

A Response to 'Another Attempt to Move Beyond the Cross-sectional U Shape of Happiness: A Reply' by Galambos, Krahn, Johnson and Lachman (2021)

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This is a reply to GKJL2's response to the Blanchflower and Graham (2021a) – henceforth BG - response to Galambos et al (2021a)'s original paper on this topic (GKJL1). As such, this paper is a point-by-point response to GKJL2 (2021). This review aims to highlight errors in most of the points raised, as well as a very large difference in views on what is appropriate statistical analysis and inference. There is absolutely nothing here to challenge BG's argument that the evidence for a U-shape is '*overwhelming*'.¹

I do not find anything that is new here (GKJL2) that would warrant publication, not least as most if not all of the points are simply incorrect methodologically. This is not an attempt to *move beyond* the U-shape it is simply a repetition of the authors' refuted claims that the evidence of a U-shape is not robust, fragile and tiny. Nothing could be further from the truth.

The GKJL2 paper is in fact nothing more than a restatement of the highly questionable arguments from the original Galambos (GKJL1) article that was the focus of the BG (2021) response. It provides no additional evidence and does not even discuss the empirical evidence BG presented. The only reason to publish it, in my view, is to continue the debate in the PPS issue. But for that to make sense, the additional critiques of the article contained here in this review should also be included.

In GKJL1, the authors concluded that “*the U shape is not as generalizable or robust as often portrayed,*” based on their examination of 21 studies. BG showed that such a statement is in error. The evidence is indeed generalizable and robust including in the vast majority of papers GKJL1 reviewed. BG showed that GKJL1 wrongly classified many of those papers as not showing U-shapes when in fact they did.

There continue to be many questionable claims, for example the statement in GKJL2 that “*cross-sectional studies confound age with cohort differences and are not appropriate for drawing conclusions about within-person change because they only assess between-person (age) differences*” is incorrect. Many studies include cohort effects with little impact on the results (e.g. Blanchflower, 2021). Equally important, many studies using longitudinal data, properly done, essentially find the same.

To study the issue longitudinally requires following individuals over their life course. There are in fact three pre-eminent *birth* cohorts in the world that do this from the UK – they are the Douglas

¹ It has come to our attention that the Galambos et al (2021) paper was accepted based on the recommendation of two referees who it turns out were not sent and hence did not read the Blanchflower and Graham (2021) critique. It is normally inappropriate to report on the reviewing process but in this case, what happened is so egregious it justifies full transparency. Here is the direct statement from one of the reviewers. “*It would have been nice to see the B & G paper, so as to better evaluate the Galambos et al. response. That said, I have a pretty good idea of what B & G said in their article. I agree with everything Galambos et al. say in their very thoughtful piece. All of their points are valid ones.... More power to Galambos et al. for taking them on*”

It is unusual to say the least for referees to guess what is in a paper. That means both referees were unaware of the misclassifications and the additional 359 papers that found U-shapes identified in B&G missed by GKJL1. Having a “pretty good idea” is not good enough. We asked for a right of reply to this paper, but this was declined by the editor.

cohort of 1946, the National Child Development Study (NCDS) 1958 and the British Cohort Study of 1970. Other studies such as the BHPS or the GSOEP do not follow individuals for so long and do not have childhood characteristics. In a new 2021 paper in Psychological Medicine that examines all three of these cohorts the authors conclude "*across three post-war British birth cohorts midlife appears to be a particularly vulnerable phase for experiencing psychological distress.*" (Gondek et al 2021). The same result as found in cross-section data.

It is also relevant that happiness is impacted by macro-economic variables (Blanchflower et al 2014) which can be controlled for with pooled cross section time series – via the inclusion of country/state and year fixed effects as used in BG (2021) or unemployment or inflation rates. Of importance also, ignored by GKJL2 is the evidence that deaths of despair – from suicide, opioid and alcohol poisoning peak in midlife, as documented by Case and Deaton (2020) for the US. There is recent evidence that, cocaine deaths in the US peak in midlife as do deaths from drug poisoning in the UK (Blanchflower and Graham, 2021b). GKJL do not seem to understand that the mortality data showing peaks in midlife helps confirm the morbidity data.

