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Understanding the mechanisms through which adverse childhood experiences affect lifetime economic outcomes

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ABSTRACT

Over the past two decades, researchers have shown a growing interest in the role of adverse childhood experiences (ACEs) – children's confrontation with maltreatment and household dysfunction – in shaping health outcomes. This is the first study to quantify the economic penalties of ACEs and identify the mechanisms which produce the relationship. We source data from the National Child Development Study to construct an ACE index based on prospective childhood information. We estimate a robust earnings penalty of 9% for each additional ACE, a 25% higher probability of being welfare dependent, and a 27% higher probability of subjective poverty at age 55, over and above the influence of childhood socioeconomic disadvantage. The income penalty of ACEs is mainly produced by parental neglect, a component of the ACE index based on teacher assessments. It is observed for children from all socioeconomic backgrounds. Observed differences in later-life earnings between children with and without neglect exposure can be almost fully explained by observable differences in human capital accumulated by the beginning of mid-age. The productivity loss in an economy due to parental neglect is likely to be high. Our findings contribute to a wider discussion on the multidimensionality of childhood poverty.

1. Introduction

Children raised in material poverty are undisputedly at a much higher risk of cognitive and socioemotional developmental delays, poorer educational and health outcomes, lifelong under- or unemployment, welfare dependence, and involvement in crime (e.g., Duncan et al., 2017; Katsnelson 2015; Duncan et al., 2012; Duncan et al., 2010; Bird, 2013; Wiborg and Hansen, 2009, Barajas et al., 2007, Bradley and Crowyn, 2002, Duncan et al., 1998). The resulting economic cost of growing up poor is sizable. Estimates of such costs to society range between 1% of GDP in the UK (Blanden et al., 2010) and between 1 and –4% in the United States (Holzer et al., 2007).

A wealth of literature has examined the impact of childhood poverty, defining "poverty" as either a lack of access to financial or educational resources (see Duncan et al., 2017 for a comprehensive review). Official child poverty statistics are exclusively based on predefined income or consumption thresholds (Adamson, 2012; UNICEF, 2012; Barajas et al., 2007; Whiteford and Adema, 2007; Roosa et al., 2005).[1] Yet, psychologists are increasingly turning their attention to tangible explanations for the harmful impact that poverty can have on children's development. One explanation centers on the family-stress hypothesis (see Duncan et al., 2017): economically disadvantaged children are confronted with more environmental inequalities during their childhood, including separation from their families, instability, violence, or generally chaotic households (Evans and English, 2002; Evans, 2004; Evans and Kim, 2010). Research points to them experiencing harsher, more punitive, and less attentive parenting (e.g., Gershoff et al., 2007; Magnusson and Duncan, 2002; Hart and Risley, 1995; Conger and Elder, 1994; McLoyd, 1998).[2] Inequalities in parenting behaviors have

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[1] In all OECD countries, a child is considered to be poor if his or her family income lies below 50% of the country's median income. Some argue that absolute- and relative-threshold-based definitions of poverty fall short of adequately capturing the needs of families or the severity of deprivation (UNICEF, 2012). Alternative measures are sometimes used in the literature such as the income-to-needs ratio or a proxy for the persistence of poverty (see Barajas et al., 2007 for a discussion).

[2] Cobb-Clark et al. (2019) suggest that attentive parenting styles in child rearing, which are considered positive for children's development, may be less common in poor households because economically poor parents have less resources available to exert cognitive effort. This model is consistent with the family-stress model which describes a process by which severe economic pressure harms parental mental health and behaviors, which in turn have an impact on children's development (Conger and Elder, 1994). Conger and Elder (1994) argue that the experience of financial strain is psychologically stressful for parents and likely results in depressed moods, which may lead to increases in irritability. Prolonged financial hardship is expected to increase family conflict and decrease parents' perceptions of parenting efficacy. The emotional distress

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been shown to partially explain the link between material poverty and child behavioral problems (Kaiser et al., 2017) or school achievement (Kiernan and Mensah, 2011). Evans (2004) suggests that "cumulative rather than singular exposure to a confluence of psychosocial and physical environmental risk factors is a potentially critical aspect of the environment of childhood poverty" (p. 77).

This paper will focus on the longer-term economic impacts of cumulative psychosocial risks, referred to in the medical literature as adverse childhood experiences (ACEs) (Felitti et al., 1998). ACEs are defined as childhood exposure to parental "abuse" and "neglect" and "household dysfunction" (e.g., Felitti et al., 1998; Dube et al., 2003; Dong et al., 2004; Anda et al., 2006). Household dysfunction can comprise many things, but most research in this area defines it as exposure to parental alcohol or drug abuse, parental mental health issues, and parental absences due to death, divorce, or incarceration (e.g., Danese et al. (2009); Kelly-Irving et al., 2013a, 2013b; Solis et al., 2015). We assess the lifetime economic handicaps of ACEs in terms of income potential and poverty experiences, and the channels through which such relationships may emerge. Although a vast literature – which we will review in Section 2 – has emerged that links ACEs with medical outcomes in adulthood, to date there is little empirical evidence on the lifetime economic opportunities of ACEs.

To explore the link between ACEs and lifetime economic opportunities, we use high-quality cohort data from the National Child Development Study (NCDS) (Power and Elliott, 2006). The NCDS followed a birth cohort of children born within one week of each other in the United Kingdom from birth in 1958 up until age 55. The study is rich in detailed information about parents and their children – at birth, age 7, age 11, and age 16. Such a wealth of data allows us to construct a standard composite measure of ACE that has been widely used in the literature (e.g., Danese et al. (2009), Kelly-Irving et al., 2013a, 2013b; Solis et al. 2015). This measure has the advantage of being an objective measure of childhood adversity. It sums individual negative family-related life-events (neglect, time in foster care, parental absence, alcohol abuse, mental health problems, incarceration) that were recorded between the age of 7 and 16 in the NCDS. Thus, we neither rely on self-reports of trauma nor on retrospective information. However, its disadvantage is that it does not allow for different weights of individual components entering the index. We address this problem by providing robustness checks in which we assess the contribution of each individual component to the measure and by constructing an index through factor analysis, which allows heterogeneous weighing of different components by summarizing the covariability among observed components using low-dimensional latent variables (Gorsuch, 1983, 2003; Thompson, 2004).

Because follow-up data were collected on the children in young adulthood up until currently (age 55), we can link earlier-life ACEs with lifetime economic outcomes – as measured by foregone earnings, welfare dependence, and subjective poverty – and identify the channels through which this connection may emerge. To quantify the importance of each underlying mechanism, we use a variance-decomposition approach that was developed in Heckman and Pinto (2015) and was applied in Heckman et al. (2013). We calculate the contribution of differences in observable characteristics, measured at a time when cohort members enter adulthood, to the observed differences in earnings, welfare dependence, and subjective poverty between cohort members with high doses of ACEs (or other components of the ACE index) and cohort members without.

The contribution of our study to the literature is to explore ACEs as an important characteristic of childhood poverty. We therefore contribute to a discussion on the multidimensionality of childhood poverty. This idea is different from understanding ACEs as the key channel through which material poverty impacts upon children's development, a hypothesis that has been widely discussed in the literature (see Duncan et al., 2017). We hypothesize that ACEs capture the key risk factors that interfere with a child's lifetime economic potential, *independent* of a child's access to material or educational resources. This argument does not preclude the possibility that ACEs are more common in poorer households. Our argument implies that children in better income-resourced households could still be considered poor if they are exposed to ACEs, even though income-based thresholds would not identify them as in need. Thus, they should still be entitled to support from appropriate welfare programs. Conversely, it also implies that children raised in economically poor families may not need to be flagged as disadvantaged because their parents provide an excellent, low-risk home environment that requires little interference from the policy maker.

Empirically, we are able to demonstrate evidence in favor of our hypothesis. Although ACEs are disproportionately more common in economically disadvantaged families – children in such families are two to three times as likely to experience at least one adverse event – ACEs also occur in more privileged families. Furthermore, we find that ACEs are strong predictors of economic outcomes at age 55, over and above the influence of standard early-life predictors including health at birth, parental education, occupation, income, and household overcrowding. Experiencing one additional ACE – on a scale that is bound between 0 and 6 – is associated with an earnings penalty of 9%, and a significant increase in the probability of welfare dependence and subjective poverty by 25% and 27%, respectively. These findings are robust to alternative parameterizations of the ACE index, and allowing for nonlinearities in the relationship between ACEs and economic outcomes. The experience of neglect, an assessment made by the cohort member's teacher between the age of 7 and 11, is the driving mechanism in the association between ACEs and economic outcomes: a child assessed as neglected by their teacher has an earnings penalty of 22%, is 80% more likely to be welfare dependent, and 43% more likely to live in (subjective) poverty. Digging deeper, the observed differences in net earnings by age 55 between those who experienced neglect and those who did not are almost entirely explained by differences in human capital – educational achievements and cognitive and noncognitive skills – accumulated by the beginning of mid-age.

Our findings lend support to suggestions made elsewhere that "the true measure of child poverty is parenting" (Heckman, 2011, p. 4). Good parenting will set the foundation for the creation of personal, social, and – as we show – economic wellbeing (see Heckman and Mosso, 2014 for a discussion). The previous literature focuses predominantly on positive parenting behaviors as investment in child development. Our study suggests that negative parenting behaviors, such as child neglect, is clearly a divestment, independent of whether neglect occurs in richer or poorer families.

Traditional public policy responses to alleviate child poverty have come in the form of conditional or unconditional cash transfers. Although much has been written on the link between household income and children's outcomes and parenting behavior, causal evidence base on the effectiveness of cash transfers on children's outcomes and parenting behaviors is relatively limited. The few studies that exist using experimental evaluation methods demonstrate that cash transfers may be successful in boosting children's human capital (Gaitz and Schurer, 2017; Dahl and Lochner, 2012). The evidence base on their effectiveness in improving parenting behaviors is more mixed. Some find that cash transfers are effective (Akee et al., 2010; Hamad and Rehkopf, 2016), while others find they are not (Gaitz and Schurer, 2017; Gennetian and Miller, 2002). As of today, we do not know whether providing more money to households automatically takes away the stressors from parents (see Duncan et al., 2017 for a discussion). If the objective of the

associated with financial strain is the mechanism by which a link between poverty and poor parenting may emerge (Conger et al., 1992). On the other hand, a few researchers argue that it is not correct that poorer parents parent worse. Recent work by Dermott and Pomati (2016), who analyze a subsample of households from the 2012 Poverty and Social Exclusion in the UK (PSE) survey, show that parents in poor households are not less likely to engage in positive parenting activities such as reading, playing games or playing sports.

policymaker is to reduce children's exposure to poor parenting behaviors as an investment into their human capital, it may be more effective to direct resources to parenting interventions in primary care (see Brockmeyer et al., 2016 and references therein) or family-home visiting programs (see Huston, 2011 and references therein). Public policy that directly addresses poor parenting may produce large economic productivity gains.

This paper will proceed as follows. In Section 2, we review the existing literature on the association between ACEs and lifetime outcomes and their measurement problems. In Section 3, we explain the data used for the empirical analysis. Section 4 outlines our empirical modeling strategy. In Section 5, we present the estimation results. Section 6 discusses the limitations of our study design and the policy implications of our findings. Supplementary material is provided in the appendix.

