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Childhood Adversities as Determinants of Cardiovascular Disease Risk and Perceived Illness Burden in Adulthood: Comparing Retrospective and Prospective Self-Report Measures in a Longitudinal Sample of African Americans

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Abstract

A large body of evidence suggests that exposure to childhood adversities increases risk for poor quality physical health in adulthood. Much of this evidence is based on retrospective measures which are believed to be contaminated by the limitations and biases of autobiographical memory. Using longitudinal data on 454 African Americans (61 percent female) this study examines the corroboration between prospective and retrospective measures of childhood adversities gathered approximately two decades apart, and the relative ability of the measures to predict self-reported

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Compliance with Ethical Standards

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Ethical Approval Standards of ethical responsibility have been followed. None of the findings in this paper have been published elsewhere. All authors read and approved the final manuscript. The order of authorship reflects the relative level of contribution make by each of the authors. All procedures performed in studies involving human participants were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964
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illnesses and a biomarker of 30-year cardiovascular disease risk. Comparisons indicated that the retrospective and prospective measures demonstrated weak convergence and did not provide completely equivalent information about self-reported adverse childhood experiences. A series of regression models indicated that the two measures of adversities exhibited similar associations with the cardiovascular disease biomarker but divergent associations with self-reported illnesses. Furthermore, both the prospective and retrospective measures simultaneously predicted cardiovascular disease risk in adulthood. That the prospective measure did not significantly predict perceived illnesses after adjusting for the retrospective measure is evidence that childhood adversities predict self-reported health burden insofar as respondents remember those adversities as adults. The findings provide evidence that retrospective self-report measures of childhood adversities do not closely converge with prospective measures, and that retrospective measures may not provide valid estimates of the association between childhood adversities and perceived illnesses in adulthood.

Keywords

Adverse childhood experiences; Physical health outcomes; Cardiovascular disease risk; Retrospective and prospective measurement; Longitudinal methods

Introduction

A growing body of research has found that people exposed to childhood adversities have a heightened vulnerability to chronic diseases and illnesses, metabolic syndrome, accelerated aging, inflammation and premature mortality (Gilbert et al. 2015; Miller, Chen, and Parker 2011). The vulnerability produced by childhood adversities is not only enduring but also pervasive across multiple bodily systems (Danese et al. 2009). These findings have attracted considerable interest from both researchers and public health advocates (e.g., the American Heart Association) because of their significant potential to unlock clues about the social determinants of physical health outcomes in adulthood (Suglia et al. 2018).

Most studies on the long-term reach of early life experiences employ retrospective designs, whereby adults concurrently report their physical health conditions and exposure to a variety of adverse childhood events. Researchers have raised questions about the validity of this evidence because retrospective reports are susceptible to the limitations and biases of autobiographical memory (see, Goltermann, Opel, and Dannlowski 2019). At issue is whether adults tend to remember the actual details of their childhoods or, instead, if they report reconstructions of what must or may have occurred. Because of these methodological concerns, researchers have probed the limitations of retrospective recall to understand how they might affect known inferences about the childhood origins of poor adult health (Baldwin et al. 2019). However, this body of methodological literature is somewhat incomplete because studies often evaluate measures of childhood adversities obtained from different informants and instruments, and rarely do they compare predictions across subjective health and objective biomarker outcomes (e.g., Reuben et al. 2016). The current study attempts to address the limitations of prior research by examining the agreement between prospective measures of childhood adversities and overlapping measures obtained

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retrospectively from the same cohort of participants approximately two decades later when they were adults. The analyses contrast the ability of prospective and retrospective reports to independently predict self-reported illness and an index of 30-year cardiovascular disease risk.

Race, Health Disparities, and Environmental Adversity

Studying the childhood determinants of adverse health outcomes among adult African Americans is important as they have a greater prevalence and earlier onset of cardiovascular disease and disability than other racial and ethnic groups (Hozawa et al. 2007; Williams 2012). African American men also have high premature mortality rates attributable to coronary heart disease and they also experience greater functional impairment than most other groups (see, Clark et al. 2001). Studies also indicate that African Americans tend to suffer from elevated blood pressure and more acute visceral adiposity than members of other racial groups (Chyu and Upchurch 2011). Researchers have argued that racial disparities in poor physical health might be partly driven by group differences in exposure to toxic stressors, including those occurring early in childhood and adolescence (Geronimus et al. 2006; Wade et al. 2014). Within the United States race is closely correlated with socioeconomic status which also varies directly with access to healthcare, nutritional resources, and multiple dimensions of social disadvantage; all of which contribute to group disparities in cardiovascular health (Pool et al. 2017). For instance, neighborhood social and physical resources are often stratified by race and these in turn are significant predictors of cardiovascular health (Diez Roux et al. 2016). African Americans also confront stressful experiences of racism, which undermine the quality of their physical health (Havranek et al. 2015). Some evidence points to a positive linkage between racist experiences and elevated ambulatory blood pressure—a marker of cardiovascular health—and reports of racism or discrimination (Brondolo et al. 2011). Survey research on youth samples concludes that racial discrimination is another, often overlooked, domain of adverse childhood experiences that likely has profound effects on adult physical health outcomes (Wade et al. 2014).

Many African American children live in low SES circumstances, and because of this they are at elevated risk for health problems across the life-course (Brody, Yu, and Beach 2016). These disparities likely begin at the earliest stages of life. For instance, children in low-income families experience disproportionately high rates of growth restriction, preterm births, and neonatal mortality (Blumenshine et al. 2010). As children from low-SES environments mature, they continue to experience health problems at higher rates than their more advantaged peers (Chen, Matthews, and Boyce 2002). Epigenetic research suggests that social environments that pose a persistent threat of hostility, disparagement, and disrespect promote chronically high levels of inflammation underlie the onset and persistence of chronic illnesses (Cole et al. 2012). This is, of course, is a routine environment for many members of ethnic minority groups living in a racially charged society (Williams 2012). Such findings have led to arguments that in large measure the poor health of African Americans may be a consequence of stressors related to social exclusion and historical oppression spanning the life-course beginning in the perinatal stages of development.

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Retrospective Reports of Childhood Experiences

Nearly all surveys that elicit self-report information from respondents about their past and current life experiences are vulnerable to the errors of human interpretation. Prospective designs reduce these errors because they capture people's lives as they are living them. Retrospective reports are most often employed because they are efficient tools to examine multiple periods of the life-course. Retrospective tools have been met with considerable skepticism and concern (Baldwin et al., 2019). Some scholars have gone as far to argue that a reliance "on retrospective measurement alone seems ill-fated, especially when one recognizes the great risks of recall bias that exist" (Scott and Alwin 1998:26). Many of the concerns involve the potentially weak construct validity and reliability of retrospective measures. As described below, several known flaws of autobiographical memory are thought to reduce the accuracy of inferences obtained from retrospective tools.

