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New Cohorts, Not Just New Times: The Declining Happiness of Young Americans in the General Social Survey, 1972-2024

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Research Article

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Abstract

Research on the age profile of well-being long placed its low point in midlife. In the United States, young adults' mean happiness was slightly below that of the middle-aged, but in several periods fewer of them described themselves as not too happy, and fewer reported serious psychological distress. Recent evidence suggests the young have become the least happy age group, and two explanations compete. On one reading the decline belongs to the times, pressing on whoever is currently young; on the other it belongs to the people, traveling with recent birth cohorts as they age. Little United States evidence distinguishes the two. This study tests them against each other in the General Social Survey, using 69,632 respondents across thirty-five waves from 1972 to 2024, with a replication on psychological distress in the National Health Interview Survey from 1997 to 2024. Same-age comparisons across cohorts, adjusted for period conditions, show that every cohort born between 1945 and 1984 reported nearly identical happiness at the same young-adult ages. The deficit begins abruptly with the cohorts born after 1995, who are less happy than any predecessor cohort at every age observed. The distress data show the same step, part of it measured before either survey changed its interview mode. Because both surveys are repeated cross-sections, the findings describe cohort averages, and they may partly reflect how recent cohorts report their well-being. Services and monitoring will need to follow the affected cohorts rather than youth as an age bracket.

Background

Youth Well-Being and the Age Profile of Happiness

Trends in national happiness series have anchored subjective well-being research since its beginning (Easterlin, 1974, 1995), and their interpretation remains contested (Clark, 2018; Stevenson & Wolfers, 2008). For decades the relation between age and subjective well-being was summarized by a U-shape, with well-being high in youth, lowest at midlife, and recovering in later life (Blanchflower, 2021; Blanchflower & Oswald, 2008). The pattern was defended against methodological critique (Blanchflower & Graham, 2022), but it was never uniform. Age profiles varied across places (Deaton, 2008). In Gallup data from 2014 and 2015, life satisfaction fell faster between ages 15 and 24 than at any later stage in most world regions (Handa et al., 2023). The young were thus never simply the happiest group; their advantage, where it existed, depended on the measure, the country, and the comparison group.

Recent studies report that the youth end of the profile has deteriorated. Ill-being in the United States and the United Kingdom now declines with age rather than peaking at midlife, because the young deteriorated (Blanchflower et al., 2025). Life satisfaction among respondents under twenty-five fell after roughly 2015 in six English-speaking countries (Twenge & Blanchflower, 2025). The World Happiness Report documents the loss of the youth advantage across North America (Helliwell et al., 2024). British evidence shows deteriorating youth mental well-being on a separate instrument (Blanchflower et al., 2026). This evidence is strongest on the change in the age profile and weakest on its source. It rests mostly on comparisons of age groups across successive cross-sections, in which a period shock aimed

at the young and a cohort deficit look alike. It is also sensitive to measurement. Across 23 countries, higher ill-being among the young is clear in web surveys and far less apparent in telephone surveys (Blanchflower & Bryson, 2026b). Swiss panel data from 2013 to 2018 still show lower well-being at ages 40 to 55 than at 25 to 39 (Gondek et al., 2024). The inversion must therefore be established country by country, with the survey mode in view.

The Youth Mental Health Evidence

The happiness reversal did not arrive alone. American adolescent depressive symptoms rose sharply after about 2012 across several surveillance systems (Keyes et al., 2019; Mojtabai et al., 2016; Twenge, Joiner, et al., 2018). Their prevalence roughly doubled between 2009 and 2019 (Daly, 2022), and elevated levels persist in recent school-based surveillance (Verlenden et al., 2024). Measures of psychological well-being among adolescents declined over the same years (Twenge, Martin, et al., 2018), and mood-disorder indicators and suicide-related outcomes rose fastest in the cohorts born after the mid-1990s (Twenge et al., 2019). Gender also structures the deterioration: the adolescent life-satisfaction gap widened (Rohrer et al., 2026), and female ill-being exceeds male ill-being across ages and countries (Blanchflower & Bryson, 2024; Kaiser et al., 2025).

Two readings of this evidence remain open. The first concerns cause. Timing has made digital and social media the leading candidate explanation (Haidt, 2024; Haidt & Allen, 2020; Twenge, 2023). The correlational evidence for screen-based accounts is contested, however, and the associations found at the individual level are small (Odgers & Jensen, 2020; Orben & Przybylski, 2019; Valkenburg et al., 2022). The second concerns measurement. Trend claims from repeated cross-sections can shift with instrumentation and sampling frame (Bor et al., 2014; Collishaw, 2015). Awareness campaigns may also lead some young people to describe milder distress as a mental health problem, which would raise reported prevalence without an equal change in underlying symptoms (Foulkes & Andrews, 2023). Much of the youth literature therefore documents rising reports, and fewer studies can say how much of the rise reflects changed experience.

Adult well-being research supplies an adjacent precedent. Mortality and morbidity among middle-aged white Americans deteriorated on a birth-cohort pattern, with each successive cohort entering worse than the last (Case & Deaton, 2015). Well-being data mapped the despair that preceded it (Graham & Pinto, 2019). Graham et al. (2026) have argued that well-being indicators should serve as warning indicators for such deteriorations, and recent evidence finds despair rising among young American workers specifically (Blanchflower & Bryson, 2026a). Whether young Americans' happiness is deteriorating on a cohort or a period pattern therefore bears on which surveillance and which response the deterioration calls for.

What Each Account Predicts

Age, period, and cohort are linearly dependent: birth year equals survey year minus age. No statistical procedure can separate their effects without an external restriction, a point established long ago (Glenn,

1976) and reconfirmed for the modern estimators that promised otherwise (Bell & Jones, 2013, 2014; O'Brien, 2011). Estimates from constrained models reflect the chosen constraint (O'Brien, 2011; Yang & Land, 2006). In response, the literature replaced decomposition with falsifiable prediction: state what each account implies for observable cells of the data, and check which implication fails. Cohort-tracking designs of this kind identified the birth-cohort structure of the obesity epidemic (Reither et al., 2009), of British mental health (Bell, 2014), and of the recent deterioration in Australian mental health, which is concentrated in the cohorts born in the 1990s and, to a lesser extent, the 1980s (Botha et al., 2023).