2. Literature review

2.1. The origins of the ACE debate

Adverse childhood experiences (ACEs) are defined as "potentially traumatic events that can have negative, lasting effects on health and well-being" (Felitti et al., 1998). There is no single ACE but rather a whole host of possibilities, including child maltreatment, household dysfunction, exposure to mental health or substance abuse problems of a caregiver, and contact of a family member with the criminal justice system. Most of the early work focuses exclusively on the role of child maltreatment, which encompasses physical, sexual, and emotional abuse as well as neglect.

The seminal work by Felitti et al. (1998), also known as the ACE Study, demonstrates a significant relationship between exposure to ACEs (defined by child maltreatment) and risky health behaviors and disease in middle age using a sample of employed adults covered by Kaiser Permanente, a US private health insurer. The study found that individuals who reported four or more categories of childhood maltreatment, compared with those who experienced none, were four to 12 times more likely to suffer from alcoholism, drug abuse, depression, and suicidal thoughts. They were also two to four times more likely to smoke, and up to 1.6 times more likely to be obese.

Brooks (2012) has described these results as "striking" (p.1), as they revolutionized the way in which researchers and health professionals perceive childhood maltreatment. The study showed that ACEs could not only be seen as the root cause of mental and social problems in victims but also the leading cause of adult morbidity in developed nations. The ACE Study has some limitations, however. The authors only control for the confounding effects of age, sex, race, and educational attainment, and fully disregard the impact of childhood socioeconomic status. This is problematic because many studies show a strong association between household poverty and the probability of child maltreatment (Goldberg et al., 2013; Cancian et al., 2010). Similarly, there is a strong link between childhood poverty and health problems in adulthood (Magnuson and Votruba-Drzal, 2008). The ACE Study does not disentangle these pathways, which Clark et al. (2010) considers its "major methodological limitation" (p.386).

Palusci (2013) notes that since the original ACE Study, almost 60 papers have followed more or less its methodological approach, corroborating and extending its findings. Dong et al. (2003), Dong et al. (2004), Danese et al. (2009), and Brown et al. (2009) have assessed the impact of ACE – measured by maltreatment factors only – on liver disease, ischemic heart disease, cardiovascular disease, and premature mortality, respectively. Using data from the NCDS and an extended measure of ACE, Kelly-Irving et al. (2013a, 2013b) and Solis et al. (2015) find significant relationships between high-dose ACEs and cancer, mortality, and general wear and tear, controlling for a rich set of early-life background factors. Isohookana et al. (2016) and Thomas et al. (2008) find a significant link between early-childhood abuse and obesity and unhealthy weight control behaviors; however, such a finding could not be replicated by Hariharan et al. (2018) using NCDS data. Schilling et al. (2007) find a significant relationship between ACE and depressive symptoms, drug use, and antisocial behavior. Investigating a sample of children from New Zealand, Danese et al. (2009) shows that those who were exposed to one or more ACEs were more at risk for depression later in life. Mersky et al. (2013) show a robust association between ACE and heavier use of tobacco, alcohol, and marijuana. More recently, Merrick et al. (2017) demonstrate that the link between childhood adversity and adult mental health service use is driven by a higher risk of depression, suicidal thoughts, drug use, and alcoholism.

2.2. The relationship between ACE and skills, education, and crime

Some studies document a link between maltreatment experiences and cognitive and noncognitive skill development. Fletcher and Schurer (2017) use sibling-fixed-effects models on a US cohort to study the causal impact of maltreatment experiences on noncognitive skill development in young adulthood. The authors find that sexual abuse experiences result in higher levels of neuroticism, while parental neglect results in lower levels of conscientiousness plus higher levels of neuroticism. Richards and Wadsworth (2004) show a long-term detriment of maltreatment on cognitive function, memory and concentration, and educational attainment. Using data from the Christchurch birth cohort study, Boden et al. (2007) convey that study participants who have experienced either sexual or physical abuse are significantly less likely to complete secondary schooling or to enroll at a university.

The impact of maltreatment on educational attainment is likely to operate through suboptimal school performance. Wodarski et al. (1990) report that students who experienced earlier-life abuse and/or neglect score lower on standardized language tests and are twice as likely to repeat a grade. Using data on siblings, Slade and Wissow (2007) find that children with maltreatment experiences score significantly lower grade point averages and have more problems with completing homework assignments in middle and high school. In line with previous evidence, the authors link poor school performance to cognitive deficits related to attention problems that result from maltreatment experiences.

Currie and Tekin (2012) further highlight the potential impact of maltreatment on the propensity to participate in criminal activity. Using siblings- and twin-fixed-effects models, the authors show that experiences of child abuse and neglect double the likelihood of committing a crime in young adulthood. Interestingly, the authors find this relationship for both boys and girls.

2.3. The relationship between ACE and economic outcomes

Despite broad empirical evidence that supports a significant link between ACEs and health, education, and skill development, less is known about the impact of ACEs on lifetime economic outcomes. Only recently, a series of studies has emerged that provide some insights. For instance, Metzler et al. (2017) use cross-sectional data from the 2003/2004 Behavioral Risk Factor Surveillance System (BRFSS) from 10 states and the District of Columbia to study the relationship between ACE and employment, and poverty in adulthood. Adults who experienced four or more ACEs in childhood (retrospective reports) were 2.3 times as likely to be unemployed, and 1.6 times as likely to live in a household reporting poverty than adults with no or less ACEs. Sansone et al. (2012) and Covey et al. (2013) find similar impacts on adulthood employment status. Currie and Widom (2010) find a 14% gap in employment probabilities at age 40 between adults with and without court-substantiated histories of abuse/neglect, controlling for background characteristics. Where participants reported earnings, individuals with documented histories of abuse and/or neglect reported almost $8000 less per year on average than controls. Using self-reported and retrospective data from the 2009 BRFSS, Liu et al. (2013) show that men who had experienced one to three ACEs were almost twice as likely to be unemployed than men with no ACEs. The authors suggest that the link between ACEs and unemployment is mediated by education, marital status, and social support. Studying the mediating factors of the relationship between ACE and health outcomes, Font and Maguire-Jack (2016) find that adults who report sexual abuse experiences have significantly lower income levels; the magnitude of the income reduction associated with sexual abuse is not reported. Using data from the NCDS (and other British cohort data), Conti et al. (2017) find no link between child maltreatment – defined by retrospective, self-assessed measures – and employment or earnings.

These previous studies show a link between ACE and adulthood economic outcomes, but they do not provide a good understanding of the magnitude of this impact. With the exception of Currie and Widom (2010), all studies rely on retrospective self-reports of ACEs. We contribute to this very recent literature by (i) providing a rigorous analysis of the later-life economic penalties of ACEs in one major OECD country, (ii) identifying the mechanisms underlying this relationship, and (iii) improving upon previous study designs. Many previous studies were not able to adequately control for childhood socioeconomic status and relied on later-life retrospective self-evaluations of maltreatment and household-dysfunction experiences. We discuss the limitations of retrospective ACE measures in the next section.

2.4. Measurement issues

When it comes to testing the relationship between ACEs and outcomes, one obstacle is that childhood adversity is difficult to measure. At the time of occurrence during childhood, it is hard for anyone outside a child's immediate environment to truly know whether a child is confronted with challenges such as familial instability or parental maltreatment. Existing studies have tackled this problem in a variety of ways, revealing that all measures of ACEs present certain benefits and limitations.

Most of the previous studies discussed above use retrospective, self-reported data on parental maltreatment, which poses reliability concerns. Some authors argue that retrospective reports of ACEs are always invalid for two reasons. First, people may forget (or choose to forget) past maltreatment as they grow older. Secondly, individuals with severe health or employment problems may perceive their childhood experiences more negatively than their healthier or more successful peers (Brown and Harris, 1978; Clark et al., 2010).

For instance, previous literature confirms the existence of recall bias, wherein the accuracy of self-reported maltreatment is a function of current health status (Widom et al., 2004; Hardt and Rutter, 2004). The phenomenon of "effort after meaning" explains such behavior: unhealthy individuals search for an explanation for their state of bad health, thus assigning more meaning to negative past events. If this is true, studies using self-reported data will likely overestimate the effect of ACEs on health outcomes. Widom et al. (2004) conclude that while "it is tempting to be convinced by the volume of retrospective studies which link child abuse to certain outcomes … the studies may all suffer from the same potential biases" (p. 721).

Conversely, Currie and Tekin (2012) assert that "several researchers have studied the validity of self-reported data on child maltreatment and have concluded that, if collected properly, this data is valid" (p.514). Data validity is improved if respondents can listen to prerecorded questions through earphones and enter their answers directly on laptops to maintain confidentiality and minimize the potential for interviewer influence. To ensure accurate responses about the timing of events, subjects should also be prompted with a calendar of important events. Currie and Tekin (2012), who use cohort data from Add Health, which explicitly followed these protocols, show that older cohort members do not differ in their reports of ACEs than younger cohort members. They also demonstrate that twins who differed in their self-reports of maltreatment did not differ in their self-reports of family information where no difference was expected. Thus, the authors conclude that the maltreated twin did not systematically suffer from recall bias or effort after meaning, reinforcing the validity of the ACE data.

To mitigate concerns about retrospective ACE measures, some studies opt for administrative data such as court-substantiated cases of child abuse, or cases of maltreatment reported to government agencies. For example, Currie and Widom (2010) and Young and Widom (2014) use court-substantiated abuses to estimate the effect of ACEs on economic wellbeing and emotional processing in adulthood. The benefit of court-substantiated data is that it is considered objective. However, Currie and Tekin (2012) argue that such data captures only a small fraction of all ACEs because of severe underreporting and low conviction rates. Official records of abuse are likely to pertain to households that catch the attention of official agencies for other reasons, such as unemployment or ill health. As such, reliance on administrative data is likely to produce a small and unrepresentative sample of families in which ACEs occurs.

In the past decade, more studies have exploited prospective longitudinal data to construct an ACE measure. Prospective longitudinal studies collect information on cohort members at several stages during childhood, during which reports are obtained from family members, doctors, or teachers. This information can be used to construct a more reliable ACE measure, since it captures objective evidence of adversity at the time of its occurrence. Danese et al. (2009), for example, use data from the Dunedin Multidisciplinary Health and Development Study to assess the effect of ACEs on adult inflammation. They construct their ACE measure from a combination of behavioral observations and parental reports during childhood in addition to retrospective reports by study members once they have reached adulthood. The authors manage to avoid using self-reports for all ACE indicators except outright abuse (physical and sexual abuse).

Kelly-Irving et al. (2013a) and Solis et al. (2015) are two of the few studies which use an ACE index that does not rely on retrospective reports. Although available in their data,[3] their ACE index does not incorporate physical or sexual abuse. We follow these two studies to construct an ACE index solely from prospective data that does not rely on self-reports and was collected decades before economic outcomes were recorded. Unfortunately, we cannot identify exogenous variation in ACEs to identify the causal impact of ACEs on economic outcomes like Currie and Tekin (2012), Fletcher and Schurer (2017), and Slade and Wissow (2007), who control for family fixed effects by using siblings or twin samples. However, we do carefully control for childhood socioeconomic status and other relevant pre-treatment conditions so our findings can be interpreted as the relationship between ACEs and economic outcomes alone, without the confounding influence of childhood socioeconomic status, family composition, and at-birth health outcomes.

