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Examining the indirect effect of income on well-being via individual-based relative deprivation: Longitudinal mediation with a random intercept cross-lagged panel model

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Although the positive relationship between income and well-being is well established, the psychological mechanisms underlying this process are less understood. One underexplored explanation is that objective wealth (or lack thereof) fosters *relative* comparisons, which, in turn, predicts well-being. Extant work has, however, mostly focused on objective indicators of relative deprivation rather than on how people *perceive* their societal status. We address this oversight by examining the longitudinal indirect effects of income on well-being via *perceived* individual-based relative deprivation (IRD) using traditional and random intercept cross-lagged panel models. Averaged across 10 annual assessments in a nationwide longitudinal panel sample of adults ($N = 66{,}560$), our results revealed reliable indirect effects of income on well-being via IRD. Specifically, within-person increases in income predicted within-person decreases in IRD, which then predicted within-person increases in personal well-being over time. Our results replicated across robustness checks, including one using a general life satisfaction measure. We thus extend previous work by highlighting the need to consider one's *perceptions* of their relative societal position as a mechanism underlying the effects of income on well-being over time.

Keywords: Income; Longitudinal analysis; Relative deprivation; Well-being; Cross-lagged panel model.

In 2021, the world's wealthiest 10% earned 52%, while the poorest half earned just 8.5%, of the year's global income (Chancel et al., 2021). These statistics highlight a growing phenomenon over the past 40 years with dire implications for health and well-being (Pickett & Wilkinson, 2015). Indeed, income correlates positively with happiness (Buttrick & Oishi, 2017), self-esteem (Osborne et al., 2015; Twenge & Campbell, 2002) and well-being (Diener et al., 2010). Conversely, economic hardship correlates positively with *adverse* life outcomes (Pickett & Wilkinson, 2015). These associations are, however, small and inconsistent (Ngamaba et al., 2018). Moreover, the psychological mechanisms underlying these processes are often overlooked. Thus, we know little of *why* the deprivation of material goods undermines people's well-being.

One comparatively underexplored reason why inequality impacts well-being is that inequities increase the salience of the differences between those who are materially well-off and those who are materially deprived (i.e., the "haves" and the "have-nots," respectively). In turn, people's relative comparisons may negatively impact well-being (Osborne et al., 2015; Schneider, 2019). Indeed, feelings of relative deprivation—stemming from *subjective* feelings of disadvantage—predict myriad individual and social outcomes above and beyond one's objective position in society (Smith et al., 2012). Notably, individuals can either feel deprived relative to

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other individuals (individual-based relative deprivation [IRD]) or that their group is deprived relative to other groups (group-based relative deprivation [GRD]), provided they appraise themselves or their group as *unfairly* disadvantaged (Runciman, 1966). While feelings of GRD (relative to IRD) more strongly predict *group*-based outcomes like collective action (Abrams & Grant, 2012), IRD (relative to GRD) more strongly predicts *individual*-level outcomes including lower self-esteem (Walker, 1999). Thus, how one perceives their income *relative to* other individuals may help explain how income affects well-being (Buttrick & Oishi, 2017).

Considerable work illustrates the impact of objective markers of relative income on well-being (for a review, see Clark & D'Ambrosio, 2015). For example, cross-sectional research demonstrates that the negative relationship between relative income and life satisfaction is stronger in nations with higher levels of income inequality (i.e., countries with more salient differences between the "haves" and the "have-nots"; see Cheung & Lucas, 2016). Experimental (Card et al., 2012) and panel (Luttmer, 2005) data further show that one's income *relative to* others dramatically impacts well-being.

Though informative, the extant literature has predominantly focused on aggregate measures of income inequality or material comparisons of individuals' income to others in the local community (see Smith et al., 2012, for discussion). However, relative deprivation theory posits that individuals must perceive themselves as *unfairly* disadvantaged to feel relatively deprived. Accordingly, studies that measure one's perceptions of—and affective responses to—their societal position reveal *stronger* associations with well-being than do studies that just assess objective conditions (Smith et al., 2012). Thus, examining people's *perceptions* of their position relative to similar others is needed to understand the psychological mechanisms underlying income's effects on well-being (Osborne et al., 2015).

Overview of the present study

The present study examines income's longitudinal associations with *perceptions* of IRD and well-being across 10 annual waves of a nationwide random sample of adults. To assess well-being, we utilise the personal well-being index, a measure comprised of multiple quality-of-life domains sensitive to the environment and, thus, changes in circumstances (Cummins et al., 2003). Importantly, the index is a cross-culturally valid, global measure of subjective well-being (Lau et al., 2005). Nonetheless, given the focus on *general* life satisfaction in previous research (Clark & D'Ambrosio, 2015), we also assess a broader measure of life satisfaction as a robustness check. Overall, we aim to provide a more nuanced understanding of how material conditions (or lack thereof) can foster relative comparisons that impact well-being over time.