Between December 9th and 21st the Household Pulse Survey conducted by the US Census Bureau reported that there was an inverted U-shape in age in the taking of anti-depressants that maximized in the age range 50-59 and then declined. Respondents were asked if over the last 4 weeks they had taken '*prescription medication to help with any emotions or with your concentration, behavior or mental health?*'

Age 18 - 29	24.8%
Age 30 - 39	28.3
Age 40 - 49	28.6
Age 50 - 59	30.8
Age 60 - 69	28.9
Age 70 - 79	24.4

This also seems like a useful validation of the hump-shape in unhappiness that peaks in midlife. It is consistent with the findings of Blanchflower and Oswald (2016) who also found an inverted U-shape in the use of anti-depressants across 27 European countries.

To more specific points.

1. GKJL cite Morgan and O'Connor (2017), who they say "*reported an M shape in Eurobarometer life satisfaction data.*" Recently, they argued "*...the U-shaped relation is, in fact, not everywhere*" (Morgan & O'Connor, 2020, p. 201)." But they failed to cite the published response by Blanchflower (2020) in the same issue showing the M-shape only emerged because the authors inappropriately dropped happy students from the analysis. Once the students – an important life-cycle cohort – were added back into their same data and the same regressions were re-run and a U shape rather than an M shape emerged again. Wrong again.

2. The GKJL2 authors draw three graphs Figures 1a to 1c – that are not Cantril Ladder as they wrongly claim - showing that if the scale on the axes is made bigger, the obvious statistically significant and large U-shape becomes more like a straight line as the scale is increased. This does

not make sense and seems designed solely to mask a result they disagree with. What matters is statistical significance and size. BG show that the size of the decline in wellbeing from youth to midlife is and GKJL2 show in Figure 1a is around 1.1 life satisfaction points and highly statistically significant. That is from 8.4 at age sixteen to 7.3 at age fifty. No amount of tinkering around changing the scale on a graph to hide the U-shape it makes that go away. The change is statistically significant and comparable or larger than major life events such as becoming unemployed or having cancer.

The only event that has resulted in a drop in well-being of the same magnitude is the drop in wellbeing during the COVID pandemic. The Office of National Statistics in the UK who conduct the surveys used in Figures 1a-1c have conducted a further series of COVID surveys during the pandemic to look at the change in life satisfaction. The mean life satisfaction score from 2015-2019 in prior data was around 7.7 and in the survey of 13-17th January was 6.4, a drop of 1.3 life satisfaction points.² So a drop of 1.1 points seems large. The GKJL2 authors have not identified a single life event that is anywhere comparable in magnitude to such enormous changes.

3. *"We referenced previous studies on life satisfaction and happiness and reviewed a sample of 29 relevant empirical studies published in peer-reviewed journals between 2013 and 2019. It was not the goal of our paper to do an exhaustive literature review or meta-analysis, and to tally up the total number of studies that found, or did not find, the U shape. Instead, we wanted to show support for the view that not all researchers find the U shape, and when they do, they often also illustrate variability in age related patterns of happiness. We concluded that the U shape is not as generalizable or robust as often portrayed."*

And later

"Without a narrative, systematic, or meta-analytic review published in a peer-reviewed journal, however, it is not possible to judge their claim that all studies they located are supportive of their stance on the U shape."

This makes no sense. The authors in their original article did not say they were examining a highly selective tiny percentage of the papers published in peer reviewed journals on the U-shape. They said:

"To inform our discussion, we conducted a literature search of relevant articles published from January 2013 to June 2019. The search was restricted to 2013 and later because of an earlier literature review that cited articles on the U shape published through 2012 (Ulloa et al., 2013). The Web of Science Core Collection and MEDLINE were searched with these terms: (well-being OR life satisfaction OR happiness) AND age AND u-shape."

It turns out that these search criteria were entirely inadequate given that they missed many more studies that did find U-shapes. Presumably if those studies had been found the authors would not have claimed there was a "purported" U-shape (GJKL1, p1). They presented no data or new

²<https://www.ons.gov.uk/peoplepopulationandcommunity/healthandsocialcare/healthandwellbeing/datasets/coronavirusandthesocialimpactsongreatbritaindata>

analysis but simply used their survey of 21 studies to make largely false claims about the validity of the claim that there was a U-shape. Nor did they conduct a meta-analysis. BG did not claim that *all* happiness and unhappiness papers found U-shapes and did not say these were the only papers and that every paper on the issue found a U-shape. They agreed there were a few cited by GKJL1's Table 1 that did not find the U and there are obviously a few others.