3. National Child Development Study (NCDS)

The analysis is conducted with data from the National Child Development Study (NCDS), a British cohort study that collected information at birth on 18,558 children born within a single week in the United Kingdom (UK) in 1958 (Power and Elliott, 2006). This study provides longitudinal data on each child's: birth outcomes; physical and educational development into young adulthood; economic outcomes; family situation; employment; health; wellbeing; social status; and behavioral attitudes. The data set includes information from different stages of cohort member lives, collected through interviews with the primary caretaker (predominantly the mother), assessments of the cohort members' ability by the interview team, and teacher assessments. In later sweeps, cohort members were interviewed themselves.

Information on children was collected at ten different points in time: at ages 0, 7, 11, 16, 23, 33, 42, 46, 50 and 55, with age 0 being sweep

[3] Conti et al. (2017), Hariharan et al. (2018), and Kelly-Irving et al. (2013a) use self-reported maltreatment indicators that were included in a special module on biomarker assessment.

0, age 7 being sweep 1 and so on. The earlier collections gathered comprehensive information on both the children's cognitive and noncognitive abilities as well as information on parental background such as: (i) family background and financial situation from birth to age 16; (ii) cohort member physical and mental health outcomes from birth to age 55; (iii) household composition and structure in terms of family composition within household; (iv) education covering information from primary school through secondary and tertiary education (here, we consider school participation and activities as well as later life course qualifications of the children as well as educational information about the mother and father); (v) cognitive and noncognitive skills covering the child's early-life test scores of reading, writing, and mathematics as well as personality trait test scores; and (v) employment and financial situation during adult years from age 17 onwards.

Although 18,585 children and their families participated in the first wave of the data collection, we are able to follow at maximum 7450 cohort members until age 55 with no missing observations on economic outcomes and ACE components. We will show in a later section the direction and the degree to which our estimation results are likely to be influenced by attrition.

3.1. ACE index components

ACE can be conceptualized in a variety of ways. Previous studies have acknowledged the difficulty in finding a definition of the concept that is clear, unambiguous and acceptable to all (Currie and Tekin, 2012). Given these challenges, we have opted to use a measure of ACE which has been frequently used in previous studies which utilized the NCDS data (e.g., Kelly-Irving et al., 2013a, 2013b; Solis et al. 2015). It is an index of experiences that captures 'a set of traumatic and stressful psychosocial conditions that are out of the child's control, that tend to co-occur and persist over time' (Kelly-Irving et al., 2013b. p.2). Kelly-Irving et al. (2013a, 2013b) developed this index from notable epidemiological studies of ACE (e.g., Rosenman and Rodgers, 2004; Dong et al., 2004; Surtees and Wainwright 2007; Benjet et al., 2009; Anda et al., 2006). The index is constructed from the following items (each item can take a value of 0 if negative or 1 if positive):

1 Child in care: child has been either in public or voluntary foster care services at ages 7, 11 or 16.
2 Physical neglect: whether the child appears undernourished or dirty at ages 7 or 11. Information is collected from teacher responses to the Bristol Social Adjustment Guide.
3 Offenders: the child has lived in a household where any given family member (who lives in the same household as the child) was either in prison or on probation at age 11, or a household member was in contact with probation services at age 7 or 11.
4 Parental separation: child has been separated from his or her mother or father due to either death or separation (including divorce) at age 7, 11 or 16.
5 Mental illness: household has been in contact or is still in contact with mental health services at age 7 or 11. Alternatively, any family member has mental illness at age 7, 11 or 16.
6 Alcohol abuse: family member suffers from alcohol problems at age 7.

All items are summed with equal weighting to construct an ACE index, bounded between 0 (no adversity) and 6 (for maximum possible adversity). The index is increasing in the frequency of ACEs. In a robustness check, we exclude parental separation as a possible category of negative experience since the literature on parental separation has produced mixed results on whether it is associated with positive or negative economic or educational outcomes in affected children (Amato, 1988; Amato, 2000). This alternative ACE index varies between 0 and 5.

As mentioned, the benefit of this ACE measure is that it is free of any retrospective self-reported data, so the potential for recall bias is minimized. However, the drawback of this is that it does not include childhood physical, sexual, and verbal abuse, as this information was collected from NCDS participants during their adulthood. As such, our chosen ACE measure focuses on neglect, family instability and risky family behaviors, rather than more egregious forms of parental maltreatment and abuse. However, we hope that some of these more extreme experiences are captured in the variable 'ever in (foster) care', which indicates the removal of the child from its parental home for safety reasons. Another disadvantage of this ACE measure is that it assumes that each of the six index components are of equal weighting. This masks considerable heterogeneity in the sample, and also assumes that the impact of ACE on lifetime economic outcomes is linear across the ACE scale. For example, it assumes that experiencing an additional ACE has the same effect, whether it is from 1 previous ACE or 4 previous ACEs.

We address this problem by providing two robustness checks to this standard way of constructing the ACE index. First, we decompose the ACE index into its individual components and test each component's effect on lifetime economic outcomes. Additionally, we estimate the impact of high-dose ACE, which is a threshold that has been applied in Kelly-Irving et al. (2013a, 2013b) to indicate a high intensity of adverse childhood experiences. Second, we construct an ACE measure through factor analysis, a statistical tool that summarizes the covariability among observed measures using low-dimensional latent variables (Gorsuch, 1983, 2003; Thompson, 2004). It allows for heterogeneous weighing of different components and an assessment of the importance of each component in entering the ACE measure. As this measure has no natural unit of analysis, we standardize this measure to mean 0 and standard deviation 1, so that an increase in ACE is considered in terms of 1 standard deviation (SD). Using this measure, high-dose ACE is defined as a score above the mean ACE score.[4]

3.2. Outcome variables

The main outcomes of interest are net individual earnings, welfare dependence, and subjective poverty recorded at age 55, currently the latest follow up with the cohort members. Age 55 economic outcomes are ideal to assess the longer-term impact of ACE on economic outcomes because they are recorded before retirement. Net earnings are measured as net monthly pay in 2011 reported in British pounds. Respondents in the survey were asked about their net monthly income in their main job/occupation after tax and other deductions. As is common in the literature, we take the log of this measure to allow for nonlinearities at the top end of the distribution and to interpret marginal effects of interest in terms of (log) percent changes.

Welfare dependence is based on a question in which respondents are asked "do you or your partner/husband/wife currently receive a regular payment from any of the following sources?" which includes government transfers, tax credits, and benefits as possible answers. Those who do receive any combination of government transfers, benefits, or tax credits would be classified as welfare dependent and those who do

[4] Note, this approach is based on the idea that all items in the index measure the same underlying construct. This may not be entirely correct for the ACE measure, since the ACE Index proposed in the literature aims to summarize cumulative risk that stems from different areas of a child's environment. Factor analysis is therefore theoretically not perfectly suitable in our setting, because it assumes an underlying latent factor, e.g., adversity. This is reflected also by a relatively internal consistency across all items. According to Cronbach's alpha, which is an internal consistency measure, $\alpha = 0.27$ across all six components. When excluding separation from parents, α increases to 0.34. We are able to identify two principal factors onto which the six components load. Five components of the ACE index load positively on factor 1, while "separation from parents" loads negatively on factor 1 (with a very small weight), and positively on factor 2 (with a very large weight). Detailed results are provided upon request. This means that our ACE index to proxy adversity has measurement error. If this measurement error is classical, then we would underestimate our treatment effects of interest. We thus consider our treatment effects as lower bounds.

not receive any of these benefits would be classified as not welfare dependent.[5]

A measure of subjective poverty experiences is constructed from a question that asks participants at age 55 whether they consider themselves financially struggling. Respondents are asked "how well would you say you personally are managing financially these days?" Those who respond as finding it quite difficult or very difficult are classified as living in subjective poverty, while those who respond that they are getting by or able to get by comfortably are classified as not living in subjective poverty. This measure is used instead of a more objective measure of poverty that requires information on the income of all household members, which is not available in the NCDS. There has been considerable debate about the use of objective poverty measures (such as income, expenditure and consumption), as these can be sensitive to survey design and arbitrarily assign people to either being in poverty or not in poverty based on a largely arbitrary line (Deaton and Heston, 2010; van Praag and Ferrer-i-Carbonell, 2008). Recently, Mahmood et al. (2019) have used Pakistan Panel Household Survey data to show that the determinants of subjective poverty (i.e., feeling poor) are different to the traditional determinants of objective poverty, and that the Spearman Rank test upholds that subjective poverty measures complement the conventional method.

For completeness, we considered as outcome variables the probability of unemployment and the probability of positive earnings. These findings are relegated to an appendix as we find no significant relationship between ACE and these probabilities.

3.3. Control variables

To rule out confounding influences, we control for various factors that have been shown in the previous literature to impact upon later-life economic outcomes and that have been used in previous studies on the impact of ACE on later life outcomes using the NCDS data (Kelly-Irving et al., 2013a, 2013b; Solis et al. 2015). We focus on factors that could have occurred before exposure to ACEs and/or which were out of the cohort member's control. These variables include:

1 the child' sex;
2 the child's initial health condition – prematurity (less than 37 weeks of gestation); low birth weight (less than 2500 g)[6];
3 the child's birth order (twins, first-born, second-born, third-born, fourth-born or higher);
4 the age of mother when she gave birth to the child (teenage mom (age <20), young adult mother (19 < age < 35), or mature aged mother (age > 34).
We also pay careful attention to control adequately for childhood socioeconomic status of the family by including the following variables:
5 overcrowding in the household - persons per room at age 5 (1 person per room (PPR), 2 PPR, 3+ PPR);
6 father's social class as measured by his occupation (if the father is present) – measured when the cohort member was age 10;
7 mother's and father's education levels (age they left full-time education) – measured when the cohort member was age 16;
8 father's income band (if present) – measured when the cohort member was age 5.

By including this extensive list of control variables, we risk significantly reducing the sample size as there are many participants who do not have complete information on all variables. For instance, income information is missing for 41.5% of cohort members, while parental education information is missing for over 20% (see Table 1). To address missingness, we recode missing observations with 0 values and flag the observations with a missing variable binary indicator (see Graham, 2009 for an overview of methods; see Conti et al., 2017; Clark et al. (2010); Kelly-Irving et al., 2013a; Power et al., 2015 for using imputation methods to maximize sample sizes in the context of NCDS data analysis). By controlling for missing observations, our final sample size is 6887 observations with non-missing earnings (including 0) and ACE data, 7450 observations for welfare dependence, and 7384 observations for subjective poverty.