Critically, we conduct longitudinal mediation analyses using random intercept cross-lagged panel modelling (RI-CLPM; Hamaker et al., 2015) to examine whether within-person *changes* in income predict changes in IRD and whether, in turn, this predicts changes in well-being over time. Because an RI-CLPM separates between-person stability from within-person *change* (Hamaker et al., 2015; Osborne & Little, in press), we can speak specifically to how material goods foster relative comparisons that decrease well-being *within* individuals. We thus overcome the limitations of traditional cross-lagged panel models (CLPMs), which, while intuitive for modelling longitudinal processes (see Zyphur et al., 2020), conflate between-person stability (i.e., stable differences between people) and within-person change (i.e., temporary deviations from one's "typical" mean level of a variable). To illustrate the unique features of the RI-CLPM, we also compare and contrast our results with a traditional CLPM.

Hypotheses

Given the known associations between income and well-being (e.g., Pickett & Wilkinson, 2015), income should correlate positively with well-being at both the between- and within-levels of analysis (Hypotheses 1a and 1b, respectively). However, longitudinally, income should indirectly affect well-being via IRD at the within-person level; individuals who experience temporary increases in income should experience *decreases* in IRD in the following year, which should, in turn, precede increases in well-being (Hypothesis 2). By examining these hypotheses, we aim to elucidate one potential mechanism for *how* material goods (or a lack thereof) impact well-being over time.

METHOD

Participants and procedure

We use data from Time 3 (2011) to Time 12 (2020) of the New Zealand Attitudes and Values Study (NZAVS), as we first assessed IRD at Time 3. The NZAVS is a nationwide longitudinal panel study based on a random sample of New Zealand adults. Participants were initially randomly sampled from the electoral roll, which is a compulsory list of registered voters in New Zealand ($N_{\text{Time1}} = 6518$, response rate: 16.6%). Subsequent booster samples were collected at Time 3 (2011), Time 4 (2012), Time 5 (2013), Time 8 (2016) and Time 10 (2018) to address sample attrition and diversify the sample. By Time 12 (2020), the sample size was 38,551. Sibley (2023) provides further information about the sample, procedure and retention of participants (see also https://osf.io/75snb/).

TABLE 1
Sample demographic characteristics across 10 annual waves

	T3	*T4*	*T5*	*T6*	*T7*	*T8*	*T9*	*T10*	*T11*	*T12*
Sample size	6884	12,176	18,259	15,820	13,941	21,935	17,071	47,942	42,677	38,549
Age (*SD*)	50.95 (15.96)	49.60 (15.03)	48.14 (14.07)	49.83 (14.03)	51.31 (13.90)	50.12 (13.93)	51.84 (13.77)	49.10 (13.86)	52.05 (13.87)	53.45 (13.69)
Gender										
Women	62.3%	62.4%	62.6%	63.1%	62.5%	62.5%	63.2%	62.6%	63.8%	63.7%
Men	37.4%	37.2%	37.1%	36.6%	37.2%	37.2%	36.5%	37.2%	35.7%	35.8%
Gender-diverse	.3%	.3%	.3%	.3%	.4%	.3%	.3%	.2%	.5%	.5%
Ethnicity										
NZ European	80.8%	73.8%	79.3%	80.5%	81.5%	81.7%	82.3%	82.8%	83.7%	85.4%
Māori	12.6%	17.1%	13.4%	12.6%	12.3%	11.6%	11.9%	10.1%	10.3%	8.9%
Pasifika	2.5%	4.3%	3.0%	2.8%	2.6%	2.3%	1.9%	1.9%	2.0%	1.9%
Asian	4.0%	4.8%	4.4%	4.1%	3.6%	4.3%	3.9%	5.2%	4.1%	3.8%
Born in NZ	78.8%	79.2%	79.5%	79.7%	80.1%	79.4%	79.7%	78.4%	78.3%	78.6%
Income[a] (*SD*)	.91 (.74)	.96 (.81)	1.02 (.82)	1.05 (.88)	1.07 (.87)	1.09 (.92)	1.14 (.94)	1.15 (.92)	1.18 (1.08)	1.21 (1.05)
Employed	71.7%	72.1%	76.8%	77.2%	76.4%	77.8%	77.0%	79.5%	75.6%	76.6%
IRD[b] (*SD*)	3.46 (1.49)	3.63 (1.54)	3.47 (1.57)	3.46 (1.50)	3.40 (1.52)	3.46 (1.54)	3.38 (1.54)	3.47 (1.57)	3.36 (1.54)	3.28 (1.53)
Personal well-being[c] (*SD*)	6.78 (1.74)	6.79 (1.77)	6.97 (1.73)	6.98 (1.73)	7.06 (1.71)	7.07 (1.71)	7.11 (1.67)	7.08 (1.66)	7.00 (1.69)	7.12 (1.67)
Life satisfaction[b] (*SD*)	5.10 (1.22)	5.08 (1.23)	5.20 (1.21)	5.24 (1.18)	5.23 (1.18)	5.22 (1.20)	5.26 (1.21)	5.31 (1.21)	5.30 (1.21)	5.22 (1.25)

[a] Annual household income (before tax) divided by $100,000. [b] Measured on a 1 (*strongly disagree*) to 7 (*strongly agree*) scale. [c] Measured on a 0 (*completely dissatisfied*) to 10 (*completely satisfied*) scale.

