

# Wellbeing public policy needs more theory

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**Abstract:** Advocacy for ‘wellbeing public policy’ (WPP) requires suitable evidence. Endorsing the ambition to focus policy on wellbeing outcomes, we nevertheless argue that the current evidence base is deficient due to a lack of theory. For the purposes of our analysis, we identify theory narrowly with conceptual clarity and the modelling of causal mechanisms underlying statistical regularities. The prevailing focus on identifying ‘drivers’ of wellbeing and their effect sizes is not well suited for such theorising. We show that this status quo creates potential for conceptual confusion, incorrect aggregation, poor robustness and external validity of policy evaluations, inept interventions, and raises the prospect of a ‘Lucas critique’ in wellbeing economics. We discuss what sort of theory addresses these pitfalls, and where WPP could proceed fruitfully even in the absence of such theory. Ultimately, we call upon wellbeing experts to invest in developing theory as this would improve the basis for WPP and outcomes for those affected by it. Moreover, such theoretical contributions from the field of WPP could spillover to other disciplines, extending the reach and influence of wellbeing research.

**Keywords:** wellbeing; happiness; public policy; causation; theory

## 1. Introduction

Wellbeing is increasingly advocated in many countries as a viable and more appropriate target than conventional economic metrics for shaping public policy (e.g. OECD 2020; Diener et al. 2009; Frijters et al. 2020). Wellbeing policies are attractive given the drive to go ‘beyond GDP’ for assessing societal progress (De Neve & Sachs 2020). A range of wellbeing metrics based on large-scale surveys is increasingly available, with longer time series and more complete coverage (across countries and at higher resolution within countries), enabling a large and growing body of empirical investigation. As Adler & Seligman (2016, p. 1) express it:

If existing economic measures of prosperity are complemented with wellbeing metrics that better capture changes in individuals’ quality of life, decision makers will be better informed to assess and design policy. The science of wellbeing has yielded extensive knowledge and measurement instruments during more than three decades.

There is indeed a large and growing body of empirical work on wellbeing measures—usually measures of subjective wellbeing (SWB). SWB, as defined in psychological science and happiness economics, consists of experiences and evaluations of life. Experiences are typically measured by asking people whether they have experienced certain moods and emotions in the last 24 hours, such as joy, happiness, stress, or loneliness. Evaluations are typically measured by asking people to assess their life satisfaction and sense of meaning and purpose on a scale from 1–10. The wide

use of such data in surveys enables estimation of their relation to socio-economic variables and policy levers. But how robustly can such data underwrite wellbeing public policy (WPP)?

In this article we explore one particular obstacle to realising the ambition of WPP – namely that the underpinning research is too focused on identifying ‘drivers’ and estimating effects without a theory that explains why, where, and when these effects hold. This is why we formulate our criticism as ‘not enough theory’. While ‘theory’ has a variety of senses in social science (Abend 2008), we focus on the sense that is especially widespread and uncontroversial in policy-related work – theory as models of causal mechanisms underlying observable phenomena. This includes conceptual clarity regarding what these phenomena and their mechanisms are. Such mechanisms are typically formulated at a level of generality that enables deployment in a variety of contexts, but can be made more specific to local environments.<sup>1</sup> Elsewhere in policy-related sciences, mechanistic thinking is recognised as important for designing interventions and understanding why they may or may not work (Cartwright & Hardie 2012; Cartwright & Montuschi 2014; Parkinnen et al. 2018; Jenke 2022). But wellbeing science has largely settled into a pattern that emphasises data-mining and prizes statistical evidence above all. This reflects a desire to make positive statements about what are by definition evaluative or normative concepts (List & Valentini 2016). We discuss why this is a problem and offer some correctives.

Numerous commentators have noted that the SWB literature has taken a largely atheoretic approach to studying its constructs of interest (Alexandrova 2017; Cohen Kaminitz 2018; Biswas-Diener & Kashdan 2021; Ryff 1989; Argyle 2001; Marsh et al. 2020; Hersch 2020). Here we focus on a specific aspect of this issue, namely the ‘black boxing’ of psychological, economic, or cultural dynamics that underlie statistical relationships concerning wellbeing. Implementing measurement and statistical inference without a firm commitment to underlying mechanisms is certainly defensible in the initial stages of a research programme. Sometimes goals of science and policy do not require knowledge of mechanisms at all (Reiss 2007). However, as we show here, in the case of WPP there are good reasons to invest more into mechanistic understanding (in terms of knowledge of causal pathways) of how SWB interacts with its covariates. Key headline findings in the SWB literature could potentially be explained by multiple underlying drivers operating through several causal pathways. Policy that fails to recognise this indeterminacy risks pulling the wrong lever, as it were, with unintended consequences.