One prominent set of concerns involves the possibility that current health status distorts how adult respondents remember their childhoods—a tendency known as retrieval bias (Schraedley, Turner, and Gotlib 2002). Recent studies find that adults who report having good health tend to recall favorable childhood experiences, but they tend to forget or choose not to report negative events (see, Susser and Widom 2012). Similarly, negative mood states (e.g., depression) can distort memories of childhood events, which is why adjusting for depressed mood is critical for predicting subjective indicators of physical health (see Colman et al. 2016). For instance, a cross-sectional study of adults found that a significant portion of the association between retrospective reports of childhood adversities and self-reported health (e.g., hypertension) was accounted for by current mental health conditions (Sheikh 2018). Similarly, longitudinal evidence suggested that prospective childhood maltreatment measures predicted psychopathology but only if the maltreatment was recalled retrospectively (Newbury et al. 2018). Further, when maltreatment was not recalled retrospectively by adult respondents, the prospective reports of maltreatment were in large measure inconsequential for the health of respondents. A longitudinal study found that retrospective self-reports of maltreatment predicted diagnosed drug abuse pathology, whereas prospective reports from official records exhibited no such association (Widom et al. 1999). A recent summary of the evidence shows that "prospective and retrospective measures of childhood adversity identify only partly overlapping groups of individuals" (Danese 2018: 348), which might result because of the way memory biases affect long term recall.

Subjective indicators of physical health outcomes, such as self-rated health and self-reported chronic illnesses, are more vulnerable to the biases of memory than indicators of objective biomarkers (e.g., inflammation, cardiovascular disease risk). Consistent with this argument, research has found that retrospective measures of "childhood traumas" (respondent reports) were more strongly associated with self-rated health than with health biomarkers (Reuben et al. 2016). Such findings suggest that it is the *belief* that one had experienced adverse childhoods that predicts subjective appraisals of one's own health state. Moreover, studies find that prospective measures of adversities (from various informant reports) predict objective biomarkers regardless of whether adult respondents recalled the events, suggesting that greater adversity in childhood is followed by poor adult objective health conditions

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(Reuben et al. 2016:1107). However, prior research on measurement concordance uses multiformat reports of child maltreatment to study retrospective and prospective adversities (see Newbury et al. 2018). Insofar as prior research comparing the two methods of measuring adversities relies (1) on different informants and (2) different data collection instruments the discrepancies between the measures are likely affected by unique sources of error. Consequently, such design differences will reduce the convergent validity between the measures. In fact, recent research suggests a fraction of the low statistical agreement between retrospective and prospective measures may result from the varying sensitivities of the two measurement strategies (see Baldwin et al. 2019: 591).

Another prominent class of methodological concerns regarding the limits of retrospective measures focuses on the fallibility of long-term memory, particularly the recollection of early life experiences. Memories are forgotten with increasing age owing to the adaptive architecture of the mind (Norby 2015). Adults may choose not to disclose traumatic experiences to avoid painful emotions and anticipated embarrassment. For instance, retrospective designs typically underestimate the incidence of confirmed childhood maltreatment due to selective under-reporting (Hardt and Rutter 2004). Because people are not disinterested observers of their own lives, their recollections might also evolve as they forge different perspectives of their pasts (Scott and Alwin 1998:25). Autobiographical memory undergoes a constant process of selection and reinterpretation as individuals develop implicit histories.

The limitations of reconstructive memory have been uncovered in prior longitudinal studies. Generally, this research has discovered low-to-moderate agreement between retrospective and prospective reports of various early life adverse events. For instance, a cohort study of young adults found that reports of childhood adversities varied substantially across measurement waves, largely due to the false negative reports of child abuse (Fergusson et al. 2000). Memory inaccuracies more commonly affect measures of psychological sentiments (e.g., parental attachment) than tangible life events (e.g., divorce), with the former being less reliably measured (see, Yancura and Aldwin 2009).

As noted, past research comparing the agreement between prospective and retrospective reports often relies on prospective reports developed from various informants and different data collection instruments (official records vs. self-reports). Multiple informants are employed to gather a comprehensive assessment of childhood experiences in different contexts than what might be provided by a sole respondent, especially in cases where respondents might not be willing to provide information (Sierau et al. 2017). Still, the various instruments—thereby different informants—adopt different definitions of childhood adversities (see Baldwin et al. 2019). Because of this mixed strategy, such work also suffers from limitations due to incident under-recording where official informants, such as social service agencies and teachers, do not record unreported incidents of maltreatment. Some informants are not able to report incidents for which they have limited information by virtue of their relationship to the respondent (e.g., quality of respondent's neighborhood). Moreover, caregivers—as informants—might be unwilling to report events that are harmful to their children or legally jeopardizing (Fisher et al. 2011). Such underreporting would reduce the corroboration between prospective and retrospective measures of childhood

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adversities. As of evidence of this possible bias, a recent study found that prospective parent reports of childhood adversities exhibited rather weak agreement with adult retrospective reports (Naicker et al. 2017: p. 12).

Prospective Reports of Childhood Adversities

Although longitudinal prospective measures are thought to be superior, they too suffer from some problems relating to validity. For instance, children might not fully understand the information requested in surveys referring to family hardships (e.g., financial strain). Furthermore, developing comparable measures of survey constructs across life-course stages is a challenge for prospective designs. To ensure the standardization of repeated measurement, a variable measured at an earlier period in the life-course must equate, conceptually, with the variable measured at later stages (Menard 2002: p. 37). The validity of prospective designs is also vulnerable to the possibility of children underreporting certain events owing to fears of repercussions. Prospective designs can also suffer from biases of panel conditioning, which can undermine the validity of statistical inferences.

The Current Study

Most of what is known about the harmful linkages between childhood adversities and adult health outcomes is derived from retrospective measures obtained from samples of adult respondents. While the evidence is fairly consistent across studies, researchers have suggested that the biases and limitations of autobiographical memory combine to reduce the accuracy of retrospective survey measures (Goltermann et al. 2019). One major implication is that measurement biases might significantly taint the association between adverse childhood experiences and poor adult health. As such, the broader purpose of this study is to answer lingering questions about the corroboration between retrospective and prospective reports of childhood adversities and to assess their capacity to predict subjective and objective health outcomes.

Building on prior research, the first hypothesis predicts weak to modest agreement between retrospective and prospective measures of childhood adversities that occurred around age 10 *(hypothesis 1).* Evidence regarding this first assumption will contribute importantly to the literature given that so few studies have examined discrepancies between different measures of childhood adversities in the context of longitudinal designs.

To further identify the possible limitations and biases of retrospective measures it is also necessary to determine if their predictions converge with health outcomes obtained from other methods including objective biomarkers (Baldwin et al. 2019: 592). As noted, prior research provides some evidence of their independent convergence with different health outcomes. This research provides the basis for the second hypothesis of the present study: Retrospective and prospective measures of childhood adversities will independently predict an objective cardiovascular disease risk score and a measure of perceived illnesses *(hypothesis 2).*

Building on the foregoing assumptions, emerging research suggests that prospective childhood reports of adverse events influence adult health outcomes insofar as individuals

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remember the events—retrospectively recall them—years later in adulthood. Moreover, this pattern appears to be more common in predictions of perceived versus objective health outcomes. According to the third hypothesis, prospective reports of childhood adversities will no longer predict a subjective health outcome once the retrospective measure of adversities is controlled in the same model (*hypothesis 3*). Note that the current study does not make a similar hypothesis about whether retrospective and prospective measures will simultaneously predict cardiovascular disease risk because the existing research does not provide a sufficient basis to offer such a prediction. However, given prior research on inflammatory and metabolic biomarkers the present study anticipates that the prospective and retrospective measures of adversities will both predict cardiovascular disease risk when simultaneously controlled in a prediction model.