We used full information maximum likelihood (FIML) estimation to utilise all available data in our sample. As such, we included participants who provided partial ($N = 64{,}130$) or complete ($N = 2430$) responses to our variables of interest at one or more of the 10 annual assessments (i.e., Times 3 to 12; $N_{\text{total}} = 66{,}560$). Most participants were women (62.6%; gender-diverse: .5%, men: 36.9%) and born in New Zealand (77.7%). Concerning ethnicity, participants identified as New Zealand European (77.6%), Māori (12.7%), Asian (5.3%) or Pasifika (2.7%; 1.6% of the sample either did not report their ethnicity or identified as another ethnic group). Table 1 displays the demographic breakdown at each assessment occasion.

Measures

Income was assessed using the following open-ended item: "Please estimate your *total household income* (before tax) for the year." Due to the large variance in responses, we divided income by $100,000.00 NZD.

IRD was assessed using two items adapted from Abrams and Grant (2012): (a) "I'm frustrated by what I earn relative to other people in New Zealand"; and (b) "I generally earn less than other people in New Zealand." These items were measured on a 1 (*strongly disagree*) to 7 (*strongly agree*) scale and averaged at each time point ($\alpha s = .58–.60$; $rs = .40–.43$, $ps < .001$).

Personal well-being was measured at each time point using four items from Cummins et al.'s (2003) Personal Well-being Index. Participants were asked to rate their satisfaction with their: (a) standard of living, (b) health, (c) future security and (d) personal relationships on a 0 (*completely dissatisfied*) to 10 (*completely satisfied*) scale. These items were averaged at each wave ($\alpha s = .72–.76$).

Life satisfaction was measured at each time point using two items from Diener et al. (1985): (a) "I am satisfied with my life" and (b) "In most ways my life is close to ideal." These items were measured on a 1 (*strongly disagree*) to 7 (*strongly agree*) scale and averaged at each time point ($\alpha s = .77–.82$).

RESULTS

Analytic method

The current study investigated income's longitudinal associations with IRD and well-being. Specifically, we conducted a traditional CLPM and an RI-CLPM in *Mplus* version 8.8 using FIML estimation. We compare these approaches to demonstrate the differences between the two models and the critical need to use an RI-CLPM when modelling within-person processes. Indeed, although traditional CLPMs provide an intuitive approach to modelling change over time and are widely used throughout the medical and social sciences (see Lucas, 2023, for discussion), they confound stable, between-person differences with within-person change (i.e., changes from an individual's "typical" levels of a construct; see Hamaker et al., 2015; Osborne & Little, in press; Zyphur et al., 2020). Thus, while CLPMs can help study between-person differences (though see Lucas, 2023), they are ill-equipped to examine intraindividual processes (Hamaker et al., 2015).

Given our focus on intraindividual change, we utilise an RI-CLPM (Hamaker et al., 2015) to examine the within-person associations between our focal variables over time. While conceptually similar to a traditional CLPM, the RI-CLPM isolates between-person stability from within-person change by estimating correlated random intercepts (Hamaker et al., 2015). Autoregressive and cross-lagged estimates thus reflect within-person change. Specifically, the autoregressive coefficients reflect the extent to which within-person *deviations* in a construct persist across sequential assessments. Conversely, the cross-lagged coefficients reflect the extent to which within-person deviations in a construct predict within-person deviations in *another* construct at the following assessment. Critically, adjusting for between-person stability can *eliminate* the cross-lagged effects observed in traditional CLPMs (Hamaker et al., 2015; Osborne & Sibley, 2020). Coupled with its computational efficiency compared to alternative methods of modelling within-person change (Lucas, 2023; Orth et al., 2021), the RI-CLPM is ideally suited to examine within-person associations between constructs over time.

Model estimation

We first estimated a traditional CLPM in which the mean scores of income, IRD and well-being were used to predict the same measures the following year across all 10 annual assessments. We modelled these associations as a stationary process by constraining congeneric paths to equality (e.g., the cross-lagged effect of income at Time 3 on IRD at Time 4 was constrained to be equal to the cross-lagged effect of income at Time 4 on IRD at Time 5). Finally, we examined the *indirect* longitudinal effects of income on personal well-being and life satisfaction via IRD by inspecting the indirect paths (i.e., the A × B pathways) estimated in *Mplus*.

