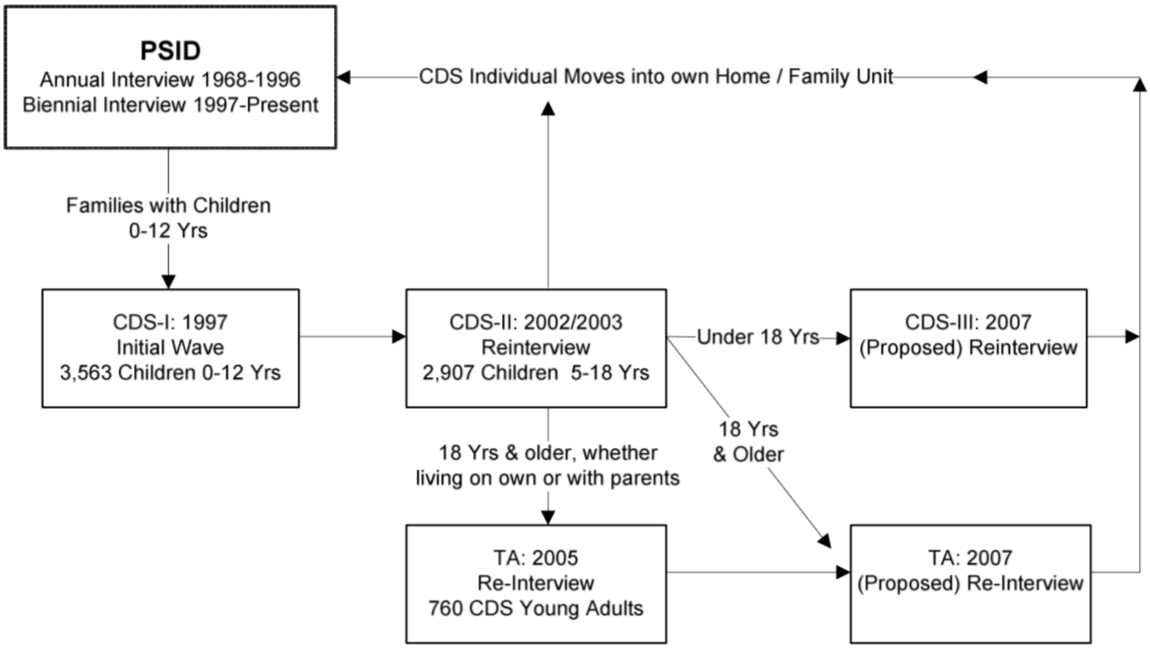
Appendix

A The Structure of the PSID

Figure A1: Relationship between the PSID, CDS and TAS.



Source: The PSID-CDS Design At-A-Glance, Institute for Social Research (2006).

B The Stranger Danger

There exist plenty of laws, newspaper articles, and other anecdotal evidence showing that kidnappings were a major national fear during our period of interest.

First, even though figures on the topic are hard to find, the first report of the Office of Juvenile Justice and Delinquency Prevention (OJJDP)'s *Missing, Abducted, Runaway, and Thrownaway Children in America* used police records data from 1988 to estimate the number of kidnapped individuals in the U.S. at that time.

Focusing on non-family abductions, they conclude that there were between 200 and 300 stereotypical kidnappings (defined as kidnappings by strangers that fit the general stereotype, involving ransom, homicide, the child being gone a substantial time or taken a substantial distance), and between 3200 and 4600 non-family legal abductions (abductions where children are moved, detained, or lured over shorter distances or time periods, usually in the course of other crimes like sexual assault).