Methods

Sample

The current study employs data from the Family and Community Health Study (FACHS) (see Simons et al. 2011, 2018). The sampling strategy was intentionally designed to generate families representing a range of socioeconomic status and neighborhood settings. At the first wave (1997−1998), the FACHS sample consists of 889 African American fifth-grade children who resided in Iowa and Georgia. The mean ages were 10.56 years (SD = .631; range 9–13). The sample had an average family per capita income of $6,956. Thirty six percent of the families were below the poverty line, and fifty one percent of the respondents identified as single parents. The second through sixth waves were collected between 1999 and 2012 to capture information when the target children were ages 12 to 13, 14 to 15, 17 to 18, 20 to 21, and 23 to 24, respectively.

In 2014–2015, a Wave 7 of data collection was completed that included blood draws. The mean age was 29 years. Only members of the sample residing in Georgia, Iowa, or contiguous states were identified as eligible owing to the logistical challenges of scheduling home visits by certified phlebotomists. After also excluding persons who were deceased, incarcerated, or otherwise unreachable, this left a pool of 545 individuals, 470 (86%) of whom agreed to be interviewed and to provide blood. Successful draws of HbA1c was achieved for 463 cases and BMI for 459 cases, leaving 455 valid cases. The unsuccessful biomarker data that are part of the dependent variable were not imputed following statistical convention. The outliers on the cardiovascular disease dependent variable were defined by the 1.5 × interquartile range and were removed from the final analysis. After eliminating a single outlier, complete data were available for 454 study respondents (173 men and 281 women).

Rates of missing data ranged from 0.9% for self-reported illness to 4.4% for parental education. Analyses indicated that those individuals who did not participate in Wave 7 did not differ significantly from those who participated with regard to Wave 1 scores on sociodemographic and health-related covariates. Given this result the data were assumed to be missing at random. Therefore, missing values were handled by multiple imputation using the “MI” function of the Stata 15 software.

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Procedures

To enhance rapport and cultural understanding, African American university students and community members served as field researchers to collect data from the families in their homes. Prior to data collection, the researchers received one month of training in the administration of the self-report instruments. The questions were administered in the respondent's home and took on average about 2 hours to complete. In an effort to further enhance anonymity, audio-enhanced, computer-assisted, self-administered interviews (ACASI) were used. Using this procedure, the respondent sat in front of a computer and responded to questions as they are both presented visually on the screen and auditorily via earphones. Participants were also asked to provide a blood sample at Wave 7. After blood was drawn into serum separator tubes by certified phlebotomists, it was frozen and shipped via courier to a laboratory at the University of Iowa to allow assessment of hemoglobin A1c (HbA1c), a marker of elevated blood sugar, as well as other blood-based indices.

Measures

Cardiovascular disease risk.—Studies adopting the life-course framework have identified how early life conditions affect risk for cardiovascular outcomes. Cardiovascular disease risk affects the structure and function of the heart and its vascular architecture. The clinical manifestations include stroke, peripheral artery disease, and myocardial infarction (Saydah et al. 2013). These debilitating consequences of cardiovascular disease usually occur in middle-aged or elderly people. However, atherosclerosis, the core pathological process, develops and progresses through adolescence and early adulthood. Several cardiovascular disease risk factors including hypertension and adiposity often co-occur, but any single factor is not strongly determinative of cardiovascular disease risk (D'Agostino et al. 2008). As a result, researchers have utilized prediction algorithms from the Framingham Heart Study to compute the likelihood of developing components of cardiometabolic health using seven common risk factors (Mahmood et al. 2014). Physicians worldwide employ Framingham indices to monitor patient health in clinical settings. The tool provides valid estimates of an individual's projected cardiovascular impairment during a 30-year timespan.

Cardiovascular disease risk was calculated following the gender-specific Framingham algorithm developed by Pencina and colleagues (2009). To estimate 30-year risk of cardiovascular disease (e.g., coronary death, myocardial infarction, fatal or non-fatal stroke), the algorithm uses systolic blood pressure, body mass index, hemoglobin A1c, and diabetes; plus, it adjusts for an individual's chronological age and gender, and whether they currently smoke or take antihypertensive medication (1 = yes, 8.10%). Resting systolic blood pressure was monitored with Dinamap Pro 100 (Critikon; Tampa, FL) while the participants sat quietly. Three readings were taken every 2 min, and the average of the last two readings was used as the resting index. Mean systolic blood pressure was 122.476 (SD = 16.291). An individual's body mass index score is calculated by weight in kilograms divided by the square of height in meters. Mean body mass index was 31.564 (SD = 8.522). Hemoglobin A1c (HbA1c) level is an indicator of average blood glucose concentrations over the preceding 2 to 3 months. It was determined by the University of Iowa Clinical Pathology Laboratories using a protocol previously described (Philibert et al. 2011). In the current study, about 4% had HbA1c above 6. At the time of the blood draw (age 29), the mean

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cardiovascular disease risk of participants was .062, or 6.2% (95% CI = .057 to .066) and the range was .011 to .393. Prior studies have reported mean Framingham index scores within the range of the current study (e.g., Doom et al. 2017).

Self-reported illness symptoms.—This measure was assessed at wave 7. Respondents were asked (0 = not experienced, 1=mild symptoms, 2=moderate symptoms=3 = severe symptoms), “In the past 3 months, have you experienced any of the following symptoms?”. The symptoms, or illnesses, includes: swollen glands, sore throat or fever, headache, stiff or aching muscles and/or joints, chronic fatigue, asthma and/or respiratory difficulties, pain in back, neck, or shoulders, urinary problems, constipation, heart-burn and/or indigestion, nausea, diarrhea, dizziness, breathlessness, racing heartbeat, palpitations, or chest pain, and numbness or tingling. Items were summed to form an index of self-reported illness. The Cronbach α for the scale was .901. Similar multi-item measures of self-reported illness have been used in prior research as global indicators of underlying health burden (see, Agorastos et al. 2014; Puig et al. 2013).

Retrospective childhood adversities.—A retrospective measure of childhood adversity was assessed based on the 10-item short form of the Childhood Trauma Questionnaire (CTQ; Bernstein et al. 2003) shown in in Table 2. This series of self-report measures refers to an overlapping and often co-occurring set of threatening, socially hostile, developmentally deficient, and physically harmful demoralizing conditions that for children are unwanted and difficult to avoid (Ebbert et al. 2019); the measures are also similar to those found in survey constructs used in prior research on the childhood determinants of disease and illness including cardiovascular disease risk (Lei et al. 2018). For the index respondents are asked to recall (1 = yes, 0 = no) whether they experienced each of ten items of childhood adversities before the age of 10 years (e.g., prior to age 10, would you say . . . I don’t have enough to eat at home; I had to wear old or dirty clothes that did not fit; I was punished with a belt, a board, a cord, or some other object; There was a lot of violence in my neighborhood).