Next, we estimated an RI-CLPM to examine the between- and within-person associations between our focal variables by following Hamaker et al.'s (2015) methodology, with two key exceptions. Specifically, we utilised a phantom/rescaling approach to standardise the scale of our random intercepts and within-person latent variables (Osborne & Little, in press). This approach is explained in detail elsewhere (see Lilly et al., 2023), but we provide a brief overview below. To estimate each random intercept, we freely estimated, but constrained to equality, the factor loadings (Hamaker and colleagues constrain the random intercept factor loadings to 1). This approach constrains the means and variances of the random intercepts to 0 and 1, respectively, thereby placing them on a common metric. Given that (a) income is measured in New Zealand Dollars, and (b) IRD and personal well-being are measured on 7-point and 10-point scales, respectively, this approach facilitates the interpretation of the effect sizes in our analyses but does not alter model fit (relative to Hamaker and colleagues' approach; Osborne & Little, in press).

To estimate the within-person latent variables at each assessment occasion for each construct, we freely estimated the regression of each first-order construct (i.e., the participants' observed scores) but constrained their (residual) variances to 1. Again, this approach places the within-person latent variables on a common metric (Osborne & Little, in press). We then estimated the autoregressive and cross-lagged effects across all 10 annual assessments. We modelled these estimates as a stationary process, as there was no theoretical reason to expect different effects at each wave (Orth et al., 2021). Finally, we inspected the A × B pathways estimated in *Mplus* to assess the *within-person* indirect effects of income on well-being via IRD.

Personal well-being

Model 1

Table 2 presents the results of our CLPM examining income, IRD and personal well-being ($\chi^2_{(396)} = 52{,}706.43$, $p < .001$; CFI = .89, RMSEA = .05 [.04, .05], $p > .999$, SRMR = .18). Turning first to the autoregressive paths, income ($b = .81$, 95% confidence interval [CI] [.812, .816], $p < .001$), IRD ($b = .56$, 95% CI [.560, .568], $p < .001$) and personal well-being ($b = .75$, 95% CI [.747, .753], $p < .001$) were all stable over time. Regarding the cross-lagged paths, income predicted decreases in IRD over time ($b = -.09$, 95% CI [−.088, −.082], $p < .001$). IRD also predicted decreases in income over time ($b = -.07$, 95% CI [−.070, −.062], $p < .001$), though the effects of income on IRD were greater than that of IRD on income over time ($\text{Wald}_{(1)} = 51.77$, $p < .001$).

Income also predicted *increases* in personal well-being over time ($b = .03$, 95% CI [.024, .030], $p < .001$), although personal well-being predicted comparable increases in income over time ($b = .03$, 95% CI [.023, .030], $p < .001$; $\text{Wald}_{(1)} = .00$, $p = .963$). Finally, IRD predicted decreases in personal well-being over time ($b = -.09$, 95% CI [.089, .081], $p < .001$). Personal well-being also predicted decreases in IRD over time ($b = -.12$, 95% CI [−.119, .113], $p < .001$), and this effect was stronger than that of IRD on personal well-being ($\text{Wald}_{(1)} = 126.77$, $p < .001$).

Mediation analyses. We examined the indirect effects of income on personal well-being via IRD by inspecting the A × B pathways estimated in *Mplus*. Table 3 shows that income had a positive indirect effect on personal well-being via IRD ($b_{\text{indirect}} = .01$, 95% CI [.007,

TABLE 2
Path coefficients for models assessing the longitudinal associations income and IRD have with personal well-being

		Model 1 Traditional CLPM: Personal well-being			*Model 2 RI-CLPM: Personal well-being*		
			95% CI			*95% CI*	
Outcome$_T$	*Predictor$_{T-1}$*	*b*	*Lower*	*Upper*	*b*	*Lower*	*Upper*
Income	Income	.81***	.812	.816	.50***	.494	.510
	IRD	−.07***	−.070	−.062	−.04***	−.046	−.033
	PWB	.03***	.023	.030	.02***	.015	.028
IRD	Income	−.09***	−.088	−.082	−.05***	−.061	−.048
	IRD	.56***	.560	.568	.17***	.160	.175
	PWB	−.12***	−.119	−.113	−.05***	−.054	−.031
PWB	Income	.03***	.024	.030	.02***	.015	.029
	IRD	−.09***	−.089	−.081	−.04***	−.044	−.031
	PWB	.75***	.747	.753	.22***	.209	.224

***$p < .001$.

TABLE 3
Indirect effects of income on personal well-being (PWB) via individual-based relative deprivation (IRD)