Happiness research has used both approaches on American data, with conflicting results. Yang (2008) applied hierarchical age-period-cohort models to the 1972 to 2004 General Social Survey and found strong age effects, period swings, and lower happiness among the baby-boom cohorts. Fukuda (2013) applied four identification strategies to the 1972 to 2008 data. One of them produced results different from the other three, a reminder that such estimates inherit their identifying assumptions. The sharpest disagreement concerns the years just before the recent decline. Twenge et al. (2016) examined the same happiness item through 2014 and attributed the movement among adults to period rather than cohort. Bardo et al. (2017) reanalyzed those data and traced the result to the low happiness of the baby-boom cohort, the short-lived Great Recession, and the way the data had been aggregated. Hierarchical models of this kind separate the three dimensions only through implicit constraints (Bell & Jones, 2014), so the dispute could not be settled within the framework both sides used. Neither side could observe the cohorts born after 1995 as adults, because those cohorts had not yet reached adulthood when the data ended.

Panel evidence from outside the United States shows both the promise of cohort designs and their exposure to measurement. Botha et al. (2023) followed Australians for twenty years and found the deterioration in mental health concentrated in the cohorts born in the 1990s and, to a lesser extent, the 1980s. Lenzen et al. (2026) compared self-reported mental health in Australian longitudinal data from 2002 to 2023 with the mental health predicted from respondents' socioeconomic circumstances, childhood conditions, physical health, and life events. They concluded that younger cohorts apply lower thresholds when reporting distress, that a genuine deterioration nonetheless remains, and that the adjusted age profile of poor mental health peaks in midlife. Generational comparisons of Australian subjective well-being likewise find cohort differences that are significant but, for many life domains, smaller than the effects of family and labor-market status (Botha & Vera-Toscano, 2022). Early-life conditions also leave durable cohort marks on life satisfaction (Kijewski, 2020). The same national record can thus support a cohort reading and a partly reporting-based reading. A new analysis has to show more than a deficit that follows recent cohorts; it must also show how much of that deficit survives changes in how, and by whom, well-being is reported.

What separates the two accounts is a specific set of cells of the data. If the youth deficit is a period phenomenon, it attaches to whoever is currently young. Recent cohorts observed at older ages should look like their predecessors did at those ages, and the deficit should not follow them. If it is a cohort phenomenon, the people born in the affected years should be less happy than earlier cohorts were at the

same ages, at every age at which they are observed. A boundary between affected and unaffected birth years should also be visible. Those predictions can be tested with means and differences, without any decomposition machinery.

The Present Study

This study tests the two accounts on the longest American happiness record available. The series has supported recent trend analyses (Berry et al., 2024), and its stability through 2014 was documented by the survey organization itself (Smith et al., 2015). It differs from the studies above in four ways. First, it extends the cohort-versus-period analysis of American happiness from 2014 through 2024, adding the decade in which the decline occurred and the first decade in which post-1995 cohorts appear as adults. Second, it replaces decomposition with a discriminating statistic that compares cohorts at the same ages and adjusts for period conditions, using the contemporaneous middle-aged as a baseline. Placebo versions show that the design does not manufacture deficits. It replaces the constraints that divided earlier analyses with one visible assumption, that period conditions affect young and middle-aged adults alike, which the placebo and mode-constant tests probe. Third, it replicates the result on psychological distress in an independent federal survey whose design changes do not coincide with the General Social Survey's 2021 change of mode. Fourth, it states which parts of the result are exposed to differences in survey mode and in reporting, and which are not.

Data and Methods

Data

The primary data source is the cumulative file of the General Social Survey (GSS), covering thirty-five survey waves between 1972 and 2024 (Davern et al., 2026). The GSS is a nationally representative survey of adults in the United States (Marsden et al., 2020) and has carried the same global happiness question in every round. Every round asks: "Taken all together, how would you say things are these days, would you say that you are very happy, pretty happy, or not too happy?" Global happiness reports of this kind are established measures of evaluative subjective well-being (Diener et al., 1999, 2018) and meet the comparability standards recommended for official measurement (OECD, 2013). Single-item happiness measures of this kind have documented test-retest reliability in the moderate range (Krueger & Schkade, 2008; Lucas & Donnellan, 2012), and its aggregates track objective indicators of well-being across American states (Oswald & Wu, 2010). After exclusions, the analysis file contains 69,632 respondents aged 18 to 88 with a valid happiness response and a valid survey weight. I use the 2024 cumulative release, published in July 2026, which closes a fifty-two-year observation window.

The replication data source is the National Health Interview Survey (NHIS), which has administered the six-item Kessler scale of psychological distress to sample adults since 1997 (Kessler et al., 2002). Its six items ask how often in the past thirty days the respondent felt sad, nervous, restless, hopeless, that

everything was an effort, and worthless, each scored from 0 to 4. Scores of 13 or higher on the 24-point total mark serious psychological distress (Kessler et al., 2003), and the 5 to 12 band marks moderate distress (Prochaska et al., 2012). My extract covers every year from 1997 through 2018 and the years 2021 and 2024. After its 2019 redesign (Zablotsky et al., 2023) the NHIS fields the scale as rotating rather than annual content, so no public data for the scale exist for 2019, 2020, 2022, or 2023.

This series serves two purposes. It measures distress rather than happiness, so it tests whether the conclusions depend on one item. It also brackets the 2021 redesign of the GSS with an independent instrument, so it separates the substantive trend from the survey-methods break discussed below. Estimates use the sample-adult final weights and condition on complete responses to all six items, following federal reporting practice.

Measures

The happiness item is scored from 1 for not too happy to 3 for very happy, so higher values indicate greater happiness. I also report the share of respondents answering not too happy. That share is the reading of the item least sensitive to movement between the two upper categories and to assumptions about the spacing of the scale (Bond & Lang, 2019). Birth year is measured by the GSS cohort variable, which equals the survey year minus age at interview for 98 percent of records. Respondents aged 89 and above are excluded because their age, and therefore their birth year, is top-coded.

Age is grouped into seven bands: 18 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 49, 50 to 59, and 60 to 88. Birth cohorts are grouped into ten-year bands from 1945–1954 through 1985–1994, with a final band for those born between 1995 and 2006. Survey periods are grouped into eleven multi-year bins from 1972–1977 through 2022–2024. All estimates use the GSS post-stratification weights, wtssall through 2018 and wtssnrps from 2021 onward.

The Identification Problem and the Analytical Strategy

Because age, period, and cohort are linearly dependent, this study does not estimate an age-period-cohort decomposition (Bell & Jones, 2013; Glenn, 1976). It instead tests the two falsifiable predictions stated above, following the cohort-profile logic of recent generational analyses of mental health (Botha et al., 2023).