Prospective childhood adversities.—To assesses childhood adversities prospectively, when respondents were adults, a set of 10 items were selected that closely resembled the information captured in the retrospective measures. At wave 1 (age 10), respondents were asked to report whether or not (1 = yes, 0 = no) they experienced a variety of negative events during the past year (ages 9 to 10) (e.g., my family did not have enough money to afford the kind of food we need; my family did not have enough money to afford the kind of clothing we need; My parents hit me with a belt, a paddle, or something else; There was a lot of murder and violence in my neighborhood). The precise wording of each question was shown in Appendix A.

Although the two sets of measures address nearly equivalent years of childhood, they do not overlap exactly in temporal terms. Specifically, the retrospective questionnaire asks respondents to recall events that occurred before 10 years of age. Whereas the prospective survey asks respondents, who were children, if they experienced a series of adverse events during the preceding year when they were approximately age 10 (the average age of the wave 1 interview). To the extent that adult respondents reported events on the retrospective

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survey that occurred prior to age 9, the prospective measure could omit events reported in the retrospective survey. As noted earlier, the discrepant recall windows would thereby reduce the statistical agreement between the two measures to some degree. Note, also, that the wording for some pairs of items is not a precise match, which comes at a cost to reliability; however, each pair of items closely reflects conceptually overlapping adversities.

Health insurance.—The analyses adjust for several measures of lifestyles and diet given their role as potential confounders. At wave 7, the survey asks respondents about their health insurance status (1 = having health insurance, 82.4%) during the previous year.

Sleep quality.—Sleep quality was measured using the subjective item (1= very bad, 4, very good): “During the past month, how would you rate your sleep quality overall?” ($\bar{x}$ = 3.074, SD = .871).

Healthy diet.—A healthy diet is a relevant determinant of good quality health and therefore an important confound in predictions about the long-term effects of childhood adversities. For this study, healthy diet was assessed using two items that asked about their frequency of fruit and vegetable consumption during the previous 7 days. Responses ranged from 1 (none) to 6 (more than once every day). Responses to these two items were correlated (r = .406, p < .001), scores were averaged to form the healthy diet variable ($\bar{x}$ = 6.64, SD = 2.43).

Exercise.—The extent to which respondents regularly exercised was measured with two items (e.g., On how many of the past 7 days did you exercise or participate in physical activity for at least 30 min that made you breathe hard such as running or riding a bicycle hard?) The response categories ranged from 1 (0 days) to 5 (all 7 days). Scores on the two items (r = .580, p < .001) were averaged to form the exercise measure ($\bar{x}$ = 5.06, SD = 2.31).

Depression.—The multivariate models also control for components of mood and emotional states because they are known to affect how respondents recall early life events. Depression was assessed with a revised version of the University of Michigan Composite International Diagnostic Interview (Kessler et al. 1998). Respondents were asked to report (1 = yes, 0 = no) whether they experienced several symptoms of depression (e.g., “felt sad, empty, or depressed most of the day” and “lost interest in things”) for at least a 2-week period in the past year. All respondents were asked all nine items, and items were summed to create a measure of depressive symptoms (Cronbach α=.857).

Anger.—Anger is another important mood state that influences subjective health and autobiographical memory and might confound the model predictions. For the current study, anger was assessed using four items from the Spielberger Trait Anger Scale (Spielberger et al. 1983) (e.g., “I have a fiery temper,” “I am a hotheaded person”). The response format ranged from 1 (almost never) to 0 (almost always) (alpha=.756).

Low self-control.—The capacity of individuals to practice self-regulation might confound some of the effects of adverse experiences on their current health. A measure of *low self-control* was measured by the 15 items from the Kendall and Williams (1982) inventory of

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self-constraint (e.g., when you ask a question, you often jump to something else before getting an answer). The response categories for all items in the measure ranged from 1 (*not at all true*) to 3 (*very true*) (alpha=.836) ($\bar{x}$ = 21.69, *SD* = 5.44).

Gender.—All models controlled for *gender* (1 = males, 38.1%) of the respondent. Research has shown that SES measures are important predictors of health outcomes and they also correlate with the childhood adversity gradient (Doom et al 2017).

Socioeconomic status.—It is important to adjust for respondents' SES and that of their caretakers in estimates of adult health outcomes which is done here with several measures. *Income* at age 29 was assessed by asking participants to report their income in the past year ($\bar{x}$ = 21, 126, *SD* = 16,529). *Family per capita income* at age 10 was calculated by dividing the total household income by the number of family members ($\bar{x}$ = 7366, *SD* = 7392). *Respondents' education* at age 29 was measured in years of education completed ($\bar{x}$ = 13.06, *SD* = 1.73). *Parental education* at age 10 reports the parent's educational level, and in cases where there are two parents or caregivers, the highest level of educational attainment between them is reported ($\bar{x}$ = 13.068, *SD* = 1.737).

Analytic Strategy

All analyses were conducted using the statistical program Stata 15 (StataCorp 2017). The intraclass correlation coefficient (ICC) was used to examine agreement between prospective and retrospective measures of childhood adversity. Unlike Pearson's correlation coefficient, the ICC reflects the degree of similarity and agreement between measurements (Fisher 1938). The values can range from 0 to 1, where 1 represents a complete agreement. A value of over .30 indicates moderate agreement (Rosenthal, 2001).

Then, a series of regression models were used to test the link between childhood adversity from both prospective and retrospective measures and cardiovascular risk and self-reported illness. Because the dependent variable, Framingham cardiovascular disease risk, is a proportion, a beta regression model was used for the analyses to account for the non-normally distributed outcome and the occurrence of boundary values 0 and 1 (Ferrari and Cribari-Neto 2004). Given that a beta regression model is a generalized linear model with a beta distribution and a logit link function, the estimated coefficients have a similar interpretation as in logistic regression (Chen et al. 2017). For the beta regression models of cardiovascular disease risk, the boundary values of 0 and 1 are excluded because every person in the sample has some risk and no one has total risk of disease. Results from the beta regression models are reported both as log odds and odds ratios. Ordinary least square (OLS) regressions are used to estimate the relationship between childhood adversity and self-reported illness.

Results

The means, standard deviations, and zero order correlations among the study variables are provided in Table 1. On average, the levels of childhood adversities were greater according to prospective ($\bar{x}$ = 2.833) than retrospective ($\bar{x}$ = 1.651) reports.