4. Empirical framework

4.1. Estimating the relationship between ACE and economic outcomes

First, we estimate a linear regression model to test for a statistical relationship between ACEs and later-life economic outcomes. The dependent variable is either log net earnings, welfare dependence, or living in poverty, which are all measured at age 55, and the main independent variable is ACE.

$$Y_i = \beta_0 + \beta_1 ACE_i + \varepsilon_i, \tag{1}$$

ACE_i is either a continuous measure of the number of adverse experiences a cohort member endured during childhood or binary measure of ACE_i^B that takes the value 1 if the individual experienced two or more ACEs, and 0 otherwise – where 1 indicates high-dose ACEs (see pp. 11–12 for a description of measures). Of particular interest is the parameter β_1. In the case of a continuous ACE measure, β_1 captures the association of one additional adverse event with economic outcomes whereas with a binary measure of ACEs, this coefficient represents the differences in economic outcomes between those with zero or one ACEs, and high-dose ACEs.

It is important to emphasize that the ACE index is an endogenous variable; some children are more likely to suffer from ACEs than others and thus have poor lifetime economic outcomes independent of ACEs. For instance, this could occur because children with ACEs are more likely to be living in low-income or education-poor families, and childhood poverty (in terms of income) is associated with negative economic opportunities later in life (see Fletcher and Schurer, 2017 for a discussion). Not controlling for this selection bias would likely overstate the estimated relationship of interest. Therefore, we estimate subsequent models that include controls for X_i to capture the confounding factors mentioned previously.

$$Y_i = \propto_0 + \propto_1 ACE_i + \propto_2 X_i + \varepsilon_i. \tag{2}$$

We identify $\propto_1$ on the assumption of conditional independence between the error term ε_i and ACE_i. A statistically significant parameter $\propto_1$ is interpreted as a robust association between ACEs and lifetime economic outcomes Y_i, over and above the influence of X_i.

4.2. Decomposition analysis

In a second step, we explore the underlying mechanisms through which ACEs are likely to impact later-life economic outcomes.

[5] It should be noted that in 2013, around the same time when the cohort members were interviewed at age 55, welfare reforms occurred in the UK. This reform came into effect beginning 1st April 2013 to replace the Disability Allowance Program with the Personal Independence Program (PIP). Similarly, limits were imposed on the total amount of benefits that a 16–64 year old could claim (Department for Work and Pensions, 2017). We believe that this policy change will not have a major impact on our welfare dependence findings as the data questionnaire was conducted for Sweep 9 of the NCDS between September 2013 and March 2014. This is during a time period after the welfare eligibility changes have fully come into effect (to ensure no crossover between the old and new system) whereby each cohort member is exposed to the same type of welfare regime.

[6] Controlling for early life health is important as such factors are associated with poor labor market outcomes. For instance, Johnson and Schoeni (2011) show that low birth weight reduces labor force participation probabilities by 5 percentage points and labor market earnings by roughly 15%. We refrain from further controlling for maternal behaviors before birth in the interest of keeping the model as parsimonious as possible. Birth outcomes should sufficiently reflect maternal health behaviors.

Table 1
Summary statistics for minimum sample size.

	Count	Mean	SD	Min	Max
Panel A: Economic outcomes age 55					
Net income (>0)	4132	3581	15,100	22	475,000
Any positive earnings	6887	0.600	0.490	0	1
Unemployed	6283	0.033	0.179	0	1
Welfare dependence	7450	0.173	0.378	0	1
Subjective poverty	7384	0.099	0.299	0	1
Panel B: Adverse childhood experiences ages 7–16					
ACE Index	4132	0.362	0.639	0	5
ACE>1	4132	0.052	0.221	0	1
Ever in (foster) care	4132	0.030	0.170	0	1
Neglect teacher assessed	4132	0.039	0.195	0	1
Separation from parents	4132	0.231	0.421	0	1
Mental illness	4132	0.035	0.185	0	1
Alcohol abuse	4132	0.007	0.081	0	1
In prison	4132	0.020	0.141	0	1
ACE Index w/o divorce	4132	0.131	0.421	0	4
ACE>1 w/o divorce	4132	0.020	0.139	0	1
ACE Index factor analysis	4132	−0.014	0.975	−0.41	10.86
ACE>mean	4132	0.106	0.308	0	1
Panel C: Control variables ages 0–7 (unless indicated otherwise)					
Female	4132	0.536	0.499	0	1
Premature (< 37 weeks)	4132	0.019	0.136	0	1
Premature missing	4132	0.087	0.282	0	1
Low birthweight (< 2500 g)	4132	0.074	0.261	0	1
Birthweight missing	4132	0.002	0.049	0	1
Twins	4132	0.018	0.132	0	1
First-born	4132	0.371	0.483	0	1
Second-born	4132	0.320	0.467	0	1
Third-born	4132	0.149	0.356	0	1
Fourth-born or above	4132	0.141	0.348	0	1
Birth-order info missing	4132	0.005	0.071	0	1
Crowding in family home					
1 person/room	4132	0.733	0.442	0	1
2 persons/room	4132	0.164	0.370	0	1
3 or more persons/room	4132	0.103	0.304	0	1
Age mother at birth					
Less than 20 years	4132	0.044	0.206	0	1
20–34 years	4132	0.827	0.378	0	1
More than 34 years	4132	0.129	0.335	0	1
Father's occupation					
No father	4132	0.024	0.154	0	1
Manager/Legislator	4132	0.060	0.237	0	1
Professional	4132	0.159	0.365	0	1
Skilled non-manual	4132	0.111	0.315	0	1
Skilled manual	4132	0.434	0.496	0	1
Unskilled/ non-manual	4132	0.016	0.125	0	1
Unskilled manual	4132	0.148	0.355	0	1
Skill undetermined	4132	0.048	0.214	0	1
Father age left FT edu (age 16)	4132	15.142	2.096	12	24
Mother age left FT edu (age 16)	4132	15.033	1.610	12	24
Father education missing	4132	0.251	0.433	0	1
Mother education missing	4132	0.234	0.423	0	1
Father Income: Missing (age 11)	4132	0.415	0.493	0	1
Father's income bounds					
Bottom 10th percentile	4132	0.075	0.264	0	1
10 to <20 percentile	4132	0.116	0.320	0	1
20 to <50 percentile	4132	0.122	0.327	0	1
50 to <75 percentile	4132	0.087	0.282	0	1
75 to <90 percentile	4132	0.106	0.308	0	1
Top 10th percentile	4132	0.078	0.268	0	1
Panel D: Variables for Mechanism analysis ages 33 (unless indicated otherwise)					
Math test score age 16	4423	11.204	8.493	0	31
Reading compreh. age 16	4423	21.292	12.267	0	35
Math test missing	4423	0.216	0.412	0	1
Reading test missing	4423	0.214	0.410	0	1
External locus of control	4423	0.066	0.249	0	1
Somewhat external	4423	0.070	0.255	0	1
Somewhat internal	4423	0.163	0.369	0	1

(*continued on next page*)

Table 1 (*continued*)

	Count	Mean	SD	Min	Max
Internal locus of control	4423	0.701	0.458	0	1
Rutter Malaise Index	4423	22.80	2.190	0	24
Physical health problem	4423	0.375	0.484	0	1
Less than min. schooling	4423	0.110	0.312	0	1
Minimum schooling	4423	0.342	0.474	0	1
A levels	4423	0.143	0.350	0	1
College training	4423	0.166	0.372	0	1
University education	4423	0.147	0.355	0	1
Education missing	4423	0.022	0.148	0	1
Married or de facto	4423	0.671	0.510	0	2
Number children	4423	1.372	1.110	0	6
Marital status missing	4423	0.273	0.445	0	1
Children info missing	4423	0.076	0.266	0	1
Workhours					
Not employed	4423	0.677	0.468	0	1
< 35 (part-time)	4423	0.127	0.333	0	1
35–48 (full-time)	4423	0.161	0.368	0	1
49+ (overwork)	4423	0.035	0.183	0	1

Note: Sample size refers to smallest possible sample in our main analysis using non-zero earnings as outcome variable ($N = 4132$) or for the decomposition analysis for earnings ($N = 4423$).

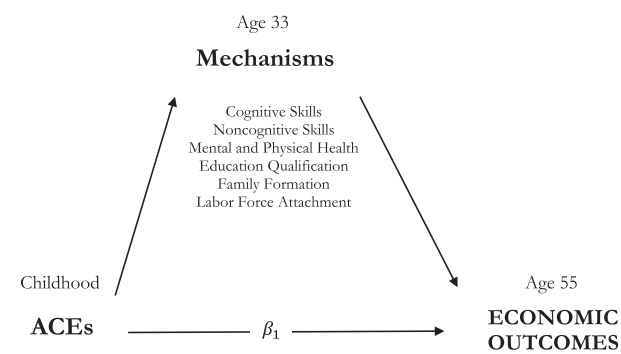


Fig. 1. Channels through which ACEs may affect lifetime economic outcomes.

To identify the likely channels, we use the same decomposition method proposed in Heckman and Pinto (2015) and applied in Heckman et al. (2013). This method decomposes the "treatment effect" of high-dose ACE into observable and unobservable components that explain the difference in outcomes between treatment and control groups. In a robustness check, we conduct the decomposition analysis using child neglect as a treatment indicator. Fig. 1 illustrates the possible channels through which ACEs may affect lifetime economic outcomes.

The starting point of the mediation analysis is the following equation of economic outcomes:

$$Y_d = k_d + \propto_d \theta_d + B_d X + \varepsilon_d, \tag{3}$$

where Y_d is the outcome of interest. Let Y_1 and Y_0 be the counterfactual outcomes when ACE equals 1 (high dose) and ACE equals 0 (no ACE or mild dose), respectively. The subscript d can take the value 0 or 1 to indicate whether the variable is "fixed" at treatment to flag people – at any given time – who had experienced ACEs compared with those who had not experienced ACEs.[7] k_d is an intercept, and θ_d captures all variables that are likely to mediate the relationship between ACEs and later-life economic outcomes as described in Fig. 1. We assume that there are specific young-adulthood outcomes θ_d that are influenced by ACEs and that produce the treatment effect. Therefore, the equation $\theta_d = D\theta_1 + (1 - D)\theta_0$ represents the counterfactual outcomes in young adulthood between the treatment and control group. X contains all variables that are not affected by ACEs because they occur before exposure.

[7] Here, fixing refers to manipulating treatment status by keeping everything else constant.

We assume that the outcomes are independent across participants conditional on observed characteristics X. ε_d is a zero-mean error term assumed to be independent of both X and θ_d.

Although the NCDS collected a vast array of young adult measures, we may not be able to capture all relevant outcomes in young adulthood that are affected by ACEs. These outcomes are summarized as unobservable characteristics. We therefore classify the potential mediating factors captured in θ_d into observable characteristics and unobservable characteristics as follows:

$$Y_d = k_d + \underbrace{\sum\nolimits_{j \sim J_p} \propto_d^j \theta_d^j}_{\text{Observed}} + \underbrace{\sum\nolimits_{j \sim J \setminus J_p} \propto_d^j \theta_d^j}_{\text{Not observed}} + \beta_d X + \widetilde{\varkappa_d}, \tag{4}$$

$$Y_d = \tau_d + \sum_{j \epsilon J_p} \propto_d^j \theta_d^j + \beta_d X + \widetilde{\epsilon_d}, \tag{5}$$

where $\tau_d = k_d + \sum_{j \epsilon J \setminus J_p} \propto_d^j \theta_d^j$ and $j \in J_p$ denotes a given mediating factor j within a set of factors J_p; $\sum_{j \epsilon J_p} \propto_d^j \theta_d^j$ are all factors for which we have measurements, and $\sum_{j \epsilon J \setminus J_p} \propto_d^j \theta_d^j$ are all mediating factors for which we do not have measurements. Under the assumption that ACE "treatment" affects young-adulthood outcomes but not the impact of such outcomes on later-life outcomes and the impact of the pretreatment variables X, we can further simplify this equation by dropping X out.