The discriminating statistic is a same-age cross-cohort comparison adjusted for period conditions. For a given age band, I compare the mean happiness of a recent cohort with the mean happiness of an earlier cohort observed at the same ages. From each I subtract the contemporaneous mean of adults aged 40 to 59. That baseline absorbs whatever was affecting all adults in each observation window, including national conditions and survey-design effects that operate on everyone. The baseline group itself turns over across cohorts, and in the recent windows it contains cohorts whose own levels are slightly depressed; a depressed baseline shrinks the measured deficit, so this drift makes the estimates conservative. What results is a difference in differences: the excess deficit of the later cohort at a fixed

age, relative to the middle-aged of its own day. Placebo versions of the same statistic compare adjacent earlier cohort pairs, for which the historical record gives no reason to expect a difference. Standard errors combine the four weighted cell variances.

Two regression specifications complement the cell comparisons. One regresses the happiness score on age-band and period indicators with weighted least squares and heteroskedasticity-robust standard errors. A second adds indicators for birth after 1985 and birth after 1995. Because the age bands and period bins are coarse, the three dimensions are not collinear in this specification, and the cohort indicators measure whatever age-and-period-adjusted signal is attributable to birth year. A third specification adds controls for sex, race, degree, marital status, and work status. Throughout, the analysis is deliberately confined to weighted means, differences, and least squares; the argument runs on which cells of the data move, not on model structure.

Repeated Cross-Sections and Synthetic Cohorts

Both surveys are repeated cross-sections. Each wave draws a new sample, and no respondent is observed twice in the data used here. A birth cohort is therefore followed as a synthetic cohort, whose mean at a later age is estimated from different members of the same birth years (Deaton, 1985). This has three implications for the present question. First, the design can show that a cohort's average remains low as the cohort ages, but not that the same individuals remain low. Persistence in this study is a property of the population born in given years. Whether an early deficit predicts later ones within persons, or whether some individuals recover while others decline, requires panel data.

Second, a synthetic cohort's membership can change between waves through immigration, mortality, and differential nonresponse. Mortality is low at the ages compared here, but immigration and shifts in who responds could move a cohort's mean without any change in the well-being of its members. The period adjustment removes changes common to all ages, not composition changes specific to one cohort, and the demographic controls in the regressions address only observed characteristics.

Third, the design avoids two known problems of panels. Each wave is a fresh probability sample weighted to the adult population, so the estimates are not exposed to cumulative attrition. Respondents are also not re-interviewed, and repeated interviewing has been shown to shift reported mental health and to narrow the dispersion of well-being reports over successive waves (Wooden & Li, 2014). For a question about whether recent cohorts are less happy than earlier cohorts at the same ages, the repeated cross-section is well suited, provided its results are read as statements about cohort averages.

The 2021 Survey Redesign

The GSS moved from in-person interviewing to web self-administration in 2021. Respondents report lower happiness online than in person under otherwise similar conditions (NORC at the University of Chicago, 2021), and age profiles of well-being differ by survey mode across countries (Blanchflower &

Bryson, 2026b). Four features of the design address this. First, within the 2021 to 2024 waves, mode is observed, and in-person interviewing resumed for part of the sample in 2022 and 2024, so mode-constant comparisons are possible. Second, the discriminating statistic differences out anything that affects all ages in a period. This removes the common part of the mode effect, though not any part that differs by age, which is why the mode-constant re-estimates below carry the most weight. Third, the cohort test is re-estimated using only waves through 2018 and, separately, using only in-person interviews, so the reader can see how much of the result survives without any post-redesign observation. Fourth, the NHIS replication brackets the break with an independent instrument.

Results

The Decline Is Recent, Large, and Steepest Among the Young

Table 1 reports weighted mean happiness and the share not too happy for adults aged 18 to 29 and 40 to 59 in each survey period. For four decades the two series moved together. Young adults averaged between 2.14 and 2.25 on the three-point scale in every period from 1972 through 2017. They were modestly less happy than the middle-aged in the 1970s, and statistically indistinguishable from them by 2013–2017, when the gap was 0.004 points. In every wave from 1972 through 2018, the share of young adults describing themselves as not too happy stayed between 6 and 18 percent. On that share the young fared better than the middle-aged in five of the nine periods before 2018.

Table 1

Happiness of young and middle-aged adults by survey period, GSS 1972–2024 (weighted)

Period	Ages 18–29, mean	Ages 18–29, not too happy (%)	Ages 40–59, mean	Ages 40–59, not too happy (%)	Gap in means (18–29 minus 40–59)	n, 18–29
1972-77	2.139	14.5	2.243	12.6	-0.104	2,320
1978-82	2.161	12.9	2.234	11.2	-0.073	1,254
1983-87	2.168	11.7	2.203	12.2	-0.034	1,860
1988-92	2.245	7.3	2.247	10.0	-0.002	1,269
1993-97	2.168	10.0	2.215	11.2	-0.047	1,408
1998-02	2.155	12.4	2.226	10.7	-0.071	1,317
2003-07	2.176	11.7	2.196	11.7	-0.020	789
2008-12	2.155	12.9	2.165	14.0	-0.010	1,063
2013-17	2.153	12.7	2.149	15.6	+ 0.004	867
2018-21	1.909	26.2	2.067	18.5	-0.158	803
2022-24	1.949	25.7	2.037	19.4	-0.088	1,006

Note. Weighted estimates using GSS post-stratification weights. The happiness item is scored 1 (not too happy) to 3 (very happy). The 2018–2021 period includes the 2021 wave, the first conducted by web self-administration

The 2018 to 2024 waves break the series. Mean happiness among 18-to-29-year-olds fell to 1.91 in 2018–2021 and 1.95 in 2022–2024. At the same time, the share not too happy roughly doubled, from 12.7 percent in 2013–2017 to about 26 percent in both recent periods. Middle-aged happiness also fell in the same years, from 2.15 in 2013–2017 to 2.04 in 2022–2024, so part of the recent deterioration is shared across ages. What is youth-specific is the reopening of the age gap. Young adults went from parity in 2013–2017 to 0.088 points below the middle-aged in 2022–2024, twice the average gap of the preceding four and a half decades. Figure 1 shows the annual series. Decline among the young is visible from 2018, when mean happiness at ages 18 to 29 reached 2.07, its lowest value recorded to that point, in a wave interviewed entirely in person.

Age Profiles Have Inverted

Figure 2 plots mean happiness by age band in four eras. From 1972 through 2018 the age profile was shallow: the youngest band was always slightly the least happy, the bands from the late twenties to the fifties sat close together, and adults over 60 were the happiest. In 2021–2024 the youngest band trails by a wide margin, and mean happiness rises steeply from the youngest band into the late thirties. In raw reports, the young rather than the middle-aged now define the low point of the profile. This inverted profile is, however, consistent with both rival accounts, because a period shock aimed at the young and a deficit carried by recently born cohorts produce the same cross-sectional picture. Distinguishing them requires following cohorts across ages.