GKJL1 set the main criteria for inclusion to be that the article was published in a peer review journal in English between 2013 and 2019 and spanned the teens or 20s into the 60s. Yet BG also cited a large number of papers that did find a U-shape that fit the original GKJL1 criteria with the list of papers being updated over time that the authors missed. This list is being continuously updated and the most recent version is attached as an appendix to this review.³

It is relevant that in addition to the 21 studies examined by GKJL1, BG have now found 359 additional papers not surveyed by Galambos et al in either of their papers that all found U-shapes in age in well-being. This, astonishingly, includes another 165 published in peer reviewed journals in English between 2013 and 2019 that also found U-shapes that fit the criteria set GKJL1 in their original paper that they missed.⁴

In addition, BG have now identified a further 73 papers published in peer-reviewed journals, or were forthcoming, in English in 2020, 2021, plus 28 book chapters and working papers. BG also found there were 93 papers published in peer reviewed journals prior to 2013 missed by the Galambos team. GKJL1 and GKJL2 have found significant U-shapes and GKJL2 has not disputed any of the additional 338 paper's findings of U-shapes. It isn't as if GKJL1 missed one or two papers; they found 21 papers that they mostly misclassified, all of which found U-shapes and missed eight times as many which found U-shapes; hence they presented a totally distorted and biased picture of the literature. Their comments on the lack of evidence for a U-shape should be ignored in their entirety.

Despite having plenty of opportunity to do so GKJL2 continue to defend all of the conclusions drawn in the first paper without apparently examining *any* of the additional studies BG have identified, including the 164 that exactly fit the author's own criteria for inclusion being published in peer-reviewed journals in English from 2013-2019. GKJL2 did not challenge the claim that the papers reported in the appendix all found U-shapes or that they fit their own criteria for selection.

The fact that so many papers were ignored brings into question GKJL1's claim they continue to make that the finding of a U-shape is not stable or if its then it is trivial. BG (2020) are entitled to criticize the appropriateness of the search criteria used to identify studies that did or did not find U-shapes. The exclusion for example of 61 papers identified as published alone in the peer reviewed journal Social Indicators Research, for example seems a major weakness worth noting (Blanchflower and Graham, 2021c). I also identified 65 peer reviewed papers in the Journal of Happiness Studies that somehow the Galambos team missed in their search. This reduces the credibility of any claim that findings of a U-shape are "*not robust*" (GKJL1, p1), when they clearly

³ The list of studies finding U-shapes is continuously updated here
<https://sites.dartmouth.edu/blanchflower/files/2021/01/jan-7-2021papers-finding-U-shapes.xlsx>

⁴ There are a small number of other papers that argue there is no U-shape that are not surveyed here, but GKJL1 or GKJL2 do not identify any of them. I simply report papers that do find them.

are robust across many hundreds of published studies. Even in the 21 studies picked by GKJL1, as BG show, the vast majority show U-shapes.

The papers published between 2013 and 2019 that GKJL missed were also published in journals across a range of disciplines and fields including; Cities; Population Ageing; Economics and Sociology; Journal of the Transportation Research Board; Epidemiology and Health; Journal of Social Policy; Quality of Life Research; the Journal of Comparative Economics; Journal of Finance and Economics; Review of Economics of the Household; The Manchester School; International Journal of Environmental Research and Public Health; Journal of Education and Health Promotion; Psychiatry Research; Addictive Behaviors; Journal of Environmental Planning and Management; Journal of Economic Psychology; Journal of Social Sciences; Review of Religious Research; The Journal of Socio-Economics: Psychology and Health; Journal of Family and Economic Issues; British Journal of Sociology; The Economics of Peace and Security and the Journal of Leisure Research.

The 71 new papers in refereed journals in English that have been published in 2020 or 2021 or are forthcoming are in a whole range of journals including JEBO and AJPH among others., as can be seen in the appendix. So, this is not a dead issue and referees in a whole range of journals and fields have accepted these findings.

4) GKJL2 do not question the finding that there were many more papers that found U-shapes but simply claimed "*our goal was not to conduct an exhaustive literature review*". A review of the literature presumably should be a reasonable summary of the research findings: it is clear GKJL1 was not as it left out many studies that challenged their conclusions. That meant they misrepresented the findings of the extensive literature.