We are not calling for better data, greater use of experimental methods, or a stronger focus on causal inference, though these would always be welcome; rather, our argument is that deeper theory would be more valuable and is indeed prior to these improvements in many ways. We outline five problems that are likely to bedevil WPP in the absence of deeper theory, regardless of whether data and empirical methods improve. These are: conceptual confusion; incorrect aggregation; lack of robustness and external validity; inept interventions; and the Lucas critique (whereby a policy intervention leads to behavioural or structural changes that alter the statistical relationships). While all are separately familiar issues elsewhere in social sciences, we show here that they all apply acutely to the case of wellbeing. We also explain why theory is required merely to interpret empirical data in wellbeing science, and how the field is therefore liable to make limited progress until richer theory is forthcoming. In closing, we highlight what sort of theory would be appropriate.

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<sup>1</sup> A brief note on terminology: economists use the word “structural” in the way we here use “mechanistic”. We instead use the sociological meaning of structural, where it refers to the institutional, socio-cultural, economic, and political configuration of society, while ‘mechanistic’ is a term encompassing both micro and macro factors that define a causal relationship.

Before proceeding, three qualifications. First, this paper does not challenge the technocratic style common to happiness economics and positive psychology (see Fritjers & Krekel 2022). This style assumes that a good policy is one designed by scientific experts on the basis of empirical data. It raises complex ethical (Fabian & Pykett 2022), political (Jupp et al. 2016), and public management objections (Fabian et al. 2022). There is an obvious need for more theory of politics and policymaking in much WPP advocacy (Bache & Reardon 2016). However, here we have a narrower scope and take the technocratic perspective as given. For even assuming that the technocratic policy paradigm is unproblematic, its agenda will not progress without more theory.

Second, we note that many of the problems we point to apply to wellbeing science in general and not just to WPP. However, we would argue that wellbeing science is acutely affected by what a recent special issue of *Perspectives on Psychology Science* referred to as a ‘theory crisis’ in psychology (see especially Eronen & Bringmann 2021). Theory-avoidance is not the sole nor even the main problem in the science of wellbeing (Alexandrova 2017). Other theoretical challenges to WPP include the prudential adequacy of SWB as a construct (Bishop 2015), difficulties in aggregating different aspects of wellbeing into single metrics (Cooper et al. 2023), the challenge of devising policies that would meet democratic norms (Alexandrova & Fabian 2021), as well as the various obstacles to validity of measurement scales (Fabian 2021; Marsh et al. 2021; Alexandrova & Haybron 2016). While our critique overlaps with these issues, it is distinct from them.

Thirdly, our call for mechanistic understanding does not imply an invitation to formulate rigid universal mechanisms of wellbeing outside history and culture. Those may not exist. Rather we advocate for more causal sensibility in wellbeing research, which is compatible both with evolving and local mechanisms, as well as with mechanisms that are more robust and persistent.

## 2. Examples of wellbeing policies as black boxes

To motivate the argument and illustrate the potential pitfalls in policy recommendations arising from theory-free WPP, rather than a systematic review, we begin with two prominent examples found in leading internationally peer reviewed journals.

Our first example of a WPP black box is widespread advocacy of progressive taxation to improve aggregate life satisfaction. To date, the causal underpinnings of the correlation between these specific variables have not been isolated. Frank (2008) emphasises status anxiety and positional competition, which is supposedly lessened by strongly progressive taxation. Oishi et al. (2018) provide evidence that progressive taxation is correlated with increases in generalised social trust, which is in turn correlated with life satisfaction. These are reasonable hypotheses with some statistical support. However, the statistical association between tax structure and life satisfaction could be driven by omitted variables that are correlated with status anxiety, positional competition, social trust, and progressive taxation. Notably, there is some evidence that they could be driven by social security spending, which tends to go hand in hand with progressive taxation (Pacek & Radcliff 2008). Insofar as periods of relatively progressive taxation are correlated with relatively progressive political parties being in power, the relationship could also be driven by other factors associated with the policy inclinations of such parties. Oishi et al. (ibid.) use US data from 1964–2014. Their result might thus be driven by efforts at racial justice, voter enfranchisement, women’s empowerment, and migration policy, among others. There are no attempts to control for such factors in the statistical modelling, let alone isolate the direct causal effect of a progressive tax structure. There is obvious potential for unintended consequences in increasing taxation if the actual causal mechanisms pertain to how tax revenue is *spent*. The period of the Reagan administration in the US, for example, is associated with