Model	*Pathway*	*Indirect b*	*SE*	*95% CI*
Model 1 (CLPM)	$\text{Inc}_{T-1} \rightarrow \text{PWB}_T \rightarrow \text{PWB}_{T+1}$	.020***	.001	[.018, .022]
	$\text{Inc}_{T-1} \rightarrow \text{Inc}_T \rightarrow \text{PWB}_{T+1}$	.022***	.001	[.020, .024]
	$\mathbf{Inc_{T-1} \rightarrow IRD_T \rightarrow PWB_{T+1}}$	**.007***	**.001**	**[.007, .008]**
Model 2 (RI-CLPM)	$\text{Inc}_{T-1} \rightarrow \text{PWB}_T \rightarrow \text{PWB}_{T+1}$	.005***	.001	[.003, .006]
	$\text{Inc}_{T-1} \rightarrow \text{Inc}_T \rightarrow \text{PWB}_{T+1}$	.011***	.002	[.008, .014]
	$\mathbf{Inc_{T-1} \rightarrow IRD_T \rightarrow PWB_{T+1}}$	**.002***	**.001**	**[.002, .002]**

Note: Hypothesised mediation pathway highlighted in bold. ***$p < .001$.

.008], $p < .001$). Thus, income predicted *decreases* in IRD which, in turn, predicted *increases* in personal well-being over time. This indirect association was small, and stronger indirect effects emerged via the autoregressive effects of income and personal well-being. Thus, IRD only *partially* mediated the relationship between income and personal well-being.

Model 2

Next, we estimated an RI-CLPM to directly examine the *within*-person associations between our focal variables over time ($\chi^2_{(390)} = 6643.99$, $p < .001$; CFI = .99, RMSEA = .02 [.015, .016], $p > .999$, SRMR = .030; $p < .001$). Notably, the RI-CLPM provided a better fit to our data than did the CLPM ($\Delta\chi^2_{(6)} = 46{,}062.44$, $p < .001$). Consistent with Hypothesis 1a, the between-person associations displayed in Figure 1 reveals that those relatively high in income across all 10 annual assessments also tended to be relatively low in IRD ($b = -.49$, 95% CI [−.499, −.481], $p < .001$) and relatively high in personal well-being ($b = .33$, 95% CI [.316, .335], $p < .001$). Those relatively high in IRD also tended to be relatively low on personal well-being ($b = -.56$, 95% CI [−.572, −.557], $p < .001$). The variances of the random intercepts[1] further revealed significant between-person variability in the random intercepts for income ($b^2 = .57$, 95% CI [.561, .580], $p < .001$), IRD ($b^2 = 1.33$, 95% CI [1.312, 1.353], $p < .001$), and personal well-being ($b^2 = 2.04$, 95% CI [2.009, 2.063], $p < .001$). These results justify our use of a RI-CLPM, as the traditional CLPM confounds these reliable between-person differences with within-person processes (Hamaker et al., 2015; Zyphur et al., 2020).

Within-person effects. Before inspecting the longitudinal within-person associations in our model, we first discuss the contemporaneous within-person correlations at each time point (see Table S1 in Appendix S1). These correlations reflect the extent to which temporary departures from one's trait-level mean in a variable correlate with temporary departures from one's trait-level mean in *another* variable at the same time point. While these estimates do not specify the direction of effects, they allow us to assess whether changes in income are associated with contemporaneous changes in IRD and personal well-being. Consistent with Hypothesis 1b, Table S1 reveals that within-person increases in income were reliably associated with within-person decreases in IRD (ps < .001) and within-person increases in personal

When using Osborne and Little's (in press) approach, the variance in the random intercepts is recast as the factor loadings squared.

well-being ($ps < .001$) at each of the 10 annual assessments. Within-person increases in IRD were also reliably associated with within-person decreases in personal well-being ($ps < .001$).

Concerning the longitudinal autoregressive associations, Figure 1 reveals that within-person deviations in income ($b = .50$, 95% CI [.494, .510], $p < .001$), IRD ($b = .17$, 95% CI [.160, .175], $p < .001$), and personal well-being ($b = .22$, 95% CI [.209, .224], $p < .001$) predicted subsequent within-person deviations in those same variables over time (see Table 2). Although autoregressive effects reflect the *stability* of the respective constructs in a traditional cross-lagged panel model, they capture *inertia* or the extent to which temporary departures from one's typical trait level of a construct *persist* at a subsequent assessment period in an RI-CLPM. Thus, a 1-unit departure from a person's typical trait-level income was associated with a .50-unit departure from their average income at the subsequent assessment period. Notably, these autoregressive coefficients are smaller than the autoregressive effects in the CLPM, as the random intercepts in the RI-CLPM adjust for the between-person stability in our constructs (see Osborne & Little, in press).