It seems rather unusual not to at least consider the evidence, or adapt your views somewhat, when it is pointed out to you that there are over two hundred peer reviewed papers that you missed in your literature review, some using cross-section and some using longitudinal all of which are unresponsive of your claims. GKJL2 could have used this as an opportunity to identify a list of other papers that **did not** find a U-shape but failed to do so. The obvious conclusion to be drawn is that their claims were not based on a careful reading of the literature and hence, wrong.

5) GKJL2 continue claiming that "*cross-sectional support for the U shape is mixed.*" The empirical evidence shows that to be essentially untrue although it is true that there a few studies that are not supportive, but they are a tiny minority. The paper by Laaksonen they cite as evidence for this says: "*we find some support for the U-shape curve over 30 countries*". Hardly strong evidence against a U-shape.

6) GKJL2 suggest that "*longitudinal support for the U shape is mixed.*" Gondek et al (2021) put this claim to rest and is consistent with the claims of Cheng et al. It seems hard to argue that the author's claims about the Cheng paper were correct when BG cited a co-author of the paper Powdthavee who made clear he disagreed and confirmed GKJL2's comments misrepresented what they did in their paper.

7) GKJL2 refer to a paper using the US Health and Retirement Study by Hudomiet et al. (2020) and use it to argue that cross-sectional findings can bias the age profile in old age upward due to differential mortality and nonresponse: *"Individuals with higher life satisfaction and in better health tend to live longer and to remain in the survey, causing average values to increase"* and they *"conclude that the optimistic view about increasing life satisfaction at older ages based on cross-sectional data is not warranted"*.

The Hudomiet paper is important as it shows that happy people live longer and after the age of 65 or so there needs to be a correction for mortality. This is because of the death of partners and ill health in the last three years of life. Once that is done, then life satisfaction falls. Conveniently GKJL2 do not say that the authors find that *"the patterns are consistent with the literature in that life satisfaction monotonically increases after age 51."* The U-shape is about a decline from youth to middle age and then a subsequent rise through to the 60s. In most papers on the U-shape including Blanchflower (2021) uses a cutoff at age 70 is used precisely to account for differential life expectancy and differing retirement rules across country. Once again GKJL mis-represent the findings of a paper.

8) In the context of the Hudomiet paper that looks at the old GKJL suggest that *"support for the U shape requires that both younger and older adults have higher levels of happiness than those in midlife."* Of course, it does not; the U-shape relates to changes from youth to midlife to around age 70. Hudomiet et al question what happens *after* retirement. Changes after retirement age are mostly excluded from these studies due to deaths of partners and closeness to death. Across countries what happens in older age varies due to differences in life expectancy and pension and other support arrangements, which are typically much weaker in poor countries. For this reason, Blanchflower (2021) explicitly examined samples age under seventy in 145 countries including less developed countries.

When older age groups over retirement age are included, then there is some evidence of subsequent turns in the data picked up by a polynomial of order three to pick up a second turndown in the data. Again, this is why the samples are normally restricted to under age seventy. The nature of the turns will likely vary by country and Hudomiet et al only look at the US, but there are many other panel retirement surveys, and we are likely to see other researchers exploring this mortality selection issue across countries. Mortality selection after age 70 is not that relevant to the midlife U-shape especially as GKJL1 specified that the age range for studies to be reviewed should be up the 60s.

9) In relation to Kessler et al., (2010) GKJL suggest that *"yet a close look at the results reveals that for many dimensions the oldest age group (65 years and older) showed considerably lower prevalence of problems than all the younger age groups"*. But as noted above, what happens to the oldest age groups approaching the final years of life, is likely impacted by mortality bias which says nothing about the U-shape that exists between the ages of sixteen and seventy.

10) *"BG provide no response as to the importance of considering diversity in patterns"*. Statements like 'unemployment fell this month' doesn't mean that it fell for everyone. And yet we still use unemployment as an important measure of social welfare. Standard metrics and measures accept that there are outliers, but there are not enough of them to change the overall

patterns they address. In any case, diversity of patterns is not what the BG response is about and obviously not everyone has a midlife crisis; it is about the overall patterns across populations.