Moreover, this same report highlights the fact that *"Even the knowledge that they are relatively rare can do little to mitigate the fears that these crimes inspire in the communities where they occur. To some degree these fears are archetypal in their source and proportions. Such events scar even children and families who are not personally involved."*

Then, the 1980s saw many new legislations emerge, as a response to parents' growing fear. Between 1982 and 1990, the Congress enacted the Missing Children's Act and the National Child Search Assistance Act, and the National Center for Missing and Exploited Children opened. Note that it is also in 1984 that the Missing Children Milk Carton Program was launched. An extract of the Wikipedia page about Milk cartons kids highlights the importance of the phenomenon: *"During the late 1970s and 1980s in the United States, missing child cases garnered a great deal of news media attention. [...] These reports developed into a type of moral panic called "stranger danger"."*

Finally, and maybe more strikingly, the way specialists talk today about that period tells a lot about how relevant the question of children's safety was. Paula Fass, professor emerita of history at Berkeley, and author of *Kidnapped: Child Abduction in America*, says:

> "What happens in the late '70s and '80s is there's this huge anxiety that children are being taken in order to be exploited sexually and then sadistically murdered. [...] People were surrounded with what looked like demonstrations of the fact that children were disappearing. [...] Parents were taking out kidnap insurance. They were being advised by local police stations to have their children fingerprinted."

All in all, these evidence suggest that the "Stranger Danger" really was a concern in American families during our period of interest.

C Age Standardization of the Measures

We standardize all the selected measures to remove the effect of age following Attanasio et al. (2020). First, we regress each measure m_i on the age of the individual a_i using kernel-weighted local polynomial smoothing methods:

$$m_i = f(a_i) + \epsilon_i \tag{C1}$$

and compute the fitted values $\hat{f}$ to obtain the age-conditional mean of each measure.

Second, we regress the square of the residuals estimated from equation (C1) on the age of the individual, again using kernel-weighted local polynomial smoothing:

$$(m_i - \hat{f})^2 = g(a_i) + \nu_i \tag{C2}$$

and compute the fitted values $\hat{g}$, which square root gives the age-conditional standard deviation of each measure.

Finally, we standardize each measure m_i by subtracting the age-conditional mean and dividing by the age-conditional standard deviation estimated in equations (C1) and (C2) respectively.

D County-Level Balancing Tests

The identification strategy relies on the fact that the occurrence of kidnapping events is uncorrelated to characteristics of the environment, after controlling for county and year fixed effects. As a results, to complement the individual-level balancing tests performed in section 4.3, we estimate the following equation on treatment-year characteristics using OLS:

$$y_{ct} = \alpha + \beta T_{ct} + \mu_c + \nu_t + \epsilon_{ct} \tag{D1}$$

where y_{ct} is the county-year-level characteristics which is being tested, T_{ct} is a dummy variable indicating whether county c is treated in year t, μ_c and ν_t are respectively county and year fixed effects, and ϵ_{ct} is the error term.

The county-year-level characteristics which we take into account are: (i) matching variables share of Whites, share of Blacks or African Americans, share of natives, share of the population living in an urban area, population density, population distribution over different income quintiles, and crimes rates (murder, rape, robbery, assault, larceny, burglary), and (ii) non-matching variables distribution of the population over educational levels, share of the population in the labour force, unemployment rate, share of the population living below the poverty level, share of renters among households, and share of vacant housing units.

We test $H_0 : \beta = 0$ against $H_1 : \beta \neq 0$, and present a summary of the results in Table D1.

Table D1: Summary on the county-level balancing.

	Rejection rate at the		
	10% level	5% level	1% level
Matching variables	0	0	0
Other variables	0	0	0

Note: This table provides a summary of the county-level balancing tests. Specifically, it shows the rejection rates of the test $H_0 : \beta = 0$ against $H_1 : \beta \neq 0$ performed after running regression D1.

We conclude from Table D1 that our matching procedure does very well in selecting control counties which are similar to the treated counties, and therefore that our treatment is conditionally random.

E The Mahalanobis Matching Procedure

Let T and and $\overline{T}$ be two groups of counties. T is composed of all the treated counties of the sample, and $\overline{T}$ gathers all the other counties. Let $X = x_1, x_2, ..., x_n$ be the set of covariates on which the matching is performed.

For each county i of group T, the Mahalanobis matching procedure computes a distance in terms of all the variables in X between i and all the counties of group $\overline{T}$, and select the counties from $\overline{T}$ which are closest to i in terms of that distance.