declines in life satisfaction despite increases in tax receipts during this period (Sowell 2012; OECD 2024). This could be because of a negative causal relationship from tax receipts to life satisfaction, or a positive one from tax progressivity to life satisfaction. Alternatively, it could have nothing to do with receipts or progressivity, and instead be determined by the allocation of government outlays across say, defense spending (which rose from 23.2% to 26.5% of Federal outlays under Reagan), human resources<sup>2</sup> (from 53.4% to 50.1%), or physical resources<sup>3</sup> (from 10.5% to 7.1%) (O'Connor 2017; OMB 2024).

Our second example is the analysis of WPP advocated by VanderWeele (2017) on the basis of existing empirical results. VanderWeele argues that the empirical evidence base is sufficiently clear and voluminous for the positive relationship between *education* and meaning in life, *marriage* and life satisfaction, and *religion* and a range of wellbeing variables, to justify policies targeting these variables. However, the meaning of these variables is unclear, as are the mechanisms connecting them with measured aspects of wellbeing.

Consider the link between education and wellbeing, and consider whether the basketballer LeBron James would have more meaning in his life if he had gone to college first rather than famously proceeding straight to the National Basketball League (NBA)? Intuitively, the answer is no. What is meaningful to LeBron James, by his own subjective statements, is winning championship rings in the NBA:

I have short goals – to get better every day, to help my teammates every day – but my only ultimate goal is to win an NBA championship. It's all that matters. I dream about it. I dream about it all the time, how it would look, how it would feel. It would be so amazing.

Getting a degree may well have undermined LeBron's meaning in life by slowing down his move to professional basketball. In contrast, deepening his skills in basketball is more likely to have helped him achieved the life he considers meaningful. This raises the question, 'what is education?' Is *practice* sufficient, or an apprenticeship, or does one need to go to a four-year college or university? Empirical results typically use formal academic qualifications as the measure of 'education'. This issue of where exactly the mechanism lies in the education–meaning relationship has implications for policy. If a college campus experience is critical, perhaps because of socialisation or networking, then government might subsidise university education for all, or lower admissions requirements. If the mechanism is instead practicing something for which you have an intrinsic motivation, then government subsidies for college would likely be an inefficient policy, at least in terms of promoting meaning in life.

Theory can help us to think through these questions and craft precise hypotheses for testing. Consider the three-factor model of meaning in life (King & Hicks 2020) developed by, among others, Steger et al. (2006, 2008). The three factors are:

- Purpose: the sense that you have a reason for being
- Significance: the sense that what you are doing has value
- Coherence: being able to make sense of world, especially normatively

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<sup>2</sup> Human resources incorporates: Education, training, employment, and social services; Health; Medicare; Income security; social security; and veterans benefits and services.

<sup>3</sup> Physical resources incorporates: Energy; Natural resources and environment; Commerce and housing credit; Transportation; and Community and regional development.

This model was developed on the basis of a substantive engagement with existing theory in existential philosophy, clinical psychology, and even theology.<sup>4</sup> Only then did the associated scholars move to psychometric scale validation.

How might education relate to these three factors? Education might open up purposeful careers that are unavailable to those without the requisite training. Courses in the natural and social sciences might help you make sense of the world, bringing coherence. And courses in the humanities might provide an ethical foundation for your life, giving your actions a sense of significance. We can use these theoretical insights to understand Lebron's situation. He gets meaning from his career, but this requires skills acquired outside formal education. In contrast, someone who wants to be a cardiologist would indeed have meaning opened up for them by getting a formal education. We could test these hypotheses at the level of specific mechanisms and appropriate metrics rather than relying on high level and ultimately ill-defined correlations for our policy inferences.