With this simplification, the treatment effect can be decomposed as follows:

$$E\left(Y_1 - Y_0\right) = (\tau_1 - \tau_0) + \sum_{j \epsilon J_p} \propto_d^j E(\theta_1^j - \theta_0^j), \tag{6}$$

We can interpret observed differences in later-life outcomes between the treatment and control group in terms of differences in mediating factors $E(\theta_1^j - \theta_0^j)$ and differences in unobservable factors $(\tau_1 - \tau_0)$, as captured by differences in the intercept. This method is a variation of a standard Blinder-Oaxaca decomposition analysis (Fortin et al., 2011).

Although uncertainty remains about which channels are the most likely ones through which a relationship between ACEs and later-life economic outcomes emerge, we focus on a standard set of intermediary factors identified elsewhere (Fletcher and Schurer, 2017; Heckman et al., 2013; Conti et al., 2016): cognitive and noncognitive skill development, health, education, labor supply, and marital and fertility decisions. Where possible, we measure these factors at the beginning of mid-age (age 33), when most cohort members are expected to have completed full-time education, their cognitive and noncognitive skill formation, formed families, and showed first signs of poor health in adulthood.

1 Cognitive skills: ACE may impair cognitive development and thus intelligence. We use mathematics and reading test scores at age 16 as a proxy for cognitive ability, the last measurement available after childhood.
2 Noncognitive skills: ACE may impair socioemotional abilities. We analyze these abilities by looking at internal locus of control tendencies (self-efficacy), the only available measure of noncognitive skills in mid-age, yet an important predictor of education, health, and labor market outcomes.
3 Health outcomes: ACE may impact health trajectories through problems with psychological developmental and immune health. As a proxy for health outcomes, we use a self-assessed measure that reports physical health problems (yes, no) and the Rutter Malaise Inventory for mental health, a 24-item index developed by Rutter et al. (1970), which is a short version of the 196-item Cornell Medical Index of Health Questionnaire. The Malaise Inventory has been widely validated to be accurate in identifying symptoms of anxiety and depression (see Johnston et al., 2014 for an application).
4 Education outcomes: ACE may directly impact educational attainment, because children may not be able to focus on school and fall behind. We use completed education levels as a proxy for educational attainment (Less than minimum schooling, minimum schooling, completing O-levels, completing A-levels, some college training, and university education).
5 Family formation decisions: ACE may impact the decision to form a family. Maltreatment experiences are characterized by a breakdown in trust between a caregiver and a child. Thus, a victim of maltreatment may struggle to build trusting relationships in adulthood. We proxy family formation decisions with marital status and the number of children.
6 Labor supply: ACE may impact early-adulthood labor supply. We proxy labor-supply decisions with part- or full-time employment measured by working hours. We categorize labor force status as follows: not employed (0 work hours); part-time work (<35 h), full-time work (35–48 h), and over-time work (>48 h). These categories are derived from official UK Government regulation of part-time, full-time and over-work.[8]

All remaining channels are captured by τ_d, and are thus considered unobservable factors.

5. Estimation results

5.1. Descriptive analysis

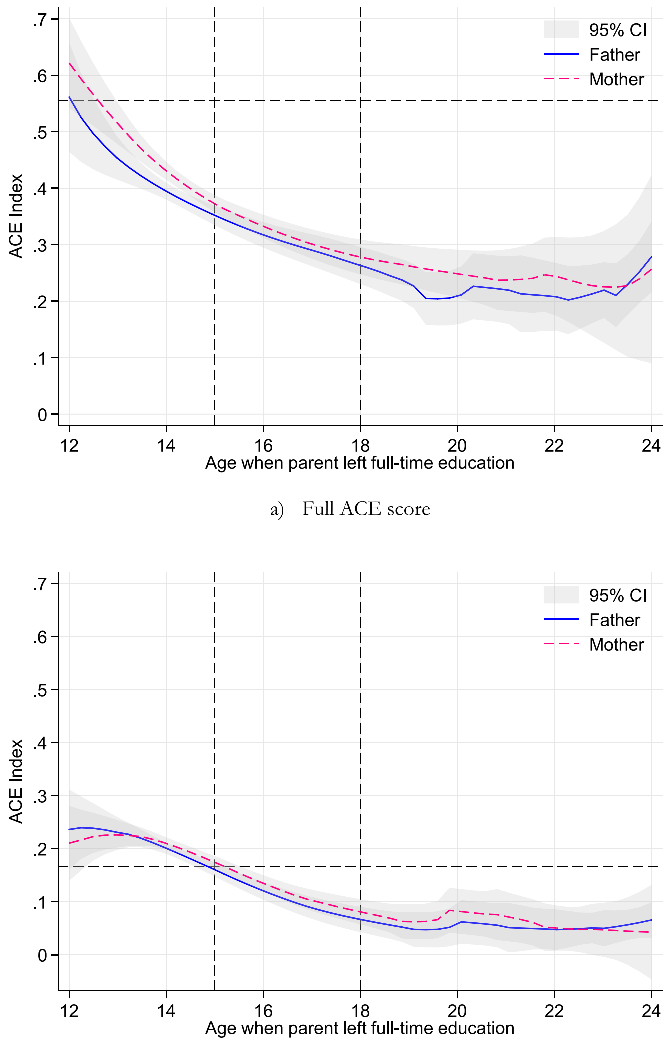
Table 1 provides summary statistics of the three economic outcome measures recorded at age 55 – net monthly earnings (logarithmized), the receipt of welfare payments, and responses about it being (very) hard to get by with financial resources – all ACE components, and all control variables.[9]

The average net monthly income in the sample is 3581 pounds for those with positive earnings ($N = 4132$), and 2270 pounds for everyone in the sample with information on earnings (including 0, $N = 6887$). Around 10% of the cohort members are classified as living in subjective poverty, and 17% are dependent on welfare payments. The average ACE is roughly 0.4, which implies that two out of five Brits born in 1958 experience at least one ACE. The maximum number of adverse events that a cohort member experienced is five. Of the full sample, 5% experienced at least two adverse experiences. Excluding separation as an ACE component, only 2% of cohort members experienced at least two ACEs, suggesting that the most common ACE is separation from parents. In fact, 25% of the cohort members experienced separation from their parents until age 16. In stark contrast, only 4% of cohort members experienced neglect (assessed by teachers) by age 11.

An important question is whether ACEs are just an alternative proxy for socioeconomic disadvantage. Fig. 2a indeed demonstrates the existence of a socioeconomic gradient in ACE but emphasizes that cohort members from more privileged backgrounds also endure ACEs. The figure depicts the bivariate correlation – estimated non-parametrically – between the number of ACEs (vertical axis) and parental education (horizontal axis) for both fathers (solid line) and mothers (dashed line). The vertical dashed lines depict the average age at which either parent leaves

[8] Work hours are heavily regulated in the UK. Part-time work is considered for work hours <35 h (see https://www.gov.uk/part-time-worker-rights). Full-time employment is legislated not to exceed 48 h (see https://www.gov.uk/maximum-weekly-working-hours), although some professions may require more work hours, e.g., industries where 24-h staffing is required, in the armed forces, emergency services or police, in security and surveillance, as a domestic servant in a private household, as a seafarer, sea-fisherman or worker on vessels on inland waterways where working time is not measured and you are in control (managing executive with control over your decisions).

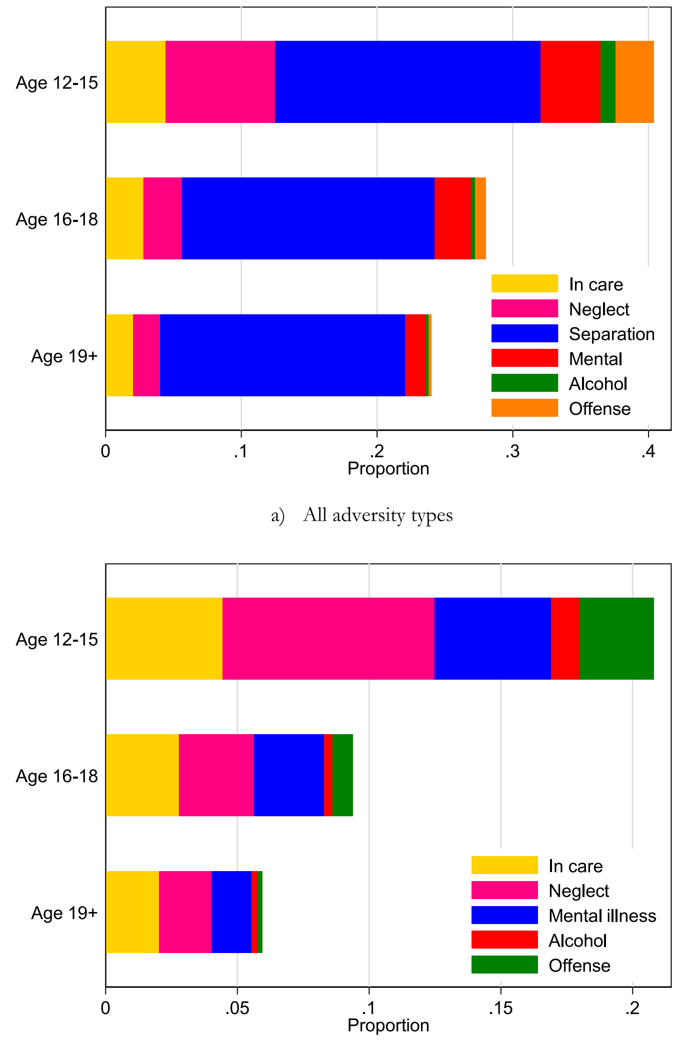
[9] Note, we present summary statistics for the smallest available estimation sample of the three outcome measures (positive earnings), to be transparent about the implied number of observations for rare events such as parental neglect or having been in foster care.



a) Full ACE score

b) ACE score excluding separation from parents

Fig. 2. Relationship between parental education and ACE score.



a) All adversity types

b) Adversity types excluding separation from parents

Fig. 3. Proportion of type of adverse events, by father's education level.

full-time education (around age 15) and the horizontal dashed line depicts the average number of ACEs in the sample (0.55). The graph shows that cohort members whose parents leave full-time education between the age of 12 and 14 (less than minimum schooling) weather more ACEs than the sample average (around 0.5), while cohort members whose mothers leave full-time education most likely with a university degree (>18 years) encounter around 0.2 ACEs. This means that at one in two children from low-SES backgrounds withstand at least one ACE, while only one in five children from higher-SES backgrounds do so. We obtain similar socioeconomic gradients in ACE when using parental occupational categories or income bands (see Figures A1 and A2 in the appendix). Because separation from parents is such an important contributor to overall ACE, Fig. 2b shows the bivariate relationship between ACEs and parental education levels when the separation component is omitted from the ACE index. We demonstrate that the education gradient in ACE remains the same, though less extreme, when removing this component.