11). *"BG are silent on the meaning of the longitudinal evidence we presented and do not comment on the implications of studies demonstrating significant heterogeneity in trajectories of change in different indicators of well-being across life."* The evidence of a statistically significant U-shape does not preclude the possibility that some people do not experience a midlife crisis. That is how statistics works. As GKJL2 say *"some people are stable, some increase, and some show a decrease in midlife"* (p.14). The evidence though is supportive that a decrease in midlife is the preponderant trajectory.

12) Additionally, the authors claim that *'selective attrition in longitudinal research is something that can be identified, characterized, and addressed.'* That is simply not the case. It is well-established that sample attriters are less happy than remaining respondents. There is also a problem of measurement error in longitudinal studies that we have known for many years biases estimates to zero (Freeman, 1984). In addition, there is a paucity of longitudinal data. These studies are costly, and we have few that follow people through their lifespans – the only major birth cohorts, as noted above are for the UK. Indeed, there are none in developing countries. Given this data limitation, and that the findings on this question are essentially the same as those in the cross-sections, we do not believe we can hinge the answer on longitudinal data alone.

13) *"The most important question concerns diversity in happiness and its sources"*. I disagree; the most important issue being discussed is whether a U-shape in happiness exists and it appears to. The fact that some people don't experience such a crisis is inevitable, given the nature of the statistical analysis but it turns out to not matter econometrically. On average the U-shape exists. GKJL1 's main point is that the U-shape is *"purported"*. It is hard to see why diversity of happiness is the most important issue in a reply to BG given they nowhere discuss it.

14) GKJL2 also made a number of bizarre claims.

a) *"The pool of respondents at different ages for cross-sectional studies is inherently biased as, for example, those at older ages who have **died** are no longer available for inclusion in the study."*

Nationally representative cross-section studies of people who are alive are not intended to be representative of dead people. Inevitably the unemployment rate calculated by the Bureau of Labor Statistics from a representative sample of the workforce in the US in January 2020 by construction does not include the labor force activity of the dead. The Census Bureau does however provide detailed data of who died as well as their characteristics. Hard to know how to respond.

b) *"Mean levels alone, however, do not provide important information about variability in happiness at any given age."* Actually, they are a pretty good measure, although there is inevitably variation around that mean. Bigger sample sizes generate more precise estimates. The authors do not seem to not understand that estimates have standard errors. If wage growth rises by 3% over the last year some people had 10% while others had 10% declines and everything in between, that is how arithmetic means work. Big samples give more precise estimates. Plus there is lots of

evidence on the variability of estimates which show U-shapes exist by gender, race, across states and regions, by education group, by marital and labor force status and country and other characteristics (Blanchflower and Oswald, 2008).

c) *“(U)sing such mean level differences to portray midlife as a low point could be seen as irresponsible”*. This is nonsense. The average number of children a couple has is 2.1, some have none some have ten. The mean is simply a descriptive statistic. The fact that some in the media *“promote misrepresentations about a normative midlife crisis such as portrayed in the media”* is irrelevant. It is irresponsible to report means, really? Each month the BLS reports the Economic Situation Report on the labor market and it is the most watched publication in the world and moves markets. The unemployment rate is a survey mean. Is publishing that 'irresponsible'? No.

d) The authors suggest that BG claim things about everyone which makes no sense *“... if ALL potential confounds that could account for cohort differences are controlled, then the age differences in a cross-sectional analysis would reveal aging effects. Notwithstanding the remarkable assumptions underlying this argument (e.g., that ALL potential confounds have, in fact, been identified)”*. No such claims were made and no social scientist would ever make such a ridiculous claim about **all** possible confounds. BG simply control for the confounds that are standard in the extensive literature. They have documented extensively the lack of sensitivity of the estimates to changes in estimation procedures and controls. Social science if it is about anything is about probabilities. As an example, a ball is taken from a bag ten trillion times and in every case it is black and it is then replaced and the drawing is repeated. Can we conclude with certainty that ALL balls are black? Of course not.

e) The authors suggest that they are doubtful *“whether cross-sectional studies, even those asking the same questions of different samples year after year (e.g., General Social Survey data), can say anything about aging at all.”* This seems extreme. Interestingly Blanchflower and Oswald (2004) that does exactly this, and has a lot to say about aging, using the GSS to look at well-being over time, is the most cited paper in happiness economics, with 3943 Google cites.

f) *“If an effect size is so small as to require samples in the many thousands to attain statistical significance, how meaningful are the age differences?”* An effect size may be large, but a sample may be too small to detect statistical significance. An effect may be small and that may also require a large sample to attain statistical significance.