The Deficit Travels With the Post-1995 Cohorts

Table 2 arranges the same data by birth cohort and age at interview, and Fig. 3 plots the trajectories. Every cohort born between 1945 and 1984 falls between 2.09 and 2.26 at every age from 18 to 39, and the sole departures anywhere in their records are mild late-life drifts after age 50. Stability at fixed ages is striking: at ages 18 to 24, the four cohorts born from 1945 through 1984 span 2.09 to 2.18, and at ages 25 to 29 they span 2.197 to 2.206. Against that stability, the two youngest cohorts separate. The 1985–1994 cohort shows a dip at ages 30 to 34 in the raw cells. Most of that dip reflects the depressed periods in which those ages were observed rather than the cohort itself, as the adjusted estimates below show. The 1995–2006 cohort is far below every predecessor at every age at which it has been observed. It stands at 1.90 at ages 18 to 24, where no earlier cohort was below 2.09, and at 2.00 at ages 25 to 29, where no cohort born before 1985 was below 2.19.

Table 2

Mean happiness by birth cohort and age band at interview (weighted; unweighted n in parentheses)

Birth cohort	18–24	25–29	30–34	35–39	40–49	50–59	60–88
1945-54	2.093 (917)	2.197 (1,476)	2.192 (1,455)	2.238 (1,586)	2.197 (2,812)	2.207 (1,793)	2.151 (3,244)
1955-64	2.143 (1,546)	2.206 (1,740)	2.221 (1,765)	2.210 (1,639)	2.200 (2,253)	2.130 (2,366)	2.033 (1,782)
1965-74	2.176 (1,386)	2.204 (1,347)	2.260 (1,171)	2.221 (996)	2.167 (2,006)	2.005 (1,503)	
1975-84	2.124 (1,041)	2.197 (936)	2.197 (1,083)	2.144 (1,202)	2.067 (1,632)		
1985-94	2.125 (947)	2.106 (1,103)	2.036 (1,222)	2.144 (581)			
1995–2006	1.900 (963)	2.001 (445)					

Note. Weighted means; unweighted cell sizes in parentheses. Cells with fewer than 60 respondents are omitted. Respondents aged 89 and above are excluded because age is top-coded

Table 3 reports the discriminating statistics. Its central estimate compares the 1995–2006 cohort with the 1985–1994 cohort at ages 18 to 24, each measured against the middle-aged of its own day. By this measure, the later cohort is 0.134 points less happy (SE 0.039, t = 3.48), about a fifth of a standard deviation of the happiness item. The same statistic finds no deficit for the 1985–1994 cohort at ages 25 to 29 relative to the 1975–1984 cohort (0.005 points, t = 0.13). At ages 30 to 39 it finds a deficit of 0.052 points (t = 1.72), a value at the design's resolution floor discussed below. For the 1995–2006 cohort at ages 25 to 29, observed only in the depressed 2021–2024 windows, the adjusted comparison is close to zero and imprecise (0.014 points, SE 0.057, n = 445); it cannot yet distinguish a deficit that persists net of the period from one that recovers. All three placebo comparisons among earlier adjacent cohort pairs range from 0.011 to 0.047 points in the opposite direction, and one of them is statistically distinguishable from zero (0.047 points, t = 2.17). Differences of about 0.05 points therefore mark the design's practical resolution, so the 30-to-39 estimates in the rows above sit at that floor and should not be pressed. The central estimate is roughly three times the floor, and the placebo drifts run in the opposite direction from the deficit.

Table 3
Same-age cross-cohort differences, adjusted for period conditions (difference-in-differences against ages 40–59)

Comparison	Difference in differences	SE	t	n (later cohort)
Born 1995–2006 vs. born 1985-94, at ages 18–24	-0.134	0.039	-3.48	963
Born 1985-94 vs. born 1975-84, at ages 25–29	+ 0.005	0.035	0.13	1,103
Born 1985-94 vs. born 1975-84, at ages 30–39	-0.052	0.030	-1.72	1,803
Born 1985-94 vs. born 1965-74, at ages 30–39	-0.042	0.030	-1.38	1,803
Born 1995–2006 vs. born 1975-84, at ages 25–29	-0.014	0.057	-0.25	445
Placebo: born 1975-84 vs. born 1965-74, at ages 30–39	+ 0.011	0.025	0.43	2,285
Placebo: born 1965-74 vs. born 1955-64, at ages 30–39	+ 0.047	0.022	2.17	2,167
Placebo: born 1975-84 vs. born 1965-74, at ages 25–29	+ 0.024	0.032	0.76	936

Note. Each estimate is the mean happiness of the later cohort at the stated ages minus the contemporaneous mean of adults aged 40 to 59, less the same difference for the earlier cohort. Standard errors combine the four weighted cell variances

Regression estimates in Table 4 summarize the pattern. Conditional on age band and period, birth after 1985 carries no penalty; the coefficient is 0.021 (SE 0.015) and strengthens slightly with demographic controls. Birth after 1995 carries an additional penalty of 0.080 points (SE 0.028) in the same specification. This deficit is not a millennial condition that has spread forward. It begins abruptly with the cohorts born after about 1995.

Table 4
Regression estimates and survey-mode sensitivity

Specification	Estimate	SE
Born 1985 or later (conditional on age band and period)	+ 0.021	0.015
Born 1995 or later, additional (same model)	-0.080	0.028
Born 1985 or later, with demographic controls	+ 0.032	0.014
Web response, 2021–2024, reference in-person	-0.093	0.024
Cohort test at 18–24, waves through 2018 only	-0.051	0.050
Cohort test at 18–24, in-person interviews only	-0.074	0.048

Note. Rows 1 to 3 are weighted least squares coefficients with heteroskedasticity-robust standard errors, conditional on age-band and period indicators; row 3 adds sex, race, degree, marital status, and work status. Row 4 is the web-response coefficient from a weighted regression of happiness on age-band and mode indicators within the 2021–2024 waves, with in-person interviews as the reference. Rows 5 and 6 re-estimate the first row of Table 3 on the restricted samples described in the text

The Result Is Not an Artifact of the Survey Redesign

Three checks address the 2021 redesign. Within the 2021–2024 waves, web respondents report happiness 0.093 points below in-person respondents of the same age band (Table 4), so the redesign does depress the recent levels. Youth deterioration nonetheless survives mode-constant measurement. Among in-person interviews alone in 2022–2024, 20.5 percent of young adults describe themselves as not too happy, against 12.7 percent in 2013–2017, when all interviewing was in person. The in-person young-versus-middle-aged gap itself is near zero in those waves, because middle-aged in-person respondents also report depressed levels. The age gap in the pooled recent data is concentrated in the web sample, which is why the cohort tests rather than the raw gap carry the argument. Restricting the central cohort test to waves through 2018 gives a deficit of 0.051 points (SE 0.050), and restricting it to in-person interviews gives 0.074 points (SE 0.048). Both restricted estimates carry the same sign as the full-sample estimate at about half its size, and both are imprecise because the 1995–2006 cohort is thinly observed before 2019. Accordingly, the full-sample estimate is best read as the upper end of a range whose mode-constant lower end retains the same sign but is not statistically distinguishable from zero.