Similar concerns arise regarding the relationship between marriage and life satisfaction. If marriage is inherently good for wellbeing, then perhaps we should embrace the practice of the Moonies, a South Korean Christian cult, which marries strangers in large mass wedding ceremonies. This suggestion would strike most readers as intuitively false, but why? Presumably because the mechanism of marriage – the source of its benefits for life satisfaction – requires more than a formal ceremony. Notably, Chapman & Guven (2016) find that people in self-assessed poor marriages are much less satisfied with life than single people. Grover & Helliwell (2019) find that people who are 'friends' with their spouse are more likely to be satisfied with their marriages and with life. But if friendship is the causal mechanism, then why do we need marriage? The question again arises, what do we mean by marriage? This is a theoretical issue pertaining to conceptual clarity. Should we understand marriage as a social institution? In that case, the causal pathways between marriage and satisfaction are likely to be different in a culture like India compared to a culture like Denmark. If it is linked to having an intimate confidante, affection, sex, and childrearing, then the institution itself might matter very little. The answer has policy implications. VanderWeele recommends state-funded marriage counselling as a sensible policy based on his review of the empirical literature. Yet this might simply prolong the poor marriages identified by Chapman & Guven (2016). If companionship is the key mechanism, then we might instead focus on developing peoples' interpersonal skills.

When it comes to religion, VanderWeele (p. 8152) writes that 'people who regularly attend religious services have better health, are more charitable, have higher gratitude, trust, and forgiveness, less divorce and more marriage, and more social support and friends.' This multidimensional relationship with aspects of wellbeing likely reflects multiple mechanisms. For example, the altruism and social life of the religious may be a function of their membership in a community that meets regularly. This could be replicated without religion *per se*. What is specifically the role of 'religion' as opposed to community organisations like social soccer clubs? Is any religious creed sufficient, or do only particular religions drive wellbeing? The extant literature on religion and wellbeing suggests that such nuances are critical. For example, Hamblin & Gross (2014) note in their systematic review that while intrinsic religiosity is negatively correlated with depressive symptoms, extrinsic religiosity (i.e. practicing religion for instrumental reasons) had a positive correlation. They also note that religious attendance was associated with higher prevalence of generalised anxiety disorder symptoms among

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<sup>4</sup> VanderWeele himself has recently contributed to this literature, providing theoretical and empirical justification for more than three factors (Hanson & VanderWeele 2021)

homosexuals. Again, the answers have specific policy implications. VanderWeele recommends tax-exemption for religious groups to foster their contribution to wellbeing. Yet if some religions, like the Jedi or certain cults, have no such effect (and perhaps even predate on vulnerable people), then we would not want to subsidise them in this way.

It is difficult to establish an empirical relationship between two variables without first pinning down what those variables, such as education, marriage, and religion as discussed above, specifically mean. Philosophers of science have noted a general tendency towards imprecise conceptualisation in psychology, with a range of consequences for scientific practice. Betzler (2019), for example, analyses how ‘empathy’ is conceptualised in different ways by different research groups, undermining the comparability of studies, notably meta-analyses of empirical results, and muddling the field’s ability to meticulously test hypotheses. This trend is also observed in studies of non-psychological variables relevant to WPP. For example, Hamblin & Gross (2014, p. 78) remark that, ‘Perhaps the most significant obstacle in interpretation of [the relationship between religion and wellbeing] is the heterogeneity in operational definitions of religion and religiosity.’

Alexandrova (2017), Fabian (2022), and Cohen-Kaminitz (2018) point to similar challenges for SWB research arising from the operationalist (as opposed to realist) epistemology of the field. This is where concepts are defined in terms of their measures. Namely, the conventional definition of SWB as ‘experiences’ and ‘evaluations’ essentially defines the construct as whatever responses people give to the experience sampling and life satisfaction scale metrics used in the field. There is no reason to think that respondents understand these concepts the same way researchers do, nor consistently across respondents. Typically, cross-cultural validity of these measures is justified only by predictable statistical behaviour of these metrics (Diener et al. 2013). However, there are many reasons to doubt this. Indeed, there is now a wealth of international evidence that happiness, satisfaction, and other concepts central to SWB studies are interpreted differently across cultures. For example, Kryszewski et al. (2021) discuss how US respondents think of happiness as something to be maximised, whereas Japanese respondents think it is desirable to be ‘balanced’ in life – neither too happy, nor too sad. Indeed, Clark et al. (2015) find that the “desired condition” among their Japanese respondents is 8.3/10 on average. Failure to account for such conceptual variation will undermine empirical analysis and policy recommendations, regardless of how sophisticated the data and methods employed. Conceptual precision is fundamentally a matter of effective theorising.

All in all, the high-profile findings of wellbeing research on the roles of education, marriage, faith, or equality are all underdetermined. There are many plausible possibilities as to why they hold when they hold, but these remain unsubstantiated. We now turn to the reasons why such under-determination is a significant problem for policy.