Fig. 3a breaks down the education gradient in ACE by the individual components that contribute to the ACE score (here illustrated to father's education level). For clarity, we show the prevalence of each ACE component within three groups of father education: those who leave school at age 19+ (university degree, approx. 5%); between age 16 and 18 (approx. 20%); and at age 15 or younger (minimum schooling or less, approx. 75%). Parental separation is the main contributor to ACE in each education category, making up between 75% of total ACEs for the least disadvantaged cohort members (fathers with university education), and 50% for the most disadvantaged cohort members (fathers minimum schooling or less).

Neglect occurs regardless of one's socioeconomic status, however it is over-represented in the most disadvantaged group (where neglect comprises 20% of total ACEs compared with 10% for the somewhat-advantaged middle group and 8% for the most well-off group). Alcohol problems and criminal offences contribute least to ACE, which may be due to systematic under-reporting in the survey. Fig. 3b demonstrates the proportions when excluding separation from parents, to better highlight the importance of foster care experiences, neglect and mental health problems contributing to the ACE score across all socioeconomic strata. Our conclusions on the socioeconomic gradient of ACE components are not sensitive to the measure of socioeconomic status (see Fig. A3 in the appendix which show the gradients by father's occupational category and income bands).

5.2. Systematic attrition

An important limitation of our analysis is that 60% of cohort members (around 11,100 of the original birth cohort for whom we have some information on their ACEs) drop out of the NCDS at some point, and

Table 2
Comparisons of means between potential final estimation sample and dropout sample.

Childhood Adversity	Estimation sample N[a]	Estimation sample Mean	Dropout sample N	Dropout sample Mean	Factor p-value[b]
ACE index[c]	7450	0.370	11,108	0.680	1.8***
High-dose ACE	7450	0.053	11,108	0.085	1.6***
ACE index (excl. separation)	7450	0.137	11,108	0.185	1.3***
High-dose ACE w/o separation	7450	0.021	11,108	0.032	1.5***
Child in care, age 7–16	7450	0.029	10,993	0.047	1.6***
Child neglect, age 7–11[d]	6634	0.051	10,208	0.070	1.4***
Separation from parents, age 7–16	7445	0.233	11,051	0.498	2.1***
Mental illness in family, age 7–16	7424	0.036	10,506	0.041	1.2*
Alcohol abuse in family, age 7	6458	0.008	9262	0.009	1.2
Offender in family, age 7–11	7417	0.021	10,495	0.029	1.4***
Less than minimum schooling[e]	5514	0.132	5577	0.168	1.3***
Income-poor household[f]	5629	0.016	5802	0.035	2.2***

[a] Estimation sample when using welfare dependence as outcome variable.
[b] Factor refers to the increased risk of ACE of dropout sample relative to final estimation sample; p-value refers to t-test statistics on a test for equality of means between estimation and full available sample.
[c] ACE Index is calculated as sum of six separate adverse childhood experiences. ACE Index would be missing if cohort members had missings on each ACE component.
[d] Child neglect is based on a teacher assessment referring to appearance.
[e] < minimum schooling refers to father's age when left full-time education (<14 years of age).
[f] Income poor household refers to father's income in 10th percentile of income distribution.
* $p < 0.01$.** $p < 0.05$.
*** $p < 0.01$.

thus we do not observe their age-55 outcomes. However, we observe for 9262–11,108 cohort members, who drop out, components of their earlier-life ACEs. The attrition in our sample is not important if it occurs at random. However, systematic attrition is more likely here, meaning that the existence of ACEs relates to the probability of a subject dropping out of the sample. Systematic attrition could lead to either an upward or downward bias in our estimated regression coefficients. Therefore, to test whether systematic attrition is an issue, we investigate the differences in means of ACE components between our estimation sample and the cohort members who drop out after the childhood sweeps and present.

Table 2 reveals that the dropout sample (Column (2)) is 1.6 times as likely to experience high-dose ACE than the final estimation sample (Column (1)). More specifically, cohort members in the final estimation sample have a 5% probability of having high-dose ACE in childhood versus 9% of cohort members in the dropout sample – a statistically significant difference of four percentage points. The difference in risk is 2 versus 3% when excluding separation from parents from the ACE Index. The risk of having ever been in care, ever been assessed as neglected by the teacher, and ever experienced parental separation is between 1.5 and 2.1 times greater in the dropout sample. For instance, the disparity for neglect is 5 versus 7%, respectively; and for separation, 23 versus 50%. Additionally, children in the dropout sample are more than twice as likely to come from an income-poor household (father's income is in bottom 10th percentile) and 1.3 times more likely to have a father with less than minimum schooling (left full-time education before age 14). There are no statistically significant difference in the risk of exposure to parental mental health problems or alcohol abuse.

If the dropout sample is also more likely to respond negatively to ACEs in the future – which is reasonable to assume given the heavier exposure and their worse socioeconomic starting point – then we are likely to underestimate the relationship between ACEs and later-life economic outcomes. Under this assumption, we conclude that selective attrition, at its worst, would lead to a downward bias of our estimates.

5.3. Estimating the economic burden of ACE

We present the estimation results of the relationship between ACE and economic outcomes measured at age 55. Table 3 reports bivariate and multivariate estimation results, wherein columns 1, 3, and 5 report bivariate coefficients (no controls, Eq. (1)), and columns 2, 4, and 6 report multivariate coefficients (full set of pre-treatment control variables, Eq. (2)). Each row represents a separate regression model with different dependent variables that measure ACE. Model 1 reports the coefficient of interest for the continuous ACE measure as a dependent variable (bound between 0 and 5). Model 2 uses a binary index that indicates whether the individual experienced high-dose ACE. Models 3 to 8 use each component of the ACE index as dependent variables. Models 9–13 present a robustness check to Models 1 and 2 by excluding separation from the ACE index (9. and 10.), or by using an ACE Index that allows for unequal weighting (11. and 13.). The ACE Index in Models 11. and 13. is standardized to mean 0 and standard deviation 1. For comparison, we also present the result of Model 1. with the same standardization (12.) Full estimation results are reported in Tables A1 and A2 in the appendix. Table A1 demonstrates coefficient sensitivity to adding each block of pre-treatment control variables individually. Estimates are considered as statistically significant for p-values smaller than 0.05 ($p < 0.05$).

We find a statistically significant association between ACEs and all economic outcomes, independent of whether we control for confounding variables or not. A one-unit increase in ACE is associated with a 12.5% reduction in (log) net earnings at age 55 (column 1). Once controlling for the full set of pre-treatment variables, this disparity falls to 8.9% ($p < 0.01$). The estimated earnings penalty is most sensitive to the inclusion of a father's occupational class, with a drop from 12.5 to 10.0 log percent (see column (7), Table A1 in appendix). The estimation results are not sensitive to imputing missing observations.[10] We find no statistically significant relationship between ACE and the probability of positive earnings or unemployment (see Table A2, appendix), which suggests that ACE affect the intensive not the extensive margin of earnings.

[10] If we had restricted the sample on no missing observations, our sample size would have been reduced to 2147 individuals for positive earnings. In this case, the treatment effect would have been −.096 ($p < 0.05$). For welfare dependence the sample would have been 3798 individuals and the resulting treatment effect would have been .064 ($p < 0.01$). For subjective poverty, the sample would have been 3764 individuals and the treatment effect would have been .040 ($p < 0.01$). Hence, our conclusions do not change.

Table 3
Relationship between ACE and economic outcomes at age 55.

	Net log income		Welfare dependence		Subjective poverty	
	No controls	With controls	No controls	With controls	No controls	With controls
Panel A: Dose ACE						
1. ACE index	−0.125***	−0.089***	.045***	.043***	.033***	.027***
(0–6)	(0.026)	(0.028)	(0.007)	(0.008)	(0.005)	(0.006)
2. ACE > 1	−0.410***	−0.335***	.110***	.098***	.058***	.036**
(0,1)	(0.076)	(0.076)	(0.019)	(0.021)	(0.015)	(0.016)
Panel B: By ACE component						
3. In (foster) care	−0.239**	−0.166*	.095***	.094***	.036*	.016
(0,1)	(0.099)	(0.097)	(0.026)	(0.027)	(0.021)	(0.021)
4. Neglect	−0.274***	−0.219***	.137***	.118***	.061***	.043**
(0,1)	(0.086)	(0.084)	(0.021)	(0.021)	(0.017)	(0.017)
5. Separation	−0.089**	−0.052	.029***	.023*	.025***	.015
(0,1)	(0.040)	(0.044)	(0.010)	(0.012)	(0.008)	(0.010)
6. Mental illness	−0.308***	−0.203**	.060**	.044*	.070***	.056***
(0,1)	(0.091)	(0.087)	(0.024)	(0.024)	(0.019)	(0.019)
7. Alcohol abuse	−0.259	−0.147	.073	.048	.068	.044
(0,1)	(0.209)	(0.200)	(0.054)	(0.055)	(0.043)	(0.043)
8. Offender	−0.288**	−0.083	.118***	.092***	.139***	.119***
(0,1)	(0.119)	(0.116)	(0.031)	(0.031)	(0.024)	(0.025)
Panel C: Robustness checks						
9. ACE index (0–5)	−0.198***	−0.132***	.073***	.064***	.050***	.039***
(excl. separation)	(0.040)	(0.039)	(0.010)	(0.011)	(0.008)	(0.008)
10. ACE > 1 (0, 1)	−0.523***	−0.373***	.145***	.120***	.093***	.067***
(excl. separation)	(0.121)	(0.117)	(0.030)	(0.031)	(0.024)	(0.024)
11. ACE unequal	−0.078***	−0.049***	.029***	.025***	.022***	.018***
weights factors (std)	(0.017)	(0.017)	(0.004)	(0.005)	(0.003)	(0.004)
12. ACE index (std)	−0.077***	−0.055***	.029***	.028***	.022***	.018***
for comparison	(0.017)	(0.018)	(0.004)	(0.005)	(0.003)	(0.004)
13. ACE unequal	−0.269***	−0.184***	.101***	.090***	.064***	.049***
weights > mean (0,1)	(0.055)	(0.053)	(0.014)	(0.015)	(0.011)	(0.012)
Mean	8.183		0.173		0.099	
Observations	4132		7450		7384	

Note: Dependent variables are: Columns (1) and (2) log of net monthly salary for individuals with positive earnings, more than 20 pounds per month and less than 1,000,000 (dropped: 3 observations). Columns (3) and (4) welfare dependence = 1 if an individual receives any government transfers including other forms of income, benefits, or tax credits, and 0 otherwise. Columns (5) and (6): subjective poverty = 1 if an individual currently finds it quite or very difficult to manage financially, and 0 otherwise: comfortably, living alright, or just getting by). Columns (2), (4), and (6) include a full set of early childhood control variables: female, low birth weight, premature birth, mother's age at birth, birth order, father's social class, father's and mother's age when he/she left full-time education, father's income bands (missing for 42% of cohort members). Full estimation results are reported in Tables A1 and A2 in the appendix. Standard errors are reported in parentheses. Significance levels:
*** 0.01.
** 0.05.
* 0.10.