Turning to the cross-lagged associations, the within-person cross-lagged estimates are considerably weaker than the parameters estimated in the CLPM. Nonetheless, within-person increases in income predicted within-person decreases in IRD over time ($b = -.05$, 95% CI [$-.061$, $-.048$], $p < .001$; see Figure 1). Additionally, within-person increases in IRD predicted within-person decreases in income over time ($b = -.04$, 95% CI [$-.046$, $-.033$], $p < .001$). Similar to Model 1, applying equality constraints to these two paths revealed that the within-person effects of income on IRD were *stronger* than the within-person effects of IRD on income ($\text{Wald}_{(1)} = 15.29$, $p < .001$).

Within-person deviations in income also predicted within-person deviations in personal well-being over time ($b = .02$, 95% CI [.015, .029], $p < .001$). However, within-person deviations in personal well-being predicted comparable within-person deviations in income over time ($b = .021$, 95% CI [.015, .028], $p < .001$; $\text{Wald}_{(1)} = .02$,

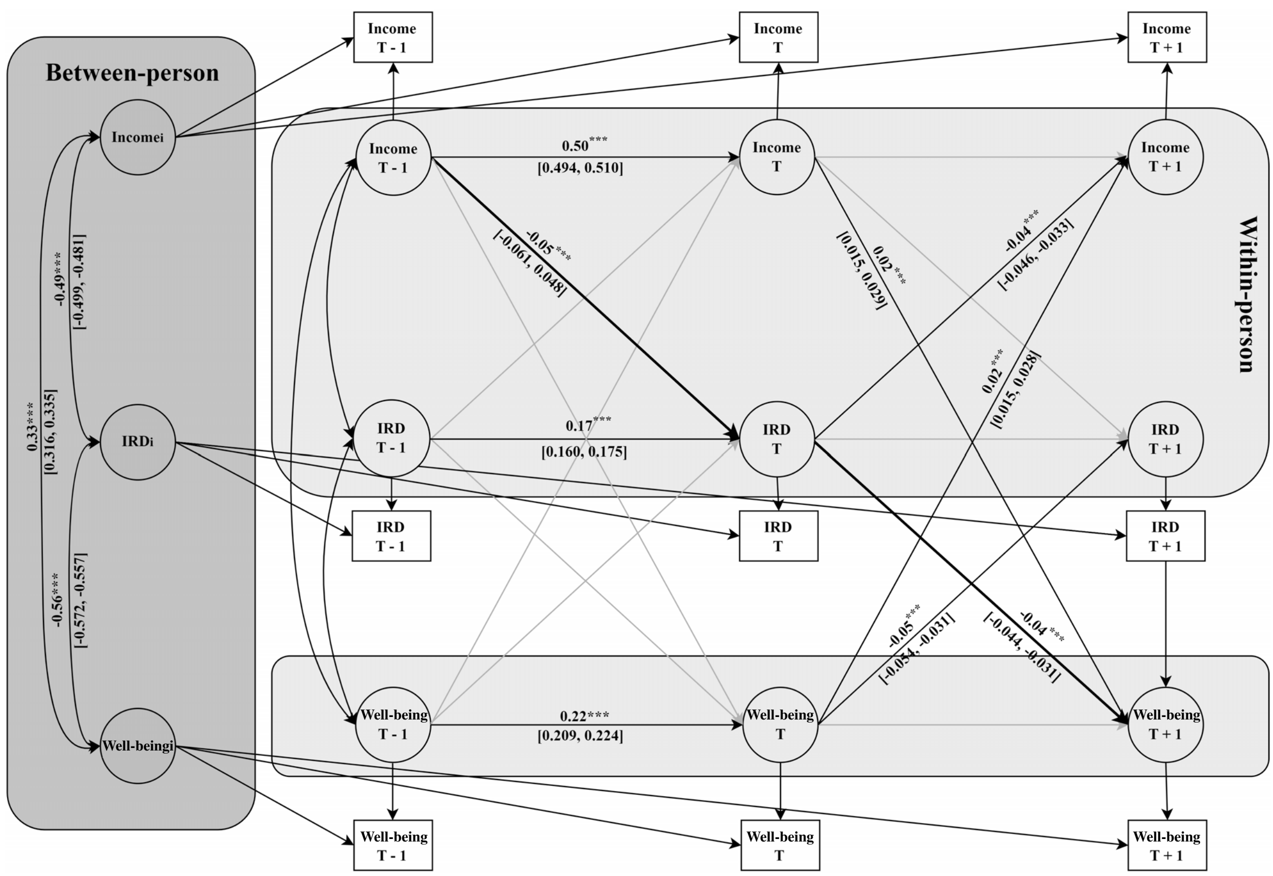

Figure 1. Results from a stationary RI-CLPM examining the temporal ordering of income, individual-based relative deprivation (IRD) and personal well-being across 10 annual assessments ($N = 66{,}560$). For clarity, the stationary autoregressive effects are displayed on the left-hand side only. Covariances between the variables at each wave were estimated but excluded from the figure for clarity. Coefficients are unstandardised (but variables were placed on a common metric using Osborne and Little's (in press) phantom rescaling approach) with 95% confidence intervals. Paths in bold represent the hypothesised mediation pathway. All paths were significant ($p < .001$).