Replication in the National Health Interview Survey

Figure 4 Serious psychological distress (Kessler scale score 13 or higher) among US adults, NHIS. Panel (a) shows weighted prevalence by age band, 1997–2024; the scale was not fielded in 2019, 2020, 2022, or 2023. Panel (b) shows prevalence at ages 18 to 24 by birth cohort, each cohort observed only in years when its full age range lies within 18 to 24

Within the observation window, the distress series reverses the age gradient. From 1997 through 2014, serious distress at ages 18 to 24 ran between 1.9 and 3.0 percent, below the rates of adults in their forties and fifties in every single year. It first rises above them in 2015, three years before the happiness series moved and under the legacy survey design, falls back below them in 2016 to 2018, and stays above them from 2021. By 2024, serious distress reaches 6.7 percent at ages 18 to 24, roughly two and a half times its pre-2015 level. Every band over forty is flat or declining, and the gradient falls monotonically with age. Between 2021 and 2024, both measured under the redesigned survey, distress rose further among the young and fell among the middle-aged and older groups, so the divergence continues within a constant design.

Panel (b) arranges the same data by birth cohort and reproduces the staircase. Each five-year cohort is included only in the years in which its full age range lies inside 18 to 24. Serious distress at those ages is between 2.5 and 2.9 percent for every cohort born from 1975 through 1994. It is 3.6 percent for the cohort born 1995–1999, measured entirely in 2017 and 2018 under the legacy design. For the cohort born 2000–2004 it is 7.4 percent, measured in 2024 under the redesigned survey, a level not directly comparable with the legacy figures. Two facts anchor the comparison regardless. The first step was measured entirely before the redesign, and the redesign moved the all-adult level very little, from 3.9 percent in 2018 to 3.7 percent in 2021. This step begins at the same birth years in both surveys, on different instruments. A period account operating on all ages offers no mechanism for the staircase while every band over forty stays flat. A shock confined to the young could mimic the step, but it would then have to fade as the cohorts age, and the distress of 25-to-29-year-olds keeps rising instead.

Discussion

This study asked whether the collapse in young Americans' happiness belongs to the times or to the people, and the data answer with a division. Part of the recent deterioration is a period phenomenon: happiness fell at all ages after 2018, and part of the measured fall reflects the General Social Survey's move to web interviewing. The youth-specific part is different. Every cohort born between 1945 and 1984 reported nearly identical happiness at the same young-adult ages, whereas the cohorts born after 1995 are less happy, and more seriously distressed, than any predecessor cohort at every age observed. Their raw cohort average has stayed low into the late twenties, although the period-adjusted comparison at those ages cannot yet distinguish persistence from recovery (Table 3).

This cohort reading revises the period reading that the American happiness record supported a decade ago. Working with data through 2014, Twenge et al. (2016) found that period, not cohort, carried the movement in adult happiness. Yang's (2008) analysis of the 1972 to 2004 record singled out the baby boom as the distinctive cohort, and Bardo et al. (2017) read the post-2008 movement as the mark of the Great Recession. Each reading fit the cohorts then observable. What changed the answer is who entered the data: the cohorts born after 1995 reached the adult samples only in the middle 2010s and became numerous in them only in the 2020s.

My results are consistent with the international evidence that the deterioration is concentrated among the young and recent (Blanchflower et al., 2025; Helliwell et al., 2024; Twenge & Blanchflower, 2025). They agree with the Australian panel finding that worsening mental health is concentrated in the cohorts born in the 1990s and, to a lesser extent, the 1980s (Botha et al., 2023), and with American surveillance showing the steepest rises in mood-disorder indicators in the same birth years (Keyes et al., 2019; Twenge et al., 2019). They add that the deficit is now visible in the American adult population, bounded by birth year rather than by age, with the boundary near 1995.

This design identifies the structure of the decline, not its cause. Birth years at the boundary place the affected cohorts' adolescence after roughly 2010, the years in which adolescent depressive symptoms began rising (Daly, 2022; Keyes et al., 2019; Mojtabai et al., 2016). Those same years give the candidate explanations, from smartphone-based social life to economic and climate anxiety, their timing (Haidt, 2024; Twenge, Joiner, et al., 2018), and nothing in the present results arbitrates among them. The results do constrain the candidates: a cause must have arrived on a cohort schedule, striking people born after 1995 during formative years and largely sparing those born earlier. Causes that press on an age group in a period, such as a labor market shock or the pandemic itself, fit the shared component of the decline but not the staircase.

By the standards of subjective well-being research, the magnitude is large. An adjusted deficit of a fifth of a standard deviation is large relative to the associations typically benchmarked in quality-of-life research (Geerling & Diener, 2020). Low life satisfaction also precedes later depressive symptoms rather than merely reflecting them (Joshanloo, 2022), so a cohort-wide happiness deficit is a leading indicator for clinical demand, not a nuisance reading. And the precedent for cohort-patterned deterioration in American data, the mortality rise among successive midlife cohorts, argues against assuming it will fade on its own (Case & Deaton, 2015).

Several limitations should be noted. First, the analysis rests on falsifiable cohort-profile predictions rather than a formal decomposition, because none is identified (Bell & Jones, 2013; Glenn, 1976). Coarse-cell regressions break collinearity by aggregation, and a reader who prefers a different aggregation can recompute the cells from the public data and code. Standard errors treat the weighted samples as independent draws and do not adjust for the surveys' clustered designs. A design effect of the usual size would scale them by roughly a quarter to a half, which the central estimate withstands and the floor-level estimates do not. Second, the GSS changed mode in 2021, and the mode-constant versions of the central test are half the size of the full-sample estimate and imprecise. The conclusion that the deficit is not an artifact of the GSS mode change therefore does not rest on the GSS alone. It rests on agreement with a distress series that brackets the break, including a staircase step measured entirely before either survey changed.