### 3. Consequences of insufficient theory

#### 3.1 Robustness and external validity

A concern with wellbeing policies derived from theory-free empirical results is that these results are liable to be contingent on ‘structural’ factors that are not stable across policy cases.<sup>5</sup> By structural, we mean features of society, culture, policy, politics, and economics that constitute a ‘social context’. As that context changes, so too will the effects of the policies, undermining their robustness. For example, consider the result that life satisfaction depends substantially on the

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<sup>5</sup> Grüne-Yanoff (2016) makes this point in relation to policies derived from behavioural psychology, arguably the first large-scale application of psychological science to public policy.

income of one's reference group – the 'keeping up with the Joneses' phenomenon (Boyce et al. 2010). The severity of this effect is contingent on respondents' pride and envy. A range of positive psychology techniques like gratitude make it possible to reduce social comparisons (Heintzelmann et al. 2020), so the size of this comparison effect is endogenous to individual behaviour. Furthermore, individuals in cultures like contemporary America with institutions that celebrate wealth and social rank tend to be more sensitive to comparison effects (Kasser & Ahuvia 2002). Comparison effects are thus endogenous to cultural change. Therefore, the effect of policies designed with comparison effects in mind, such as taxation of positional goods (Frank 2008), depends on factors that are heterogenous across contexts and can easily shift. Theory would help to clarify what the central mechanisms are in a wellbeing policy and in what ways they are contingent on contextual factors. Such a structural model can then inform predictions about how a policy will travel to other contexts (Cartwright & Hardie 2012).

Closely related to robustness is external validity, which bedevils even experimental studies. Such studies provide strong *internal validity*, which means that they confidently estimate a causal relationship between two variables. However, in the absence of theory, that relationship can easily be a function of omitted variable bias. This will undermine *external validity* – the degree to which the estimated treatment effect will replicate in a different setting (Deaton & Cartwright 2018). For example, Micari & Pazos (2014) evaluated an intervention that seems able to reduce social comparisons among university students, especially in terms of intelligence and academic ability. To what extent might this result generalise? If it is contingent on the extent of social comparisons specific to intelligence and academic ability, then it will likely perform more strongly in contexts where such comparisons are prevalent, such as China or South Korea. If it is effective at reducing social comparisons *in general*, then perhaps it could be applied to dampen the keeping up with the Joneses phenomenon. Theory can help us think through these issues and learn more from experiments that merely whether one specific intervention worked in a narrow context.

### 3.2 Naïve interventions

While experiments can achieve causal identification, they can be naïve about the complex pathways between two causally related variables. This can lead to inept policymaking if the causal relationship identified between  $x$  and  $y$  is actually driven by  $z$ , which is correlated with  $x$ . Care about mediated pathways is essential for correct statistical inference, and requires sufficient theory.

This can be illustrated with reference to the literature on 'social prescribing', which involves linking patients in primary care or clients in social services to sources of support within their community. There is no systematic evidence of the efficacy of such programs (Bickerdike et al. 2016), although there are some long-established, well-regarded local schemes. The advocacy of social prescribing seems to rest on atheoretical assumptions about a consistent positive link between the various kinds of interventions that come under the social prescribing rubric and wellbeing outcomes. One theoretical explanation for this observed link, from social psychology, is that group membership is in itself a source of wellbeing (Stevenson et al. 2017). In this case, all social prescriptions will be beneficial. Yet the mechanisms may also be deeper and more specific in the case of many interventions. For example, in the case of socially prescribed exercise, the mechanisms may be biological rather than social – exercise may lower cortisol levels and thereby reduce stress (Steptoe et al. 2015). Then again, the social element may be a crucial commitment device in this intervention. Other theories might justify social prescription with different

underlying drivers of wellbeing change, such as self-expression through the arts or the ‘eco-therapy’ of being in green space.

Again, what is needed is a *theory* that isolates different potential drivers of wellbeing so that precise hypotheses can be tested regarding why a particular policy has an impact. Higher quality evaluations like randomised control trials do not resolve this issue if they simply test the causal effect of ‘social prescribing’ rather than specific competing mechanisms within social prescribing that theory would identify.

### 3.3 *The Lucas critique*

Wellbeing policies, like any other type of policy including behavioural interventions, may be vulnerable to changes in behaviour or structure *in response to the policy intervention*, that subsequently break the previously observed relationship. In other words, the effect of a policy may be endogenous to that policy itself. The statistical relationship upon which the policy is premised would in this case not be *persistent* and is thus not, on its own, compelling grounds for a policy. This is a well-known issue in economic policy advice (Coyle 2021), often referred to as the Lucas critique (Lucas 1976).