$p = .901$), suggesting that personal well-being and income have a reciprocal relationship over time.

Finally, within-person deviations in IRD predicted within-person decreases in personal well-being ($b = -.04$, 95% CI [−.044, −.031], $p < .001$). Likewise, within-person deviations in personal well-being predicted within-person decreases in IRD ($b = -.05$, 95% CI [−.054, −.031], $p < .001$). Similar to Model 1, the within-person effects of personal well-being on IRD were stronger than that of IRD on personal well-being ($\text{Wald}_{(1)} = 5.66$, $p = .017$).

Mediation analyses. We formally examined the within-person *indirect* effects of income on personal well-being via IRD to further determine the temporal ordering of these variables. Consistent with Hypothesis 2, income has a positive indirect effect on personal well-being through IRD ($b_{\text{indirect}} = .002$, 95% CI [.001, .002], $p < .001$). That is, within-person increases in income predicted within-person decreases in IRD, which, in turn, predicted within-person increases in personal well-being (see also Table 3). Similar to Model 1, this effect reflected *partial* mediation.

Supplementary analyses

Given the conceptual similarities between item (a) of our personal well-being measure (i.e., "standard of living") and IRD, we replicated the RI-CLPM with this item removed from the scale as a robustness check. The results displayed in Appendix S1 reveal similar, albeit slightly weaker, associations to those observed in our main analyses (i.e., the 95% confidence intervals for estimates largely overlap). Thus, we are confident that our results reveal reliable associations between income, IRD and personal well-being.

Life satisfaction

We then examined the longitudinal associations between income, IRD and a general life satisfaction measure to determine whether the indirect effects of income on well-being via IRD replicate. Overall, these analyses mirror our results for personal well-being but are slightly weaker (Appendix S1 displays the full results). The only notable difference was the associations between life satisfaction and IRD; while personal well-being temporally preceded within-person change in IRD, the within-person associations between life satisfaction and IRD were reciprocal over time ($\text{Wald}_{(1)} = 1.28$, $p = .258$). Nonetheless, consistent with our main analyses and Hypothesis 2, within-person increases in income predicted *decreases* in IRD which, in turn, predicted *increases* in life satisfaction over time.

DISCUSSION

The present study used 10 annual waves of panel data from a large, nationwide random sample to examine the between- and within-person associations income had with IRD and personal well-being. A traditional CLPM revealed significant cross-lagged associations between these variables and that increases in income were *indirectly* associated with increases in well-being via IRD. An RI-CLPM partitioning between-person stability from within-person change also revealed that, at the between-person level, people who were higher (vs. lower) on income across the 10 annual assessments were also lower in IRD and higher in personal well-being. Likewise, people who were higher (vs. lower) in IRD were relatively lower in personal well-being. Similar effects emerged contemporaneously at the *within*-person level, demonstrating reliable cross-sectional associations between these constructs both within and between people.

In addition to these contemporaneous results, an RI-CLPM revealed that within-person *changes* in income indirectly predicted changes in personal well-being through IRD. Notably, these results replicated in a robustness check for personal well-being and when examining a global measure of life satisfaction. Overall, these results corroborate and extend past research examining objective measures of relative deprivation (Cheung & Lucas, 2016; Clark & D'Ambrosio, 2015) by demonstrating how changes in material conditions can (temporarily) increase the *perception* of differences between the "haves" and the "have-nots" and, in turn, affect well-being.

Our comparisons between traditional CLPMs and RI-CLPMs highlight important distinctions between these two approaches. Although our focal variables were significantly associated across models, a traditional CLPM cannot explain within-person change processes at the core of most psychological theories (Hamaker et al., 2015; Osborne & Little, in press). Critically, recent work argues that traditional CLPMs are also unable to speak to *between*-person processes and that this modelling approach should be abandoned altogether (Lucas, 2023). Here, our results reveal that the autoregressive and cross-lagged estimates in the CLPM were conflated, which, when used in isolation, may lead to incorrect conclusions regarding the strength of associations across time. Conversely, the RI-CLPM estimates were more conservative because they appropriately separated between-person stability from within-person change. Thus, the RI-CLPM is better suited to examine both between- and within-person processes. While the RI-CLPM is not a "perfect" model, researchers should move away from the CLPM as the "workhorse" of psychological research (Berry & Willoughby, 2017) and consider alternative approaches to modelling longitudinal change over time.