Third, both surveys are repeated cross-sections, so the cohort comparisons describe the average well-being of people born in given years rather than the trajectories of individuals. The finding that the deficit has followed the post-1995 cohorts into their late twenties means that their average has remained low,

not that the same people have remained unhappy. Changes in cohort composition through immigration or differential response could contribute to the pattern, and the period adjustment does not remove them. Panel data that include these cohorts would allow within-person tests of persistence and recovery, at the cost of the attrition and repeated-interview effects that the cross-sections avoid.

Fourth, reported well-being can change without an equal change in underlying well-being. Lenzen et al. (2026) find that younger Australian cohorts apply lower thresholds when reporting distress, and that adjusting for these thresholds flattens, without removing, the rise in poor mental health among recent cohorts. Their adjusted estimates also reveal a midlife peak in poor mental health, which suggests that raw age profiles such as the one in Fig. 2 may overstate the change in the underlying age gradient. Replicating that adjustment would require longitudinal information on individual circumstances that the repeated cross-sections used here do not provide, so reporting thresholds cannot be examined directly. The design bears on the issue in two ways. A reporting shift common to all ages, such as a general change in how Americans describe their mental health, is removed by the period adjustment. A shift confined to recent cohorts is not removed, and it would produce the same birth-year boundary reported here. The estimates are therefore best read as the combined change in underlying well-being and in how recent cohorts report it. The global happiness item may be less exposed than a symptom scale to changing mental-health vocabulary, but the present data cannot test that conjecture, and the NHIS corroboration is the part of the evidence most exposed to such thresholds.

Fifth, the ordinal happiness item cannot support claims about the spacing of categories (Bond & Lang, 2019). The not-too-happy share, which requires no spacing assumption, shows the same pattern, and youth life-satisfaction reports show measurement invariance across adolescent groups where it has been tested (Jovanović et al., 2022). Sixth, the 1995–2006 cohort has been observed only into its late twenties, so whether its deficit persists, narrows, or compounds at later ages is not yet knowable. The 1985–1994 cohort's marginal deficit in its thirties also suggests that the boundary may not be perfectly sharp. Continued observation as these cohorts age is the single most informative future test, and the UK record already shows a parallel deterioration following its young cohorts forward (Blanchflower et al., 2026).

Practical implications differ from those of a period reading. Under a period reading, investment goes to the environment the young currently share: campus and school services, the platforms and labor markets they inhabit, and monitoring of current conditions. Under the cohort reading, services must be planned to follow the affected cohorts as they age, into workplaces, primary care, and family services that youth services do not reach. Age-bracketed youth spending may, within a decade, be aimed largely at cohorts that were never affected, and employers and clinical systems should expect elevated need among people in their thirties.

Monitoring practice should also change. Well-being dashboards that report age snapshots cannot distinguish a cohort-wide deficit from a hard year, so national and organizational monitoring should

report indicators by birth cohort as well as by age. As the present design shows, this can be done with means and differences on existing public surveys.

Conclusion

For half a century, every American birth cohort arrived at adulthood about as happy as the one before it. The cohorts born after 1995 did not. Their deficit is visible in two national surveys and on two instruments, and in the NHIS on both sides of its redesign. In raw cohort averages, it has persisted from the earliest adult ages into the late twenties. Part of it may reflect how these cohorts report their well-being rather than how they fare, and the present data cannot yet apportion the two. The decline of young Americans' happiness is partly a story about hard years, and those pass. It is at least as much a story about particular generations. Well-being research, and the services that draw on it, should now track these cohorts forward rather than watch the age bracket they are already leaving.

Data Availability

The General Social Survey cumulative file (1972–2024, Release 3a) is publicly available from NORC at gss.norc.org without registration. National Health Interview Survey public-use files are available from the National Center for Health Statistics at cdc.gov/nchs/nhis. All analysis files are constructed from these public sources as described in the text.

Code Availability

The complete analysis code, from raw-data download through every table and figure, is available from the author and will be deposited in a public repository upon acceptance.

Declarations

Author Contribution

S.T. is the sole author and performed all parts of the work

Acknowledgement

The author conceived the research, conducted the analysis, and is responsible for all intellectual content, arguments, and conclusions in this manuscript. Claude (Anthropic) was used iteratively and extensively throughout manuscript preparation for language editing and revision, including substantial rewriting of passages drafted by the author for clarity, structure, and style. The author directed all such revisions, reviewed and approved the resulting text, and takes full responsibility for the accuracy, integrity, and originality of the final manuscript.Every passage the system touched was examined in substance by the author, who holds the subject-matter competence to approve, alter, or reject it and who holds editorial responsibility for the manuscript. The manuscript therefore meets the conditions of the human review

and editorial control exemption in Article 50(4) of Regulation (EU) 2024/1689, whose transparency obligations became applicable on 2 August 2026. The declaration is made in full regardless, so that no reader has to infer the extent of the assistance. Machine-readable marking of synthetic output is an obligation of the system provider under Article 50(2) of that Regulation and not of the author; no marking, watermark, or provenance metadata present in any output was removed, altered, or obscured at any stage, and no assisted passage is presented as unassisted.Claude was additionally used to write analysis code and to produce figures from author-specified content. The author specified every design decision, ran and inspected the output, and verified the reported quantities against the source data. Generative artificial intelligence was not used to supply references; every source cited was verified by the author against the original publication. No image in this manuscript was produced by a generative artificial intelligence system; the figures were computed from the data by the analysis scripts. Claude is not an author of this manuscript and is not listed as one.

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Data availability: The General Social Survey cumulative file (1972-2024, Release 3a) is publicly available from NORC at gss.norc.org without registration. The National Health Interview Survey public-use files are publicly available from the National Center for Health Statistics at cdc.gov/nchs/nhis. All analysis files are constructed from these public sources as described in the manuscript.Code availability: The complete analysis code, from raw-data download through every table and figure, is available from the author and will be deposited in a public repository upon acceptance.