Formally, if i and j are the indices of two counties from group T and $\overline{T}$ respectively, the Mahalanobis distance is:

$$D_{ij} = (X_{i;} - X_j)\Sigma^{-1}(X_i - X_j) \tag{E1}$$

where Σ usually is the true or estimated variance-covariance matrix of X in the control group $\overline{T}$ (see Stuart and Rubin (2008) for more details). Note that equation (E1) can be re-written as:

$$D_{ij} = \sum_n \frac{(x_{in} - x_{jn})^2}{s_{nj}^2} \tag{E2}$$

where s_{nj} is the standard deviation of variable n in the control county j, and is therefore not more than a standardized Euclidean distance of all the elements of X between treated and non-treated counties.

F The Double LASSO algorithm

The algorithm developed by Belloni, Chernozhukov and Hansen (2014) aims at preventing making model selection mistakes while doing inference. Consider the linear model estimated on a sample of size n:

$$y_i = \beta T_i + X_i'\gamma + r_{yi} + \mu_i \tag{F1}$$

where X_i' is a p-dimensional vector of controls where $p \gg n$ is allowed, T_i is a treatment variable, r_{yi} is an approximation error, and the parameter of interest capturing the effect of the treatment on the outcome is β. Let the reduced form relation between the treatment and the controls be:

$$T_i = X_i'\theta_d + r_{di} + \nu_i \tag{F2}$$

By substituting (F2) into (F1), we get the following reduced form system:

$$y_i = X_i'\pi + r_{ci} + \epsilon_i \tag{F3}$$

$$T_i = \beta T_i + X_i'\gamma + r_{yi} + \nu_i \tag{F4}$$

Double LASSO consists in applying variable selection methods to each of the two reduced form equations, and then use the union of the two sets of selected variables in the estimation of β. This strategy ensures that any excluded control variable is at most mildly associated with y_i and T_i. Moreover, it improves estimation efficiency by selecting variables which strongly predict the outcome and therefore removing residual variance.

G Mechanism Heterogeneity

Table G1: Heterogenous effect of the treatment on young adults' and parents' socio-emotional distress.

	(1)	(2)	(3)	(4)	(5)	(6)
MECHANISM	RECENT DATE		EDUCATED FATHER		WORKING MOTHER	
Variable	TAS Socio-Emotional Distress	Family Socio-Emotional Distress	TAS Socio-Emotional Distress	Family Socio-Emotional Distress	TAS Socio-Emotional Distress	Family Socio-Emotional Distress
Treated * $\mathbb{1}\{M=0\}$	0.0293	0.166	0.0736	0.0835	0.0721	0.0529
	(0.115)	(0.183)	(0.0895)	(0.170)	(0.0836)	(0.133)
Treated * $\mathbb{1}\{M=1\}$	0.0112	-0.00482	-0.0266	0.0129	-0.0740	0.00642
	(0.0904)	(0.121)	(0.0862)	(0.127)	(0.0940)	(0.135)
Constant	0.124*	0.189	0.116*	0.184	0.113*	0.193
	(0.0643)	(0.142)	(0.0625)	(0.137)	(0.0621)	(0.142)
Observations	3,490	1,767	3,490	1,767	3,488	1,767
R-squared	0.118	0.145	0.120	0.144	0.122	0.143
Mean dep var	-0.00377	-0.0605	-0.00377	-0.0605	-0.00344	-0.0605
P-value	0.876	0.259	0.124	0.638	0.0229	0.642

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table reports results on mechanisms when allowing for the effect to vary across different groups. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double Lasso for each outcome. The first two columns (1) and (2) report the effect of treatment separately by event date (recent vs. old). Columns (3) and (4) split the results according to the father's educational level (high-school graduate or higher vs. non high-school graduate). The last two column (5) and (6) allow the estimates to vary depending on the mother's employment status (working vs. non-working mothers). Therefore, the results read as follows: treatment increases the socio-emotional distress of young adults with non-educated fathers by 0.07 of a standard deviation. This effect is not statistically significant at the 10% level.

Table G2: Difference-in-differences heterogenous effect of the treatment on parenting styles.