An example of the Lucas critique from the wellbeing context is policies to improve happiness at work. Bellet et al. (2023) use data on the sales and other workplace behaviour of call centre employees working for a telecoms company, combined with weather data and data about the number of workers who would be exposed to that weather via windows to establish a causal relationship between good mood and productivity (there is an established empirical relationship between pleasant weather and good mood). This is exactly the sort of high-quality causal analysis that WPP needs. Yet there it is an open question whether mood effects *driven by the weather* would be identical to mood effects driven by other interventions, notably managerial policies. If, as observed by Cook (2021), workers perceived workplace happiness policies as a cynical ploy to extract more labour value from them, they may deliberately slack off in response, reversing the expected relationship between happiness interventions and productivity. This would be the Lucas critique in action. Another real-life example is that mandated positive attitude training for the long-term unemployed further demoralised them and deepened their cynicism towards the system (Friedli & Stearn 2015). Theory is again helpful for thinking through what the causal mechanism is in a particular policy intervention, and what confounders might exist across varying contexts.

### 3.4 *Incorrect aggregation*

A final complicating factor for theory-agnostic WPP is that different empirical relationships between SWB and its covariates emerge at different scales of analysis, such as individual, local area, or national. This is essentially due to averaging when the effect of an independent variable, such as public transport infrastructure, is heterogeneous and therefore not constant across subgroups of a population. With heterogeneous effects, the weights given to different subgroups matter for the magnitude (and possibly even for the direction) of the average effect. Without a suitable theoretical underpinning, we cannot specify the correct process of aggregation.

Felici and Agarwala (2022) illustrate how this is relevant for subjective wellbeing policy. They document how the relationship between having a degree and reported life satisfaction is heterogeneous across the distribution of life satisfaction, with a stronger, positive relationship at low levels of life satisfaction and a weaker one at higher levels of life satisfaction, even turning negative at very high levels. This variation reflects the hypothesis that education plays a different role in the lives of individuals in different circumstances, specifically that it may act as a buffer



against shocks to SWB that fall more frequently on those in low incomes. Depending on the composition of the population in terms of different types of individuals, the aggregate average relationship of education and SWB will differ, possibly so much as to switch sign. This compositional effect could explain the different estimates of the effect of education on SWB at different geographic scales, as observed in Florida et al. (2013). They find that education is one of the strongest predictors of wellbeing at the level of cities, while in the literature it is generally found to be a weak predictor at the individual level.

#### 4. Measurement needs theory

That measurement is a form of theory testing and that it can only improve together with improvement in theory is common knowledge in philosophy of science (Tal 2020, section 8.2). Specific to our case, there are two reasons why theory is critical to WPP, regardless of whether wellbeing data becomes more widely available or experimental methods more widely utilised. The first is that we do not know what data to collect without theory. For example, there is growing agreement among more theoretically inclined psychologists studying subjective wellbeing that basic psychological needs for autonomy, competence, and relatedness are fundamental to wellbeing, subjective or otherwise (Martela & Sheldon 2019; Fabian 2022; Marsh et al. 2020). Yet these items are not measured in any general social survey except the European social survey in 2012. Meanwhile, even machine learning methods are unable to explain more than 30% of the variance in life satisfaction scores using existing social survey questions (Oparina et al. 2022). Calls for 'more data' that merely result in the proliferation of life satisfaction and affect measures will fail to deepen our understanding of wellbeing if we are not using theory to expand what data we are collecting.

The second reason why theory is critical to WPP is that the most widely used wellbeing metric, namely life satisfaction scales, produces data that cannot be effectively interpreted without theory. Scale data reflects a mapping from respondents' latent life satisfaction to a response category on the survey instrument. To date, we have almost no understanding of the 'reporting function' (Oswald 2008) that prosecutes this mapping. Fabian (2021) provides a theoretical overview and summary of the existing empirical evidence for the possibility that differences in the reporting function across responses or over time can bias statistical analysis, and Kaiser (2021) demonstrates empirically that these biases can be large. Note that appeals to the psychometric validity of life satisfaction scales do not address this issue, which is about the validity *for what* of life satisfaction data. Life satisfaction scales may be psychometrically valid for high resolution epidemiological research, but nonetheless be invalid for economic cost-benefit analysis (see Fabian 2021 for a longer discussion). It is also worth noting that these issues affect many metrics in wide use in social science, like levels of educational attainment and democracy indexes, and addressing them is thus not a burden that life satisfaction research should bear alone (Chen et al. 2022).