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Figures

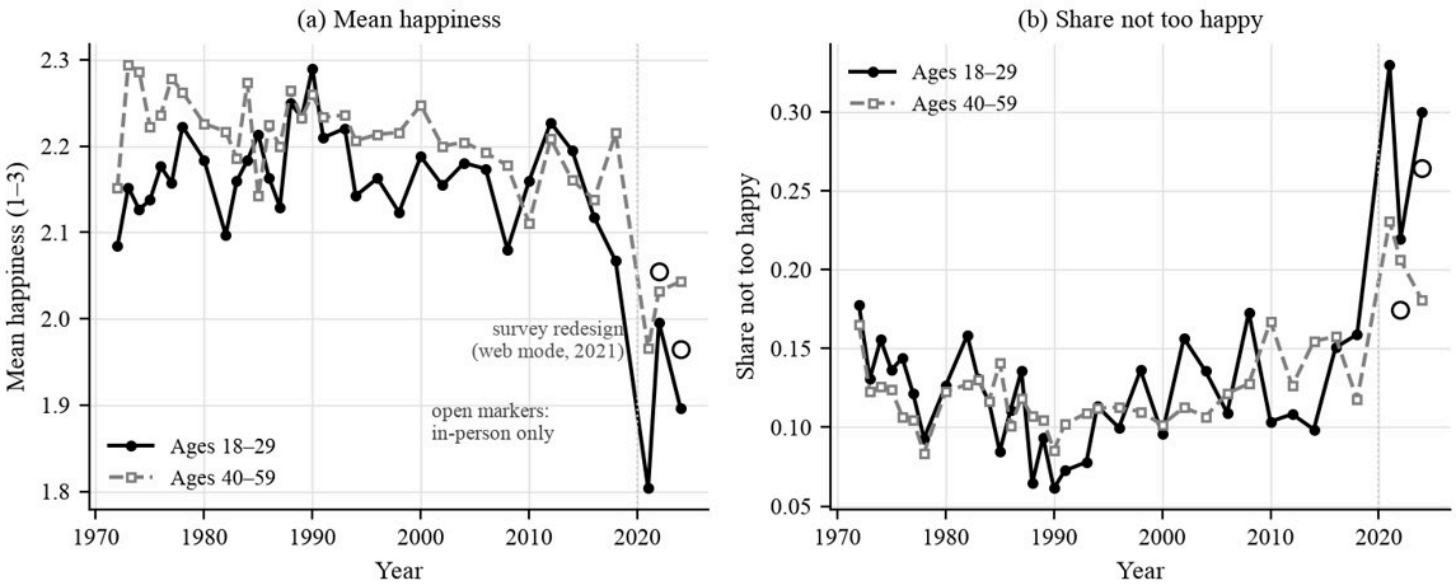


Figure 1

Happiness of adults aged 18 to 29 and 40 to 59 by survey year, GSS 1972-2024. Panel (a) shows weighted mean happiness; panel (b) shows the weighted share answering not too happy. Open markers show in-person interviews only, in 2022 and 2024; the dotted vertical line marks the 2021 move to web self-administration