Critically, our results highlight the potential to improve people's well-being by reducing the material disparities that foster social comparisons. Although the stigma of poverty and its effects on access to essential resources directly affect one's health and well-being (Rosenberg & Pearlin, 1978; Twenge & Campbell, 2002), people's perception of how they *compare* relative to others is often a better predictor of one's response to inequality (Smith et al., 2012). For the objectively disadvantaged, their status *relative* to those at the "top" increases social comparisons that produce greater feelings of personal deprivation and, in turn, poorer well-being. Creating more equitable conditions could thus close the "gap" between the advantaged and disadvantaged, improve one's perceived relative position, and—ultimately—bolster one's well-being.

Strengths, caveats and future research directions

A notable strength of our study is the use of a large, nationwide probability sample alongside a well-established measure of IRD (Smith et al., 2012), which increases confidence in the generalisability of our results. Moreover, income and IRD were significantly associated with personal well-being and life satisfaction at both the between- and within-person levels of analysis across 10 annual assessments, which speaks to how wealth (or a lack thereof) can reliably predict disparities in well-being between *and* within people. Finally, our results demonstrate the utility of RI-CLPMs (relative to traditional CLPMs) when examining temporal precedence at the within-person level in an externally valid context (Zyphur et al., 2020). In sum, our study provides a novel examination of the longitudinal association between income and well-being, as well as the psychological process through which this relationship emerges.

Despite these strengths, our study has some limitations. Namely, while our study identifies the temporal ordering of our focal variables, the within-person associations in an RI-CLPM alone cannot determine causality. Indeed, many of our cross-lagged associations were bidirectional, and we cannot rule out other reciprocal pathways nor claim changes in income *cause* changes in one's feelings of IRD and well-being. Moreover, the mediating effects of IRD were small and only partially explained the relationship between income and well-being over time. Thus, future research should consider other factors that may explain this association. Relatedly, while our results replicated across measures of personal well-being and life satisfaction, research examining a broad range of well-being domains is needed to determine the extent to which income affects various aspects of well-being via IRD. Nonetheless, the current study provides tentative evidence that material factors (namely, decreases in income) temporally precede increases in IRD and ensuing decreases in well-being, thereby laying the critical groundwork for future research.

Our study also only assessed *annual* longitudinal associations. Future research is thus needed to determine the influence of income on IRD and well-being at shorter (or longer) intervals. This is particularly relevant in the present study, as RI-CLPMs are best suited for shorter intervals (i.e., days or months; Orth et al., 2021). Thus, future research utilising this method should employ shorter assessment intervals to investigate whether more recent changes in income exert a stronger (or, counterintuitively, weaker) influence on IRD and well-being over time.

Finally, our measure of income assessed *household*, rather than *personal*, income. Given that personal income is a more individual-level indicator of income, the association between *personal* income and IRD may be stronger (or weaker) than the corresponding association between household income and IRD. That said, household income is an appropriate measure of relative wealth (or lack thereof), as it captures individuals' available resources and the considerable variability between low- and high-income households. An interesting avenue of future research could examine the associations between IRD and one's *contribution* to household income (i.e., personal income). Moreover, examining the effects of children or dependents on this association would allow for a more nuanced understanding of how material conditions impact well-being. While outside the scope of this study, future research should consider different avenues for measuring how one's material conditions foster (or undermine) well-being over time and whether these associations differ from the effects of household income.

Conclusion

Given the unprecedented increase in income inequality observed in the last 40 years, it is essential to understand how material conditions affect well-being. We investigate one potential mechanism whereby objective wealth (or a lack thereof) fosters relative comparisons, which, in turn, predicts well-being over time. As hypothesised, our results demonstrate that within-person changes in income predict well-being via *perceived* IRD. These results extend previous research by showing how one's *perceptions* of their status explain the relationship between *objective* material conditions and well-being, highlighting the need to consider one's *relative* societal position when explaining *why* income inequality undermines well-being.

COMPLIANCE WITH ETHICAL STANDARDS

All procedures performed in studies involving human participants were in accordance with the ethical standards of the institutional research committee at The University of Auckland and with the 1964 Helsinki Declaration and its later amendments or comparable ethical standards. The participants provided their written informed consent to participate in this study.

DATA AVAILABILITY STATEMENT

The data described in this research are part of the New Zealand Attitudes and Values Study (NZAVS). Full copies of the NZAVS data files are held by all members of the NZAVS management team and advisory board. A de-identified dataset containing the variables analysed in this manuscript is available upon request from the corresponding author or any NZAVS advisory board member for replication or checking of any published study using NZAVS data. The Mplus syntax used to test all models reported in this manuscript is available via the Open Science Framework: https://osf.io/75snb/.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Appendix S1. Supporting Information.

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