Theory can help us to consider whether peculiarities of reporting style might be especially pernicious in some policy domains. For example, Fabian (2021) provides evidence from Rasch analysis that ceiling effects may be widespread in life satisfaction reporting in advanced nations. This is where individuals reporting near the top of their scales change the meaning of the points on their scale as life continues to improve over time, rather than changing the response category they choose. This points to the need for caution in comparing the effect size of interventions where groups have high or low initial levels of life satisfaction, as there is more 'room' for improvement in the responses of the low baseline individuals. Another case is comparing across groups in radically different contexts. Kapteyn et al. (2013) found that adjusting for scale

consistency using vignettes eliminated the difference in income satisfaction between Dutch and American respondents observed in raw data, and Angelini et al. (2013) show similar results across EU countries. Comparing the same individuals before and after major shocks is a third case where caution is required. The response shift literature in medicine provides decades of empirical evidence showing that people change the way they understand their health, and their satisfaction with it, following injuries, illnesses, and surgeries (Schwartz 2016; Vanier et al 2021). Finally, psychologists have long noted cognitive biases of ‘effort justification’ and ‘implicit theories of change’ when people make subjective assessments of the impact of training programs on their skills (Ross 1989). These cognitive biases can lead people to report larger effects from these programs than actually occurred. We should thus be cautious about the impact of happiness interventions where we regard effort justification as likely. The four cases above cover comparisons across groups, over time, and in response to treatments – precisely the sorts of things studied in wellbeing research. In that case, we need theory on hand to ensure that raw data does not lead us astray. It is not a virtue to ‘let the data do the talking’.

### 5. What sort of theory do we need?

While this is not the place to specify a theory of SWB, our analysis points to some characteristics of a theory suitable for policy. First, it should be conceptually clear about definitions of wellbeing and any conjectured causes and correlates. Some contemporary trends in social sciences, such as short word limits and a tendency to jump to a discussion of statistical modelling and results without first clarifying terms, specifying hypotheses, or other conceptual framework, are not helpful in this regard. Second, it should explain how measures of wellbeing work. For example, what goes on psychologically when people make life satisfaction judgements, what do these judgements depend on, and how are these mapped into responses on life satisfaction scales? Third, it should describe mechanistic, causal, relationships between wellbeing and its covariates. For example, does employment directly improve life satisfaction, or is the relationship rather a function of things contingent on a job, like a social life at work, societal approval, goal achievement, basic psychological needs for competence, or identity? Causal relationships at a high level of abstraction, such as the oft-cited relationship between life satisfaction and ‘social cohesion’, can be useful, but more high-resolution identification of mechanisms would be less prone to unintended consequences. Fourth, a mechanistic understanding of the deep determinants of SWB would illuminate how sensitive these deep mechanisms are to shifting structural factors. It would reveal what drivers of SWB are germane to particular contexts and scales of analysis, and which are in some sense universal.

There are encouraging efforts towards this sort of theory building in wellbeing studies (Fabian 2022). For example, Martela and Sheldon (2019) recently developed a model linking SWB, motivation, and basic psychological needs for autonomy, competence, and relatedness. DeYoung & Tiberius (2023) have developed links between values, personality, and SWB. Kryszewski et al. (2021) are deepening our understanding of cultural psychology and life satisfaction reporting styles. Future research in this vein could be prosecuted as part of the current turn towards cross-cultural analysis, explicitly embracing the role of contextual factors.

It is also valuable to underline what sort of ‘theory’ would *not* address the issues we have raised. First, wellbeing scholars should avoid the general tendency in psychology, identified by Gigerenzer (2010), of rephrasing empirical regularities as theory. This is circular reasoning. Theorising is not a matter of correlation-mongering, where statistical relationships are assembled on a largely ad hoc basis into a ‘theory’. Both the rephrasing of empirical results as theory and correlation mongering produce theory that is acutely sensitive to structural breaks that alter

empirical regularities without illuminating what structural changes to watch out for. Even more concerningly, they do not increase policymakers' confidence that they have identified the correct lever to pull to improve wellbeing.