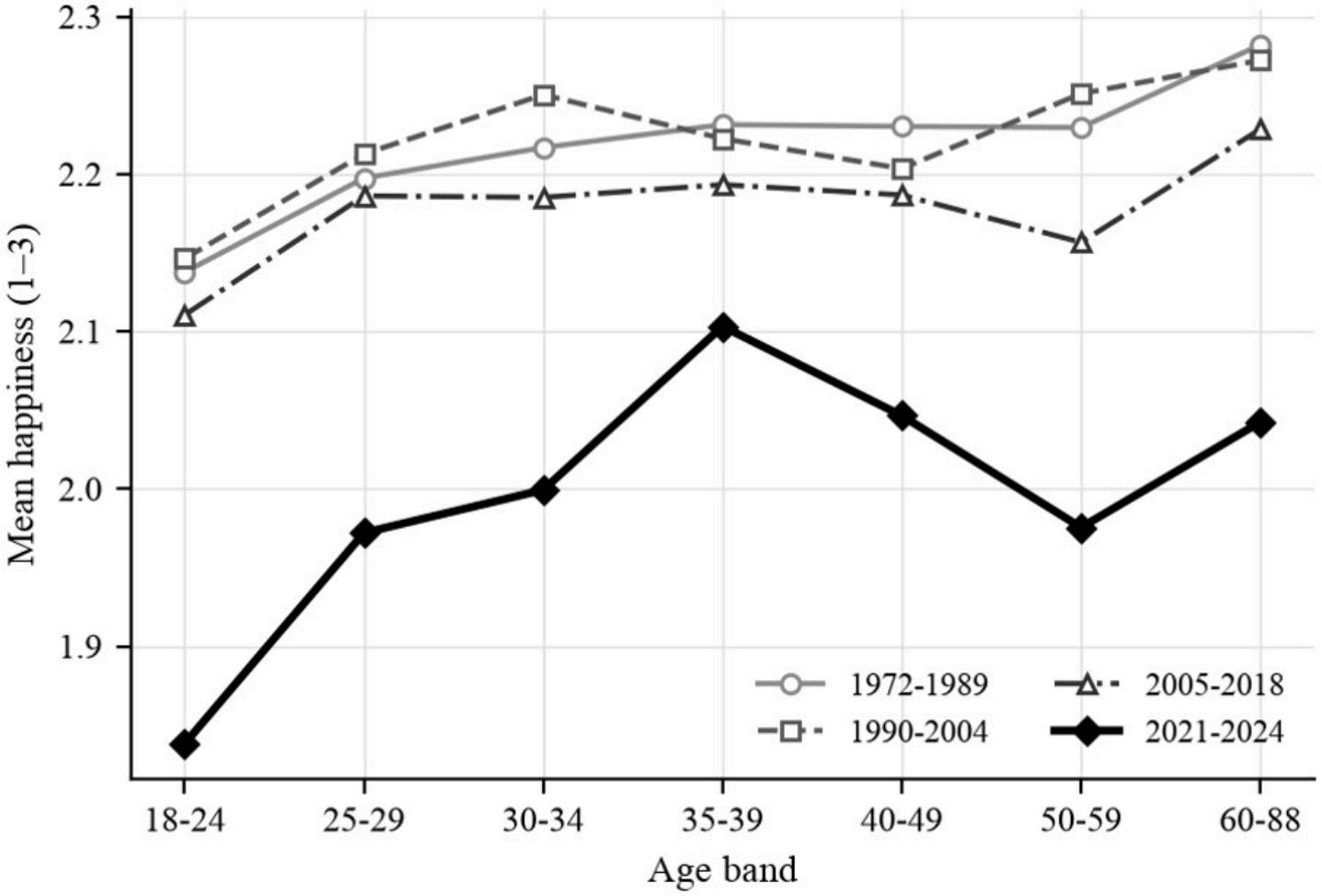


Figure 2

Weighted mean happiness by age band in four eras of the General Social Survey

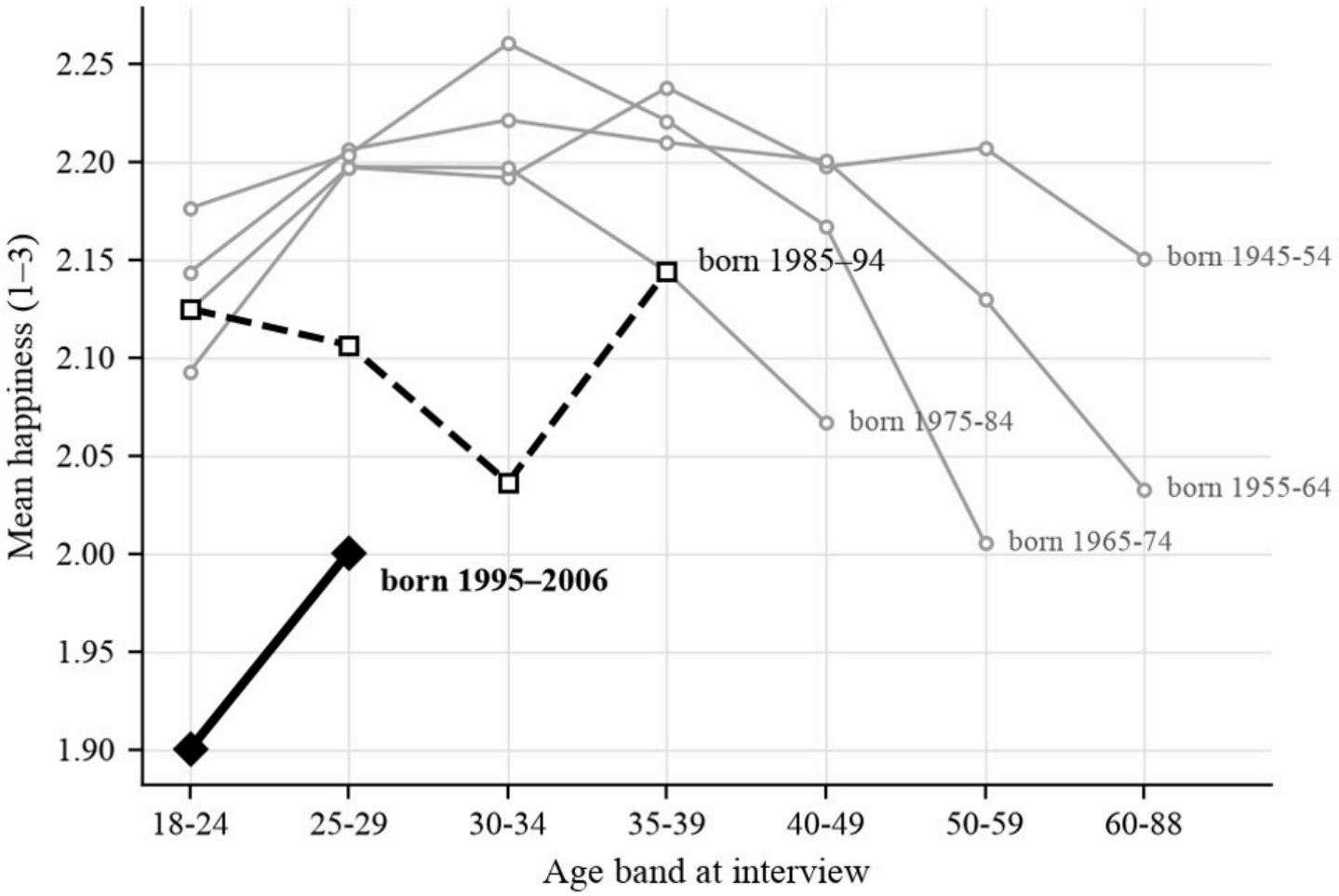


Figure 3

Weighted mean happiness by age band at interview, by birth cohort, GSS 1972-2024. Cells with fewer than 60 respondents are omitted

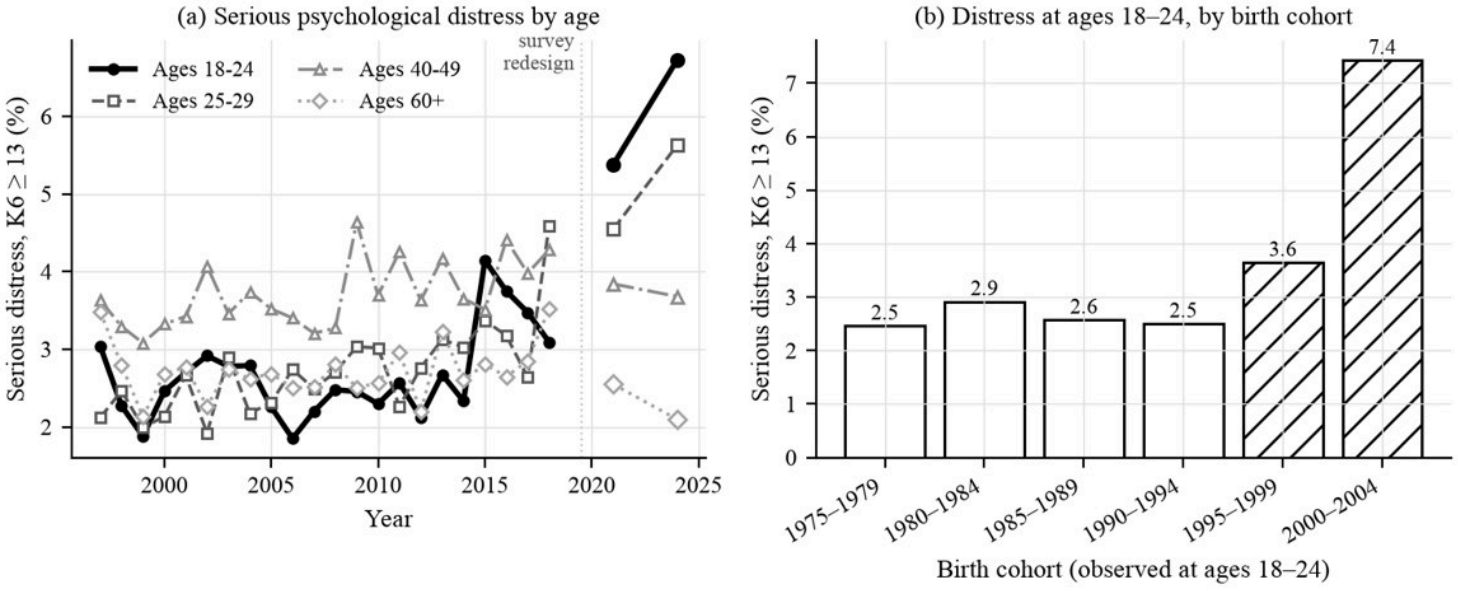


Figure 4

Serious psychological distress (Kessler scale score 13 or higher) among US adults, NHIS. Panel (a) shows weighted prevalence by age band, 1997-2024; the scale was not fielded in 2019, 2020, 2022, or 2023. Panel (b) shows prevalence at ages 18 to 24 by birth cohort, each cohort observed only in years when its full age range lies within 18 to 24