g) The authors go on to suggest *“Many highly influential studies in psychology and sociology, and no doubt, in economics, have presented statistically significant findings with samples containing fewer than 1000 participants.”* I know of none in economics in the last two decades and the authors cite none.

h) *“Many factors (e.g., illness, poverty, immigration, peacetime versus wartime) are sources of diversity in happiness trajectories. They are not to be explained away or controlled, but to be welcomed as potential predictors so that we can better understand interindividual differences in intraindividual change in happiness.”* Statistical analysis tests for significance; without that it is impossible to generalize about particular findings. They either help to explain movements in the dependent variable. so the coefficient is different from zero or they don't and the coefficient isn't

different. It is unclear what 'welcoming' has to do with much of anything; it certainly doesn't appear in any econometrics textbook.

i) "*Present bias is indeed a limitation of retrospective studies, but one must acknowledge that **all** self-report measures raise similar concerns.*" Actually, that seems unlikely and I certainly don't acknowledge it. All self-report measures do not raise 'similar concerns'. 'Are you in pain now?' is one example. If a respondent is asked about how they felt yesterday it seems they are likely to more accurately recall that than how they felt fifty-seven years ago when they lived in Japan. Plus, it is possible to place the current response in context, for example, what the unemployment rate was that month in Tennessee where they live now, say, as a means to check for potential bias.

j) '*Longitudinal research on other well-being indicators challenges the U shape.*' The evidence presented in this section is almost entirely orthogonal to the argument about whether there are U-shapes. It is bizarre to claim that evidence of stress among some young Canadians (p12) or suicidal thoughts among ages 18-25 in the US has much anything to say at all about the U-shape. People experience stress through the lifespan but Graham and Pozuelo (2017) provide clear evidence of a hump-shape in stress across many countries.

k) GKJL2 claim that BG say that happiness "*invariably*" follows a U-shape (p5). No such claims were made, and the word invariably is not something that a social scientist would ever use. BG simply showed on average a U-shape exists in the data and the evidence for this is widespread across countries and time. There will be exceptions though but on average U-shapes are found in the data. Saying that, on average something is true, doesn't mean there are no exceptions, as documented in potentially unrepresentative anecdotes.

l) "*(A)ging is a within-person phenomenon **only** observable with repeated assessments of the same people.*" Really? It seems extreme to argue that longitudinal data is the **ONLY** way to look at aging; it is one way. The US Census Bureau are running weekly cross-section surveys to look at the impact of the pandemic – measured by anxiety, depression and ability to cope across age groups.

Moreover, GKJL2 made the following claim on page 12 in relation to young people, citing approvingly two recent papers that didn't in fact use longitudinal data.

"Understanding the state of well-being among young people is critical, given serious levels of depression, anxiety, and stress in Canadians under age 25 (Nwachukwu et al., 2020) and recent increases in mood disorders and suicidal outcomes among 18-25-year-olds in the United States (Twenge et al., 2019)."

Of note is that Nwachukwu et al., (2020) use cross-section data from a survey of 8267 Albertans in March 2020. On page 2 of their paper, they note that "*this was a cross-sectional survey exploring the mean differences of perceived stress, anxiety, and depression symptom scores among subscribers of various age categories...*"

Twenge et al (2019) used cross-section time series from the National Survey on Drug Use and Health (N=611,880) for the years 2005-2017, a large nationally representative sample of

Americans age 12 and older. This is the same type of data used by BG. The authors examine severe psychological distress over time by age groups in the cross-section time series.

Longitudinal data seems to not be the *only* way to look at aging when it suits.

m) "*BG and their colleagues have replicated their analyses and findings on the U shape over and over again.*" Replicating results and checking out their stability across time, country and characteristics, including controls and excluding them, as well as focusing on the size of estimates and whether they are large or not seems the right way for science to proceed.

I find both GKJL1 and GKJL2's arguments totally underwhelming and mostly bizarre. The job of social scientists is to examine the evidence and not to ignore or distort it. GKJL1 and GKJL2 are basically advocacy not science. The evidence of a midlife low in well-being is overwhelming and one of the most well-documented patterns in social science data.

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Appendix 380 papers finding U-shapes in age in well-being
380 Papers finding significant wellbeing U-shapes in in age

21 Papers from Galambos #1 finding U-shapes

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