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As expected, retrospective and prospective reports of childhood adversity were significantly positively associated with each other ($r = .242$, $p < .001$), and both were significantly and associated with cardiovascular disease risk ($r = .170$, $p < .001$; and $r = .099$, $p = <.05$, respectively) and self-reported illness ($r = .357$, $p < .001$; and $r = .166$, $p < .001$, respectively). See Table 1 for the full list of correlations. It is noteworthy that the retrospective report is more strongly correlated with self-reported illness than the cardiovascular disease score given arguments that mental health biases perceptions of current health and early childhood experiences. The emotional affect and self-regulation measures were not significantly associated with the perceived health question, which suggests they might color perceptions of one's own health conditions or vice-versa. Cardiovascular disease risk was significantly associated with dietary intake and age 29 education, whereas self-report illness was related to health insurance, sleep quality, dietary intake, depression, anger, and low self-control.

Agreement Between Prospective and Retrospective Measures

To test the first hypothesis, the initial analyses examines the strength of agreement between prospective and retrospective measures of childhood adversity. The results are shown in Table 2 (note that Table 2 lists the text of the retrospective items for reference). For most items, the percentage of respondents reporting affirmatively on the prospective measures was larger than the percentage reporting affirmatively on the retrospective measures. For instance, 62% of respondents at wave 1 reported being "punished" with an object (item 4) compared to 33% at wave 7. Respondents reported fewer adversities, in general, looking back on their childhoods as young adults. Nine out of ten items were significantly correlated for both measures, ranging from .106 to .186 ($p < .05$). The exception was an item asking participants whether a family member was the victim of a crime ($r = .084$). As for the total scores of each measure ($r = .243$, $p < .001$), the retrospective was weakly correlated with the prospective measure of childhood adversity. The intraclass correlation coefficients (ICCs) are used to provide a measure of agreement for the two measures. As shown in Table 2, all ten items had ICCs which were significant at the .05 level with a range of .150 to .272. The ICC for the total score was .351 ($p < .01$), suggesting weak-to-moderate agreement between prospective and retrospective measures. Altogether the initial findings support the hypothesis that retrospective and prospective measures of childhood adversities demonstrate weak to moderate statistical agreement.

Childhood Adversity and Cardiovascular Risk

The next stage of analyses tests hypothesis 2 and hypothesis 3. Results from the regression analysis using GLM with a beta distribution and a logit link function are shown in Table 3. Multicollinearity is assessed among the study variables by assessing variance inflation factors. The VIF values ranged between 1.09 and 1.38, and all measures of tolerance were above .70 which altogether indicates no clear evidence of multicollinearity (VIF < 10 and tolerance > .20) among the study variables (see, Myers 1990).

Results from model 1 in Table 3 indicate that the prospective measure of childhood adversity was significantly associated with cardiovascular disease risk in adulthood (b = .046) after controlling for various sociodemographic and health-related covariates.

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Model 2 in Table 3 indicates that the retrospective-based measure of childhood adversity, gathered around age 29, is a significant predictor of cardiovascular disease risk (b = .068) net of the control measures. Notably, the effects withstand a concurrent measure of depression symptomology, which is an important source of confounding. This series of results suggests that retrospective reports of adversities, obtained from respondents when they were adults, increase 30-year cardiovascular disease risk in adulthood. Model 3 in Table 3 includes both measures of adversities in the regression equations to examine whether prospective and retrospective measures of childhood adversity independently affect cardiovascular disease. Both the prospective (b=.036) and retrospective measures (b=.061) are significantly associated with the objective measure of cardiovascular disease risk. Providing support for hypothesis 2, the results from Table 3 indicated that regardless of which measures are used as predictors—whether assessed around age 30 or during the first decade of life, childhood adversities have a robust association with 30-year cardiovascular disease risk. Moreover, the prospective measures remain strong predictors irrespective of whether adult respondents recall these adversities years later on the retrospective survey.

Childhood Adversity and Self-Reported Illness Symptoms

Table 4 presents the results of the OLS regression analyses where self-reported illness is predicted separately from both the prospective and retrospective measures of childhood adversity. The purpose of these models is to compare the capacity of the measures to independently and simultaneously distinguish self-reported illness burden. Model 1 shows that the prospective (age 10) measure of childhood adversity is a significant determinant of self-reported illness (β = .089) during adulthood. Notably, this effect is robust to adjustments for several key lifestyle, demographic, and dietary variables as well as depression.

The retrospective (reported in adulthood) measure of childhood adversity is the focal independent variable in Model 2 of Table 4. According to the estimates, there is a significant association between childhood adversity (β =.196) and perceived illnesses net of adjustments for several relevant control variables. Models 1 and 2 therefore also provide support for the second study hypothesis, demonstrating that each measure independently predicts perceived illnesses.

Model 3 of Table 4 tests hypothesis 3 which posits that the association between the prospective reports and self-reported illnesses symptoms will be reduced once the models adjust for retrospective measures. The prospective (age 10) and retrospective (adulthood) measures of childhood adversity were entered simultaneously into the regression model to test their associations with adult self-reported illness. The results indicate that the retrospective measure was a significant predictor of self-reported illness (β =.183) but the association between the prospective measure (at age 10) and self-reported illness burden was not significant (β =.052) in the same equation. Altogether, the results in Table 4 suggest that prospective measures of adverse childhood events predict self-reported illnesses but only insofar as they are remembered by respondents in adulthood (see also, Newbury et al 2018). Put differently, how and whether adults recall adverse events during their childhoods appears to by shaped by their concurrent perceived burden of illnesses.

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A sensitivity analysis was conducted to explore if the childhood adversities measure predicts two different albeit related dependent variables: an inflammatory biomarker and perceived chronic health conditions. Specifically, appendix B reports regression estimates that predict a measure of C-reactive protein, a commonly used indicator of vascular inflammation obtained in Wave 7 from the blood draws ($\bar{x}$ = 1.904; SD = 1.623) (Iob, Lacey, and Steptoe 2019). Because the measure of C-reactive protein displayed a skewed distribution, it was converted using a log transformation to meet the assumption of linearity for ordinary least squares. Further, the supplementary models also specify a multi-item index of self-reported chronic conditions. At age 29 respondents were asked, "Have you even been diagnosed with any of the following health illnesses?" The list of health problems consisted of seven chronic diseases and an "other" category: coronary heart disease, hypertension, diabetes, peptic ulcer, kidney disease, liver disease, thyroid disease, and other disease. For each illness, "no" was coded as 0 and "yes" was coded as 1. The list of items was summed to form an index of chronic diseases ($\bar{x}$ = .433; SD = .779). It is worth noting that there is a low prevalence of self-reported chronic diseases in the sample which is not unusual given that the respondents are around 30 years of age.

The results from the regression models reported in Appendix B suggest several important findings. First, the Wave 7 measure of C-reactive protein is not significantly predicted by either of the childhood adversities measures, whether retrospective or prospective reports, and neither measure has a significant association with the outcome when examined separately (results not shown). Second, the retrospective measure of childhood adversities significantly predicts self-reported chronic diseases whereas the prospective measure does not have a significant association with diseases. Additional analysis reveals that the estimate of the association between the prospective measures and perceived chronic diseases is stronger when the retrospective measure is examined alone—a pattern similar to the findings from the main analysis. Also, the retrospective reports exhibit the strongest association with perceived chronic diseases. The supplementary results therefore provide further evidence that the association between adverse childhood experiences and perceived health partly reflects whether respondents recall those experiences years later.