The earnings gap increases to 34% when considering high-dose ACE (Model 2., Table 3). This relationship is robust when excluding separation from the ACE index (Models 9. and 10.) or when using an ACE measure that allows for unequal weighting its components.[11] The key contributor to the negative relationship between earnings and ACE, in terms of magnitude and statistical significance, is the experience of neglect as reported by teachers (Model 4.). The multivariate correlation coefficient indicates an earnings reduction of 22% ($p < 0.01$) due to neglect. The only other significant component is mental illness (Model 6.), which is associated with an earnings penalty of 20.3 log percent ($p < 0.05$).

Similarly, ACE is also positively associated with both welfare dependence and subjective poverty. A one-unit increase in ACE is associated with a 4.3 percentage point increase in the likelihood of being welfare dependent, ruling out the influence of pre-treatment control variables. Relative to the base probability of 17.3%, this implies an increase in this probability of 25%. This probability increase is again substantially larger for cohort members with high-dose ACE (10.0%age points, or 60% higher than the base probability). These findings are robust to alternative parameterization of the ACE Index (Panel C). Consistent with our findings for earnings, the experience of neglect is the strongest predictor of welfare dependence (11.8 percentage points, $p < 0.01$), dwarfing the impact of any other ACE component. The second and third largest and significant contributors are family member offender (Model 8.) and in foster care (Model 3.), which both are associated with welfare dependence in the magnitude of 9 percentage points ($p < 0.01$).

We also find a statistically significant relationship between ACE and subjective poverty. A one-unit increase in ACE is associated with a 2.7 percentage point increase in the probability of subjective poverty ($p < 0.01$), which implies a 27% increase from the base probability. High-dose ACE is also significantly associated with subjective poverty, increasing this probability by 36% relative to the base probability ($p < 0.05$). Again, these findings are robust to alternative parameterization of the ACE Index (Panel C). The significant predictors of subjective poverty are the following, in order of magnitude: family member offender (12 percentage points, $p < 0.01$); family member with mental illness (5.6 percentage points, $p < 0.01$), and neglect (4.3 percentage points, $p < 0.05$).

Finally, we explore whether the relationship between high-dose ACE or neglect and economic outcomes differs across socioeconomic status. If our hypothesis, that ACE should affect children independent of their socioeconomic status, is true then we should observe earnings penalties of ACE across the income spectrum. In Table 4 we present separate

[11] A one standard deviation increase in the unequal-weighting ACE measure is associated with a 5 log percent reduction in earnings (Model 11). In contrast, a one standard deviation increase in the equal-weighting ACE is associated with a 5.5 log percent reduction in earnings (Model 12).

Table 4
Heterogeneity of treatment effect by father's income.

	(1) Income poor	(2) Middle class	(3) Income rich	(4) Income missing
Panel A High-dose ACE	−0.388**	−0.153	0.259	−0.367***
(with separation)	(0.17)	(0.22)	(0.28)	(0.11)
Panel B High dose ACE	−0.173	−0.202	−0.0144	−0.592***
(without separation)	(0.25)	(0.29)	(0.44)	(0.17)
Panel C High dose ACE	−0.235**	−0.135	−0.0728	−0.155**
(unequal weighting)	(0.11)	(0.12)	(0.14)	(0.08)
Panel D Neglect	−0.116	−0.274	−0.174	−0.321**
	(0.17)	(0.18)	(0.24)	(0.13)
Observations	791	862	763	1376

Note: Each panel represents estimation results from a separate regression in which non-zero earnings is the dependent variable. Each column presents separate regression results by income class of father. Each model controls for the same set of control variables as presented in Table A1, appendix. Income poor is defined as incomes in bottom third of the income distribution; Middle class is defined as incomes between bottom third and upper third of the income distribution; Income rich is defined as incomes in the top third of the income distribution (approximately). Income missing refers to father's income information is missing. Standard errors are reported in parentheses. $^{*}p < 0.10$.
** $p < 0.05$,.
*** $p < 0.01$.

estimation results on non-zero earnings for different income groups (father income poor, father middle-income class, father income rich, father income information missing) and different ways to construct high-dose ACE (Panels A–C) and neglect (Panel D). We observe that among the income poor or for those where income information is missing, high-dose ACE is always negatively associated with earnings and in most cases the estimated association is statistically significant. The estimated earnings penalty for children from –income poor households ranges between 12 (neglect, Panel D) and 40 log percent (high-dose ACE, equal weighting, Panel A). For individuals where income information is missing the earnings penalty of high-dose ACE ranges between 16 (high-dose ACE unequal weighting, Panel C) and 60 log percent (high-dose ACE without separation, Panel B). There is also an earnings penalty of high-dose ACE for middle-income class children, with penalties lying between 14 and 27 log percent, and children of the income-rich households for all specifications, except for when considering the high-dose ACE measure that includes separation from parents (Panel A), but these are never statistically significant. Nevertheless, the income penalties of neglect are sizable for all groups: 12 log percent for the income poor, 27.4 log percent for middle-income class children, 17.4 log percent for income-rich children, and 32 log percent for those where father's income information is missing. These tentative, because inefficiently estimated, results demonstrate that ACE indeed affect most negatively children from income-poor families (or families that do not report father's income), but neglect experiences result in earnings penalties for children from all socioeconomic backgrounds.

5.4. Channels through which ACE may affect lifetime economic outcomes

So far, we have shown that ACE is significantly and robustly associated with earnings and increased welfare dependence as well as subjective material poverty. We have furthermore demonstrated that neglect experiences is the key contributing factor to the significant association between ACE and later-life economic outcomes, and that this is true for children from all socioeconomic backgrounds.

In what follows, we identify the channels through which early-life adverse experiences impact later-life economic outcomes. To do so, we decompose the raw outcome differences observed between cohort members with and without ACEs into differences due to observable characteristics measured at the start of mid-life – including human and health capital, and family formation decisions – and differences in unobservable characteristics (see Eq. (6)). To distinguish between a "treatment" and "control" group, we use the binary measure of high-dose ACE. Treatment is defined as having two or more ACEs, and that is compared against zero or one ACE.[12]

Using the same imputation method as previously, we have an estimation sample for each age-55 outcome measure of 4432 observations for earnings, 7883 observations for welfare dependence and 7806 observations for subjective poverty.[13] In this slightly different estimation sample, the raw differences between the treatment and control group are slightly less pronounced. For instance, the raw difference in net earnings is 31.3 log percent, that of welfare dependence is 9.5%, and the disparity in subjective poverty is 5.5%.

We decompose these observed raw differences into the relative contribution of the following observable characteristics as witnessed at the beginning of mid-age (age 33), if available, which we bundle into the following categories: (i) cognitive skills: math and reading test scores (only available at age 16); (ii) noncognitive skills: indicator variables for different levels of locus of control; (iii) health: indicators for mental and physical health problems; (iv) education: indicator variables for highest level of completed education; (v) family: whether married and the number of children; and (vi) labor supply: indicator variables for type of employment. All other remaining differences are thought to be attributed to unobserved characteristics.

Fig. 4 summarizes the decomposition analysis for all components (full estimation results are presented in the appendix, Table A3). Statistically significant contributions of blocks of variables are based on an *F*-test for a joint significance of all variables within each block. We will discuss statistically significant contributions for p-values <0.05. The first thing to note L is that observable characteristics in young adulthood explain more than one half of the observed earnings differences at age 55 between cohort members with and without high-dose ACE. Less than 45% of the earnings gap is due to unobserved characteristics. Allowing for unequal weighting of the components in the ACE measure increases the contribution of observed factors to the ACE impact to more than three quarters (see Fig. A4 and Table A5, appendix), but identical results are obtained when excluding separation from parents from the ACE Index (see Fig. A5 and Table A6, appendix).

[12] We emphasize that we use the terms treatment and control group not to imply random variation in assignment but to distinguish between two groups that can be compared.

[13] We impute missing values for Age 16 cognitive test scores, marital status and number of children information missing, and education information missing. The proportion of missings is reported in Table 1.

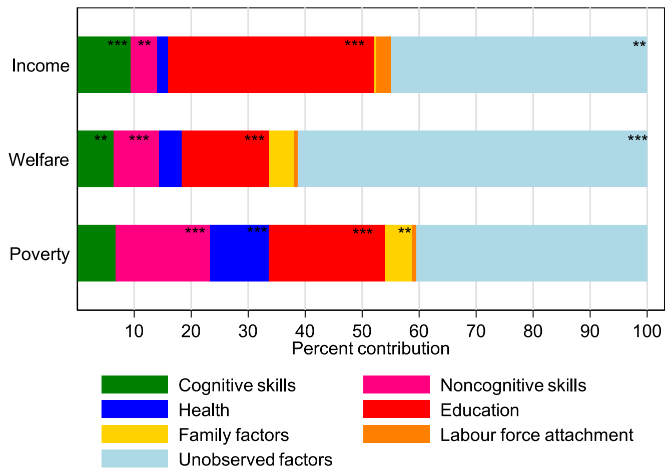


Fig. 4. Mechanism analysis-contribution of life domains to predicted difference in age 55 outcomes between high-dose ACE and low-dose/no ACE individuals.

Each bar represents the decomposition of the treatment effect of neglect on a specific economic outcome recorded at age 55 (earnings, welfare dependence, subjective poverty) into the impact of neglect on young adulthood outcomes (cognitive skills, noncognitve skills, physical and mental health, education levels, family factors (marital status, number of children), labor force attachment (no employment, part-time, full-time, over-time) and unobservable factors. Each bar stacks the scaled, absolute percent contribution of age 33 outcomes (age 16 for cognitive skills, age 50 for labor force attachment) to the treatment effect of high-dose ACE. Welfare dependence is a dummy variable that takes the value 1 if an individual is welfare dependent, and 0 otherwise. Poverty is a dummy variable that takes the value 1 if an individual reports that it is hard to get by financially, and 0 otherwise. High-dose ACE is a dummy variable that takes the value 1 if cohort member experienced two or more adverse events in childhood by age 16. The predicted difference in outcomes between control and treatment groups are, respectively: Earnings: +31.3% (significant at the 1% level), Welfare dependence: −9.5% (significant at the 1% level); and Subjective poverty: −5.5% (significant at the 1% level). The estimation sample sizes are, respectively: Earnings $N = 4423$, Welfare: $N = 7883$, Subjective poverty: $N = 7806$. Full estimation results are presented in Table A3 in the appendix. Significant contributions of block of variables are indicated by stars: ***0.01 **0.05. Robustness check results using high-dose ACE measure with unequal weighting of individual components are reported in Fig. A4 and Table A5 in the appendix.