Nor do we call for more traditional philosophy of wellbeing, which has typically searched for a set of universal necessary and sufficient conditions for wellbeing. That literature has yielded a useful conceptual library of definitions of wellbeing and their relation to happiness and quality of life, but it has proceeded at too abstract a level to be helpful for WPP (Haybron 2008; Alexandrova 2017; Tiberius 2023). What WPP needs is a wide array of theoretical efforts to *fill out* the knowledge missing for purposes of application of existing empirical results.

## 6. When can policy proceed even without theory?

This paper is not a call for perfection; we often have to act and should act in the absence of mechanistic knowledge (Reiss 2007, 2013). To underline this, we close with a discussion of where theory-light empirical observations can potentially justify public policy.

The first and most obvious case is where mechanisms are already quite well understood. In some examples, causal evidence confirming that knowledge appears to generalise to a new context, providing substantial justification for policy action. For example, the ENHANCE program (Heintzelmann et al. 2020) and Healthy Minds curriculum (Lordan & McGuire 2018), which teach techniques such as gratitude for improving subjective wellbeing, have both been independently experimentally evaluated with encouraging results and their mechanisms, such as reference point effects for gratitude, are relatively well understood. Occasional systematic reviews, such as Folk & Dunn (2023) are useful in this regard.

A second case is where urgency or the cost of inaction are so severe as to justify policymaking on the basis of evidence of the existence of a causal relationship even in the absence of a mechanistic understanding. An instructive example is psychopathology. The biological, psychological, and social mechanisms of mental illness are not well understood, but its debilitating impacts are clear. And while the effect sizes of various interventions, notably widely-used pharmaceutical and behavioural interventions like mindfulness, are arguably underwhelming, their causal efficacy is experimentally established (Garrote-Caparrós et al. 2022). It is reasonable in these circumstances to make such therapies more widely available through policy, especially considering the strong cost-benefit case (Clark et al. 2009). That said, we note that arguments similar to ours about the need for *theoretical adequacy* and *mechanistic understanding* are also common in the area of mental health (Ratnayake 2022; Murphy et al. 2021).

A third case is where the counterfactual policy option, often the status quo, is worse than action. Admittedly, this is common in WPP, whose advocates are often contrasting their proposals against a narrow economic paradigm that has arguably underpinned unsustainable growth models and an explosion in deaths of despair (Case & Deaton 2020), among many other concerns. It is nevertheless still instructive to consider the role of theory, mechanisms, and causal evidence here, because the economic policies being challenged, such as the emphasis on efficiency over equity, are often themselves justified on the basis of incomplete theory and descriptive rather than causal evidence. Indeed, the high tide of economic influence over policymaking was the 1980s, prior to the so-called 'empirical turn' in economics in the 1990s. The risk in WPP in such cases is principally one of scientific hubris or overreach. We should not present policy proposals as 'evidence-based' when they are not. We should instead be frank about the limits of our knowledge and recognise that such policy discussions are substantially political rather than scientific.

## 7. Conclusion

We have argued that wellbeing data and the empirical regularities recorded in the literature can justify different and even conflicting policies depending on unexplicated mechanisms. There is a strong case for considering wellbeing as an appropriate aim for policy outcomes, not least the reductive limits of referring to economic growth alone. The policy interest in a wider range of success metrics is welcome. However, it would be equally reductive to determine policies with reference to wellbeing metrics alone. WPP needs more theory, including what exactly wellbeing is, how its components interact, how they interact with social and economic conditions, and how policy interventions affect them.

This is not a demand for perfect knowledge, but the evidence provided in the wellbeing literature is rarely sufficient for strong claims about policy choices. Furthermore, causal inference is impossible without bringing to bear some theory-based structure from outside the set of highly interrelated, serially correlated observational wellbeing variables considered in the empirical literature (Cunningham 2021; Pearl 2019). Policy recommendations made on the basis of relationships identified in the wellbeing literature often have an implicit theory embedded in their assumptions (including normative ones) about the link between changes in levers and measured SWB outcomes. Wellbeing theories, which may differ or conflict with each other, should be explicit. Our argument that SWB *theory* is not ready for policy complements recent analyses arguing that SWB *measurement* is not ready for policy (Benjamin et al. 2020), and that more ethical analysis needs to be done to bridge wellbeing science into WPP (Fabian & Pykett 2021). Recognising the need to move WPP beyond correlational empirics opens up a rich research agenda to pursue.

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The authors have no competing interests to declare.

### Author contributions statement

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This study involved no subjects whatsoever. All standards of good research conduct were observed in the production of this research paper.

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