The fact that inflammation is measured with a single item indicator of C-reactive protein levels, rather than a broader measure of multi-systemic health conditions (e.g., cardiovascular disease risk, allostatic load), might explain why the supplementary estimates of C-reactive protein showed a non-significant association with childhood adversities. Estimates from single item markers of inflammation, including C-reactive protein, are often inconsistent and the findings demonstrate week effect sizes (see, Rasmussen et al. 2020). Still, the consistency of the associations with an alternative measure of perceived health conditions is noteworthy because it suggests the current findings have validity.

Discussion

Researchers have expressed skepticism about the validity of retrospective measures of childhood adversities, and retrospective designs more generally, to predict adult health conditions owing to the known biases and limitations of autobiographical memory. This study examined the corroboration between self-reported retrospective and prospective

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measures of childhood adversities and the capacity of each to predict subjective and objective biomarker health outcomes in a sample African Americans roughly 30 years of age. Providing support for hypothesis one, the results suggest there is weak-to-moderate degree of agreement between retrospective and prospective reports of childhood adversities. The ICC value of the composite scores exhibited scarcely moderate agreement, whereas most items exhibited values on the lower end of the range, towards weak or poor agreement. A clear pattern was not detected whereby salient and conceivably more severe events demonstrated stronger agreement. For instance, items that exhibited the strongest agreement included parental divorce and verbal abuse (“insults”). Not surprisingly, exposure to neighborhood disorder (“graffiti”) and family victimizations—two events particularly vulnerable to (re)interpretation—had lower levels of concordance. The physical violence items also exhibited weak levels of concordance—this finding is somewhat unexpected given the severity of the actions in question, but similar patterns have been reported previously (Schraedley et al. 2002).

Such poor-to-modest agreement between childhood adversities measured concurrently and prospectively—measured decades apart—is useful to consider in the context of prior work on the social determinants of health (e.g., Colman et al. 2016; Naicker et al. 2017). The hypothesized relations have a basis in an established body of research. For instance, studies covering much shorter periods of time than the current design have discovered “substantial unreliability” in self-reports of child abuse (Fergusson et al. 2000). Moreover, a recent study observed that a large fraction of respondents in a two wave 12-year study gave inconsistent reports of exposures to childhood stressors (Colman et al 2016). Collectively, the weak-to-modest concordance between the different measures indicates a general pattern common to these designs: adult respondents do not consistently recall or tend to forget previously reported childhood adversities (Hardt and Rutter 2004:270). As reported here, with some exceptions, adult respondents recalled fewer childhood adversities than they reported as children on the prospective survey. Perhaps such a tendency is due to ordinary or purposeful forgetting, memory reconstruction, or a cognitive bias. Regardless of the reason, the weak-to-modest agreement between the measures suggests they share some common information about childhood events, but also some unique information. Moreover, retrospective measures might not provide accurate representations of the actual occurrence of adverse childhood events but perhaps they constitute critical perceptions of past events and, as such might, be useful for assessing how perceptions of one’s childhood influence dimensions of health functioning.

It is important to note that certain design features unique to FACHS could account for the weak to modest corroboration between the measures. Specifically, the prospective measures were collected at age 10 and referred to events that occurred in the previous year, whereas the retrospective measures, collected at age 29 (wave 7), asked about experiences prior to age 10. The slightly different recall windows could produce discrepant patterns. However, if so, this design difference should have caused the retrospective measures to report a higher prevalence of adversities (due to the longer window). Because the retrospective reports captured a lower prevalence of events (see Table 2) it is unlikely that the different recall periods account for the discrepancy. The possibility remains that the prevalence rates reported in the prospective measures are higher because children might classify a broader

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array of adverse events as constituting the experience in question owing to developmental differences in perceptions of the social environment.

As alluded to earlier in the study, a fraction of the discordance between the retrospective and prospective measures might also result from the way individuals reinterpret adverse events with the passage of time (see, Hardt and Rutter 2004). As people age certain experiences especially those subject to a great deal of interpretation, such as exposure to “a lot of” violence or graffiti in the neighborhood, may not be remembered as qualitatively similar as when the person was much younger. Perhaps for some adults such experiences are considered less important in the context of their current lives than how they first understood the event as a child.

To test hypothesis two, the multivariate models assessed if the retrospective and prospective measures independently predicted subjective and biomarker health outcomes. Regarding the biomarker scores, the retrospective measure of childhood adversities significantly predicted cardiovascular risk, controlling for the prospective measures. Conversely, the prospective measures displayed independent associations with the cardiovascular outcome. Together these results support the expectations of hypothesis 2 and provide further evidence that retrospective and prospective measures predict an objective biomarker of cardiovascular disease when examined simultaneously. This is a noteworthy finding: for it suggests that childhood adversities, assessed around age 10, are robust predictors of long-term cardiovascular risk regardless of whether the FACHS sample recalled the adversities years later in adulthood. The belief that they suffered or did not suffer adversities as a child did not appear to determine how strongly prospective reports predicted cardiovascular health. Put differently, notwithstanding the weak agreement between prospective and retrospective measures, how adults perceive their childhoods does not significantly alter the imprint of early adverse environments on their cardiovascular health. Moreover, exposure to childhood adversities increases cardiovascular disease risk regardless of whether the adversities are measured prospectively or retrospectively. As a point of emphasis, the findings would also indicate that even if adult respondents reported minimal or no adversities on the retrospective instrument, had they reported greater adversities on the prospective survey—decades earlier—they would have likely registered lower quality cardiovascular health.

Because the two measures of adversities demonstrate weak statistical agreement and therefore do not considerably overlap it is possible that the retrospective measures capture some adversities not reported earlier in the prospective waves that increase poor cardiovascular health. However, it is only possible to speculate about this possibility with the current findings and therefore further research is needed to validate this assumption.

A series of models tested the imprint of childhood adversities on perceived illness to examine hypothesis two and three. First, when examined alone, the prospective and retrospective measures each predicted self-reported illnesses. These patterns provide further evidence to support hypothesis two indicating that each measure, when examined alone, predicts perceived illness burden. Noteworthy is the fact that the standardized estimates for the retrospective measure were larger than the prospective estimates, which is suggestive of a reporter bias. Second, when the measures were simultaneously modeled, the retrospective

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self-reports maintained a significant association with self-reported illnesses, whereas the strength of the prospective measure was attenuated. These results provide support for hypothesis three and suggest that regardless of the adverse events adult respondents reported years earlier when they were age 10, the belief they experienced childhood adversities significantly affected how they perceived their current illness burden.[1]

The results offer important insights about retrospective measures of childhood events and their ability to predict self-reported physical illness in adulthood: when adult respondents look back on their lives and do not recall, or choose not to report, experiencing adverse childhood events they often do not evaluate their current health profile poorly. Put differently, it appears that adult respondents who recalled suffering childhood adversities were more likely to report illnesses than those who did not disclose such adversities as adults. This interpretation pairs with some arguments suggesting that unhealthy adults are more inclined to recall problematic experiences in their childhoods than healthier adults (Susser and Widom 2012:674).[2] The reverse situation may also be true: perhaps adult respondents who do not recall childhood adversities are unlikely to perceive themselves as suffering from poor health. Either way, there is reason to believe that current health conditions color how respondents view their childhoods. As such, perhaps the way people define their early childhoods is meaningful and important for understanding their current subjective health profile. Still, since the measures of perceived illnesses and retrospective adversities were gathered in the same wave (7), the current study cannot establish a causal link between impaired perceived health and retrospective reports of childhood adversities. Regardless of the interpretation, this evidence favors the possibility that self-reported retrospective measures of adversities overstate the association between childhood adversities and perceived (self-reports) illnesses.