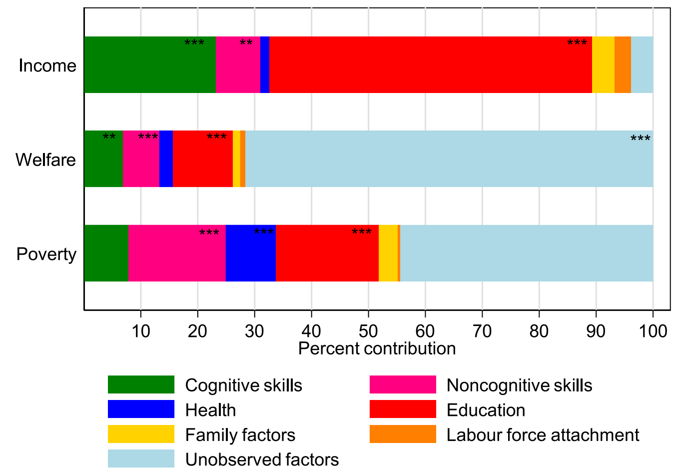


Fig. 5. Mechanism analysis-contribution of life domains to predicted difference in age 55 outcomes between neglected and not neglected individuals.

Each bar represents the decomposition of the treatment effect of neglect on a specific economic outcome recorded at age 55 (earnings, welfare dependence, subjective poverty) into the impact of neglect on young adulthood outcomes (cognitive skills, noncognitve skills, phys./mental health, education levels, family factors, labor force attachment) and unobservable factors. Each bar stacks the scaled, absolute percent contribution of age 33 outcomes (age 16 for cognitive skills, age 50 for labor force attachment) to the treatment effect of high-dose ACE. Welfare dependence is a dummy variable that takes the value 1 if an individual is welfare dependent, and 0 otherwise. Poverty is a dummy variable that takes the value 1 if an individual reports that it is hard to get by financially, and 0 otherwise. Child neglect is a dummy variable that takes the value 1 if the teacher assessed the child to appear malnourished or dirty by age 11. The predicted difference in outcomes between control and treatment groups are, respectively: Earnings: +22.3% (significant at the 5% level), Welfare dependence: −11.3% (significant at the 1% level); and Subjective poverty: −5.7% (significant at the 1% level). The estimation sample sizes are, respectively: Earnings $N = 4423$, Welfare: $N = 7883$, Subjective poverty: $N = 7806$. Full estimation results are presented in Table A4 in the appendix. Significant contributions of block of variables are indicated by stars: ***0.01 **0.05.

The biggest contributor to observed earnings differences are educational outcomes by age 33, which explain 36% of the earnings gap ($p < 0.01$). The second and third largest and significant contributors are cognitive skills measured at age 16 (9.4%, $p < 0.01$) and noncognitive skills measured at age 33 (4.7%, $p < 0.05$). Health, family formation and labor supply factors do no significantly contribute to the earnings gap.

The variation in welfare dependence associated with ACEs is slightly less well explained by differences in observable characteristics by age 33. Differences in unobservable factors explain more than 60% of the welfare dependence gap. Yet, it is again human capital differences that explain almost 30% of the welfare dependence gap (cognitive skills: 6.4% ($p < 0.05$), noncognitive skills 8.0% ($p < 0.01$), and education: 15.4% ($p < 0.01$)). A slightly different picture emerges for subjective poverty gaps by high-dose ACE exposure. First, unobserved factors do not contribute significantly to the observed differences in the probability of subjective poverty. Second, the channels through which ACE impacts upon later-life poverty experiences include also health (10.3%, $p < 0.01$) and family factors (4.8%, $p < 0.05$), in addition to noncognitive skills (16.6%, $p < 0.01$) and education (20.3%, $p < 0.01$). Conclusions about the importance of observable characteristics in explaining the ACE gap in economic outcomes are not sensitive to the way we construct the ACE measure (see Figs. A4 and A5 in the appendix).

Child neglect is an important reason for child protection agencies, if substantiated through a court, to remove a child from its home. Given its strong and significant predictive power in economic outcomes (see Model 4., Table 3), we repeat the decomposition analysis using neglect as the "treatment" indicator. The results are presented in Fig. 5, while full estimation results are reported in an appendix (Table A4). In this estimation sample, the difference in raw earnings between those who were flagged by their teacher as neglected and those who were not is around 23%. Strikingly, almost 100% of the earnings penalty due to neglect is explained by differences in human capital attainment by age 33. Differences in cognitive skills and noncognitive skills explain 23% ($p < 0.01$) and 8% ($p < 0.05$) of the earnings penalty, respectively, while differences in educational attainment by age 33 explain almost 60% ($p < 0.01$). Again, differences in welfare dependence and subjective poverty are less well explained by differences in observable characteristics (72% and 44%, respectively). Again, cognitive and noncognitive skills are the key factors explaining differences in the probability of welfare dependence, with a combined contribution of 24% to the difference. Noticeable is the dominant role of noncognitive skills and health for subjective poverty, which combined explain almost 20% of the difference in the probability of subjective poverty. Education explains almost another 20% of the difference (Fig. 5).

In summary, we can say that early adulthood human capital – cognitive skills, noncognitive skills and education – explains between 50 and 90% of the earnings gap observed between those who experience high-dose ACE and neglect, respectively; between 25 and 30% in the welfare dependence gap; and around 40% in the subjective poverty gap. Young adulthood health significantly contributes about 10% to the later life subjective poverty gap, mainly because of the influence of mental health problems (see Table A4, appendix), but does not significantly contribute to the earnings and welfare dependence gap.

6. Discussion and conclusion

This study quantifies the degree to which early-life adverse childhood experiences (ACEs) are associated with later-life economic outcomes; it identifies the core components of adversity that are linked with economic outcomes; and it shows the likely mechanisms through which this link is established. This study is built upon the assumption that what matters for the life trajectories of children is not only lack of access to income or educational resources but also confrontation with negative, chronic life events during childhood. Such an assumption has important implications, because it presents the idea that some children from more disadvantaged families are not at risk of later-life disadvantage, and – crucially – that some children in better off families are very much at risk of later-life disadvantage.

Using high-quality British cohort data, this is one of the first studies to quantify the earnings disparity for people with ACEs and the role of adverse experiences in later-life welfare dependence and (subjective) poverty. Such obstacles – which include out-of-home care, neglect, separation from parents, and a host of other negative experiences – occur disproportionately in economically disadvantaged families, but they exist within privileged households as well. We estimate a later-life earnings disparity of 34% for children with high-dose (two or more) ACEs, that increases to even 37% when excluding separation from parents (the most common ACE component). Strong associations were similarly found for both welfare dependence and subjective poverty. It is important to state that these associations are robust to different parematrization of the ACE index and hold over and above the confounding influence of parental education, occupation, income and household overcrowding.

Of all the components in our ACE index, teacher-assessed neglect yields the strongest association with age-55 earnings (22%). Although these findings cannot be interpreted as causal, they suggest that what a teacher observes is a powerful predictor of lifetime outcomes – and they carry important implications for policy makers. We demonstrate that the earnings penalty of neglect is almost fully explained by differences in human capital attainment – cognitive and noncognitive skills, and educational achievement – by age 33. These are channels that have been highlighted elsewhere to explain how influential early childhood programs such as the Perry Preschool program boosted adulthood outcomes (e.g., Heckman et al., 2013; Conti et al., 2016).

It is hard to explain why teacher-assessed neglect stands out so prominently in our analysis. One explanation could be that most other components in our analysis are more strongly associated with socioeconomic status than teacher-assessed neglect. As we have demonstrated, once conditioning on a full set of control variables, the significant associations between earnings and foster care, separation from parents, or offender in family are significantly reduced in magnitude and they are no longer statistically significant. The only other component of our ACE index for which this is not true are mental health issues, which is the second strongest predictor of later-life earnings after neglect. The latter finding is consistent with previous evidence (Johnston et al., 2013, and references therein). It could thus be that what the teacher observes as neglect is a good measure for when parents are struggling to attend to their children's needs. Such struggle may be the result of increased levels of stress or mental illness (see Duncan et al., 2017 for a review of such arguments).

Perhaps the most surprising finding is that alcohol problems in the household have no impact whatsoever on any of the later-life economic outcomes. Parental alcohol abuse has been shown to have substantial negative effects on children's wellbeing, one of the reasons why it is a key policy target in Britain (Houses of Parliament, 2018). One explanation for this lack of evidence in our analysis is that alcohol problems were only reported at Age 7, and thus alcohol problems may be dramatically underreported in our sample.

The key limitation of our study is that we cannot interpret our findings as causal even though we control for a significant number of early-childhood factors, including health at birth, parental socioeconomic status, overcrowding in the household and birth order. Nevertheless, we cannot say for sure that if cohort members had not experienced ACEs they would earn similar salaries or face similar rates of welfare dependence as cohort members who did not. In other words, there may be unobservable factors that occur in the life of the child between age 7 and 16 that correlate with one of the ACE components and affect health and human capital accumulation, thus shaping later-life economic outcomes. For example, a factor could be parental cognitive ability, which we only measure through approximations (parental education, father's occupational status, father's income). One way to overcome such a problem is to use siblings- or twin-fixed-effects methodologies that more carefully control for fixed family factors. These methods are used in Fletcher and Schurer (2017), Currie and Tekin (2012), and Slade and Wissow (2007) to identify the causal impact of maltreatment experiences on personality, crime, and education in young adulthood, respectively. Unfortunately, the NCDS does not provide siblings information.

Another important limitation of our study is that, although we initially have ACE information on over 18,400 cohort members at sweep 0 (age 0–1), our final estimation sample is greatly reduced by sample attrition due to systematic dropout. In a descriptive analysis of comparing ACEs and pre-treatment covariate means between final members and dropouts demonstrates that we lose cohort members with higher ACE rates as well as those from poorer socioeconomic backgrounds in childhood. If these same cohort members respond most strongly to the experience of ACEs in terms of health and human capital accumulation and labor market outcomes, then we are likely to underestimate the impact of ACEs on later-life economic indicators. For these reasons, we interpret our estimation results as a conservative estimate.

Since we find that ACEs are significantly associated with later-life economic outcomes, independent of socioeconomic status, this research contributes to a discussion about the multidimensionality of childhood poverty. Standard definitions are based on disposable household income thresholds, adjusting for family size and composition (Roosa et al., 2005; Whiteford and Adema 2007; Adamson, 2012). Our findings make clear that childhood poverty is much more complex and that many children may not be flagged as in need if poverty is solely defined on the basis of income. One recommendation of our study could be to better resource child protective services to be able to be at the forefront of battling childhood adversity. A few recent economic evaluations calculated that non-fatal child maltreatment has an estimated average lifetime cost of US$210,012 per victim in the US (Fang et al., 2012); of £89,390 (US$127,000) in the UK (Conti et al., 2017); and of A$176,437 (US$142,125) in Australia (McCarthy et al., 2016). Hence, large public savings may be achieved if children exposed to maltreatment were targeted and nurtured early on. Although child protective services are very expensive, and case workers are often overwhelmed by the complexity of the family dynamics they work with (Ferguson, 2016), more can be done to reduce adverse experiences and inequality among children as well as the vicious cycle of intergenerational maltreatment (Schelbe and Geiger, 2017). Another potential avenue for policymakers to support families and protect children could be to direct resources to parenting interventions in primary care (see Brockmeyer et al., 2016 and references therein) or family-home visiting programs (see Huston, 2011 and references therein). Putting children at risk on a path of health and success in life might therefore start with thinking outside the cash-transfer box.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.labeco.2019.06.007.

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