	(1) Time Invested in Chore	(2) Time Invested in Entertainment	(3) Home Monitoring	(4) Advising	(5) School Monitoring	(6) week_out	(7) weekend_out
			RECENT DATE				
Treated * $\mathbb{1}\{M=0\}$	0.122	-0.0455	-0.104	-0.0183	-0.0534	0.0233	-0.0766
	(0.146)	(0.156)	(0.156)	(0.129)	(0.0896)	(0.0219)	(0.0520)
Treated * $\mathbb{1}\{M=1\}$	-0.158*	0.0522	-0.0401	0.00438	-0.0698	-0.00526	-0.0516*
	(0.0866)	(0.0856)	(0.0738)	(0.0791)	(0.0647)	(0.0134)	(0.0274)
Constant	-0.557**	-0.670***	0.478**	1.890***	0.423***	0.0855**	0.263***
	(0.248)	(0.116)	(0.220)	(0.247)	(0.0817)	(0.0342)	(0.0202)
Observations	2,324	2,324	2,324	2,324	2,324	2,581	2,010
R-squared	0.243	0.195	0.224	0.344	0.265	0.178	0.171
Mean dep var	-0.0826	-0.0378	-0.0504	-0.00202	0.0264	0.114	0.200
P-value	0.0838	0.552	0.712	0.868	0.882	0.229	0.651
			EDUCATED FATHER				
Treated * $\mathbb{1}\{M=0\}$	-0.0584	0.0303	-0.0398	-0.126	-0.108*	-0.00421	-0.0703***
	(0.0816)	(0.0852)	(0.0743)	(0.0782)	(0.0612)	(0.0140)	(0.0266)
Treated * $\mathbb{1}\{M=1\}$	-0.148	0.0359	-0.0658	0.124	-0.0241	0.00468	-0.0435
	(0.0948)	(0.0878)	(0.0798)	(0.0792)	(0.0611)	(0.0130)	(0.0284)
Constant	1.632***	-1.436***	-1.234***	2.016***	-0.649***	0.118***	0.348***
	(0.271)	(0.150)	(0.197)	(0.208)	(0.0847)	(0.0264)	(0.0629)
Observations	2,324	2,324	2,324	2,324	2,324	2,581	2,010
R-squared	0.243	0.195	0.224	0.353	0.268	0.177	0.172
Mean dep var	-0.0826	-0.0378	-0.0504	-0.00202	0.0264	0.114	0.200
P-value	0.367	0.915	0.733	0.000195	0.0170	0.416	0.193
			WORKING MOTHER				
Treated * $\mathbb{1}\{M=0\}$	-0.0924	0.0588	-0.00740	-0.0202	-0.0810	0.000376	-0.0579**
	(0.0776)	(0.0824)	(0.0675)	(0.0748)	(0.0528)	(0.0122)	(0.0262)
Treated * $\mathbb{1}\{M=1\}$	-0.152	-0.0874	-0.265**	0.0930	0.00136	0.00139	-0.0414
	(0.114)	(0.135)	(0.119)	(0.109)	(0.0824)	(0.0214)	(0.0369)
Constant	1.674***	-0.727***	0.398*	0.139	0.448***	0.114***	0.357***
	(0.230)	(0.107)	(0.211)	(0.209)	(0.0794)	(0.0248)	(0.0686)
Observations	2,324	2,324	2,324	2,324	2,324	2,581	2,010
R-squared	0.242	0.197	0.231	0.345	0.267	0.177	0.171
Mean dep var	-0.0826	-0.0378	-0.0504	-0.00202	0.0264	0.114	0.200
P-value	0.540	0.155	0.0112	0.239	0.208	0.958	0.617

Notes: This table reports difference-in-differences results when allowing for the effect to vary across different groups. All the regressions are estimated by Ordinary Least Squares. All columns control for gender, age, race, and the set of variables which is chosen by Double Lasso for each outcome. The first panel reports the effect of treatment separately by event date (recent vs. old). The second panel splits the results according to the father's educational level (high-school graduate or higher vs. non high-school graduate). The third panel allows the estimates to vary depending on the mother's employment status (working vs. non-working mothers).