There are several limitations to the current study that temper the findings but also provide opportunities for future research. First, data collection began when the respondents were approximately age 10. With regard to the collection of prospective reports, this period of time might not capture children who experience adversity only at earlier ages. Past studies have shown strong effects of early socioenvironmental adversity from infancy to around age 5 on adult physical health (Chen, Martin, and Matthews 2007). However, it is difficult to collect reliable data from younger children because they may misunderstand and misinterpret the questions being asked. Second, although the retrospective and prospective items refer to conceptually equivalent adverse events, the content differences between certain pairs of items—regarding the nature of the events in question—could account for their weak to moderate statistical corroboration. Third, although the study focused on two separate outcomes that have been extensively studied, readers are urged to use caution when generalizing the findings to outcomes not examined here, including other biomarkers (e.g., cognitive aging) and other subjective indices (e.g., self-rated global health). Fourth, researchers should consider the possibility that the findings could vary in a different sample comprised of older adults with a more substantial health burden. Fifth, the personal income

[1]Also, it may be that concurrent (adult) reports of limited childhood adversities predict self-reported illnesses even among adults who reported adverse events as children.

[2]Adults with optimal health might also tend to forget or not report adverse childhood experiences (Hardt and Rutter 2004:267).

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of the target sample is slightly lower than the personal income of African Americans in the population. Given that adversities and toxic stressors are so often class stratified, this means the current sample might suffer a higher burden of childhood adversities and poorer health relative to African American adults of a similar age in the population. Nonetheless, the findings are consistent with past research based on different samples (e.g., Baldwin et al. 2019). Sixth, future research might consider including a prospective measure of socioenvironmental adversities obtained in adolescence in addition to the measure from childhood. Assessing adversities in adolescence would allow for researchers to determine to what degree the strength of corroboration between the adult retrospective measure and the childhood prospective measure varies across developmental periods. Finally, the sample is comprised only of African Americans, which limits the generalizability of the findings to other population sub-groups. However, it is important to stress that African Americans suffer a more acute physical health burden across multiple domains, including an earlier onset of disease, relative to other racial and ethnic groups even after adjusting for social class (see, Turner et al. 2017). As noted, such findings have led to claims that in large measure the poor health of African Americans may be a consequence of stressors related to social exclusion and oppression (Williams 2012). Future research should also conduct comparative examinations with samples of older adults, including African Americans, in which the burden of illnesses and cardiovascular disease is more prevalent.

Conclusion

Public health advocates and social scientists alike have increasingly argued that adverse childhood experiences are critically important modifiable risk factors in the treatment and prevention of adult illness and disease (see, Suglia et al. 2018). As such, they should be the target of policy programming including clinical intervention at the federal and local levels. Still, empirical knowledge about the long-term imprint of adverse childhood experiences should shape programming only insofar as the tools from which it is derived are scientifically valid. Longstanding scientific concerns about the various flaws of retrospective self-report measures raise critical doubts in this regard. The findings reported in this study are further evidence in favor of the argument that the prediction of perceived illnesses based on retrospective reports of childhood adverse experiences may not be a valid method to determine the early life socioenvironmental origins of subjective health in adulthood. As such, reliance on retrospective measurement alone to predict perceived health conditions should be done so with caution. Prevention programs that target the health burden of adverse childhood events should therefore weight the potential weakness of evidence derived from retrospective assessments—a common design tool in this area of work. Although the current study contributed new findings to this discussion, more research is needed to gain a deeper understanding of the limits and strengths of retrospective tools to understand how adult physical health conditions are determined by adverse childhood events. Given that retrospective tools are efficient and cost-effective methods of assessing the implications of early life experiences for a variety of developmental outcomes, researchers should consider alternative techniques to employ them in tandem with prospective measures or in ways that adjust for their known biases to closely approximate valid inferences.

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Appendix A:

Measures of Childhood Adversity Across Retrospective and Prospective Reports.

Items	*Retrospective measures*	*Prospective measures*
1	I didn't have enough to eat at home	My family did not have enough money to afford the kind of food we need
2	I had to wear old or dirty clothes or clothes that did not fit	My family did not have enough money to afford the kind of clothing we need
3	People in my family hit me so hard that it left me with bruises or marks	My parents slap or hit me hard
4	I was punished with a belt, a board, a cord, or some other object	My parents hit me with a belt, a paddle, or something else
5	There was a lot of violence in my neighborhood	There was a lot of violent crime in my neighborhood
6	There was a lot of graffiti and run-down buildings in my neighborhood	In my neighborhood, there was graffiti on buildings and walls.
7	A family member was the victim of a crime	A family member was a victim of a violent crime
8	Did your parents separate or divorce?	Did your parents separate or divorce?
9	Someone said something insulting to you because of your race or ethnic background	Someone said something insulting to you because of your race or ethnic background
10	Members of your family or close friends were treated unfairly	Members of your family or close friends were treated unfairly

Appendix B:

Regression Models Predicting Logged C-reactive Protein and Self-Reported Chronic Diseases in Adulthood

	C-reactive protein		Chronic diseases	
	b	β	b	β
Childhood adversity (prospective)	.007 (.017)	.020	.044 (.022)	.092
Childhood adversity (retrospective)	−.004 (.016)	−.012	.085** (.020)	.199

Note: Unstandardized coefficients (b) and standardized coefficients (β) shown with standard errors in parentheses; $N = 454$.

* $p \leq .05$;

** $p \leq .01$ (two-tailed tests).

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Table 1:

Correlations, Means, and Standard Deviations among the Study Variables ($N = 454$)

Variable	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1. Cardiovascular disease risk	—															
2. Self-reported illness symptoms	.013	—														
3. Childhood adversity (prospective)	.099*	.166**	—													
4. Childhood adversity (retrospective)	.170**	.357**	.242**	—												
5. Males	.403**	−.260**	−.008	−.016	—											
6. Income (Age 29)	−.030	.028	−.070	.152**	.095*	—										
7. Education (Age 29)	−.150**	.081	−.075	.126**	−.070	.314**	—									
8. Family per capita Income (Age 10)	.032	−.040	−.208**	−.025	−.011	.136**	.180**	—								
9. Parental education (Age 10)	−.054	−.001	−.015	.025	.046	.096*	.252**	.293**	—							
10. Health insurance (Age 29)	−.046	144**	−.104*	.090	−.120*	.168**	.241**	.032	.066	—						
11. Sleep quality (Age 29)	.067	−.395**	−.185**	−.271**	.158**	−.095*	−.101*	−.011	.053	.020	—					
12. Healthy diet (Age 29)	−.122**	.119*	.031	.060	−.150**	.013	.160**	.020	.031	.132**	−.074	—				
13. Exercise (Age 29)	−.019	.037	.071	.126**	.181**	.161**	.126**	.021	.075	.064	−.092	.146**	—			
14. Depression (Age 29)	.070	.355**	.079	.395**	−.151**	−.039	−.005	−.027	−.007	.099*	−315**	.028	−.016	—		
15. Anger (Age 29)	.018	.237**	.114*	.260**	−.045	−.097*	−.094*	−.022	.027	.020	−174**	.020	−.047	.296**	—	
16. Low self-control (Age 29)	.076	.189**	.051	.176**	.036	.038	−.008	.028	.034	.083†	−.131**	.017	.019	.230**	419**	—
Mean	.062	5.463	2.833	1.916	.381	21126	13.068	7366	12.687	.824	3.074	6.641	5.055	1.773	6.351	21.699
SD	.048	6.433	1.651	1.836	.486	16529	1.737	7392	2.106	.381	.871	2.429	2.310	2.251	2.390	5.444

*$p \leq .05$,

**$p \leq .01$ (two-tailed tests).

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Table 2:

Response Means, Pearson's Correlations, and Intra-class Correlation Coefficients Comparing Prospective and Retrospective Measures of Childhood Adversity

	Prospective	Retrospective		
	%	*%*	*r*	ICC
1. I didn't have enough to eat at home	4.0	9.9	.159**	.249**
2. I had to wear old or dirty clothes or clothes that did not fit.	8.1	2.0	.131**	.185*
3. People in my family hit me so hard that it left me with bruises or marks.	20.8	4.0	.118*	.151*
4. I was punished with a belt, a board, a cord, or some other object.	62.4	33.4	.108*	.166**
5. There was a lot of violence in my neighborhood.	19.8	11.9	.143**	.241**
6. There was a lot of graffiti and run-down buildings in my neighborhood.	27.4	17.4	.109*	.176*
7. A family member or friend was the victim of a crime.	20.4	8.1	.084	.150*
8. Did your parents separate or divorce?	22.0	38.2	.138**	.227**
9. Someone said something insulting to you because of your race or ethnic background	62.2	34.5	.186**	.272**
10. Members of your family or close friends were treated unfairly	44.3	31.9	.106*	.198*
Total score			.243**	.351**

Note: each item in the table displays the wording content of the retrospective assessments. *N*=454

* $p \leq .05$;

** $p \leq .01$ (two-tailed tests)

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Table 3:

Beta Regression Models Predicting Estimated 30-year Cardiovascular Disease Risk in Adulthood

	Model 1		Model 2		Model 3	
	b	e^b	**b**	e^b	**b**	e^b
Childhood adversity (prospective)	.046**(.016)	1.048			.036*(.017)	1.037
Childhood adversity (retrospective)			.068**(.016)	1.070	.061**(.016)	1.063
Control variables						
Males	.645**(.056)	1.906	.643**(.055)	1.902	.646**(.055)	1.909
Income (Age 29)	−.023(.029)	.977	−.035(.028)	.965	−.030(.028)	.970
Education (Age 29)	−.068*(.028)	.934	−.084**(.028)	.920	−.082**(.028)	.921
Per capita Income (Age 10)	.069*(.029)	1.072	.057*(.028)	1.058	.071*(.029)	1.074
Parental education (Age 10)	−.035(.028)	.966	−.028(.028)	.973	−.033(.028)	.968
Health insurance (Age 29)	.099(.068)	1.104	.072(.068)	1.075	.088(.067)	1.092
Sleep quality (Age 29)	.028(.032)	1.029	.030(.032)	1.030	.040(.032)	1.041
Healthy diet (Age 29)	−.005(.011)	.995	−.006(.011)	.994	−.006(.011)	.994
Exercise (Age 29)	−.016(.012)	.984	−.019(.012)	.981	−.021(.012)	.980
Depression (Age 29)	.040**(.012)	1.040	.023†(.013)	1.023	.024(.013)	1.024
Anger (Age 29)	−.009(.013)	.991	−.014(.013)	.986	−.015(.013)	.985
Low of self-control (Age 29)	.006(.005)	1.006	.007(.005)	1.007	.007(.005)	1.007
Constant	−3.331**(.194)		−3.243**(.185)		3.367**(.193)	

Note: Log odds (b) and odds ratio (e^b) shown with standard errors in parentheses; income and education are standardized by z-transformation; $N = 454$.

*$p \leq .05$;

**$p \leq .01$ (two-tailed tests).

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Table 4:

OLS Regression Models Predicting Self-Reported Illness Symptoms in Adulthood

	Model 1		Model 2		Model 3	
	b	β	b	β	b	β
Childhood adversity (prospective)	.347 *(.166)	.089			.206(.167)	.052
Childhood adversity (retrospective)			.685 **(.161)	.196	.642 **(.164)	.183
Control variables						
Males	−2.358 **(.579)	−.178	−2.460 **(.571)	−.186	−2.452 **(.571)	−.185
Income (Age 29)	.108(.284)	.017	−.076(.281)	−.012	−.053(.282)	−.008
Education (Age 29)	.176(.295)	.027	.040(.291)	.006	.062(.292)	.010
Per capita Income (Age 10)	−.236(.281)	−.037	−.297(.271)	−.046	−.228(.277)	−.035
Parental education (Age 10)	.082(.283)	.013	.125(.278)	.019	.101(.278)	.016
Health insurance (Age 29)	1.367(.701)	.084	1.198(.687)	.074	1.285(.690)	.079
Sleep quality (Age 29)	−1.885 **(.335)	−.256	−1.833 **(.328)	−.249	−1.774 **(.332)	−.241
Healthy diet (Age 29)	.112(.112)	.043	.110(.110)	.042	.107(.110)	.041
Exercise (Age 29)	.075(.120)	.027	.045(.118)	.016	.038(.118)	.013
Depression (Age 29)	.538 **(.129)	.188	.362 **(.133)	.127	.373 **(.133)	.131
Anger (Age 29)	.247(.130)	.092	.175(.130)	.065	.171(.130)	.064
Low self-control (Age 29)	.080(.055)	.068	.078(.054)	.066	.078(.054)	.066
Constant	4.700 *(2.010)		5.346 **(1.900)		4.669 *(1.977)	
R-squared	.286			.308		.310

Note: Unstandardized coefficients (b) and standardized coefficients (β) shown with standard errors in parentheses; income and education are standardized by z-transformation; $N = 454$.

* $p \leq .05$;

** $p \leq .01$ (two-tailed tests).