The Transition to Grandparenthood and its Impact on the Big Five Personality Traits and Life Satisfaction

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Abstract

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Becoming a grandmother or grandfather is a pivotal life event for many people in midlife or old age (Infurna et al., 2020). At the same time, there is considerable heterogeneity in how intensely grandparents are involved in their grandchildren's lives and care (Meyer & Kandic, 2017). In view of an aging demographic, the time that grandparents are alive and in good health during grandparenthood is prolonged compared to previous generations (Leopold & Skopek, 2015; Margolis & Wright, 2017). In addition, an increased share of childcare functions are being fulfilled by grandparents (Hayslip et al., 2019; Pilkauskas et al., 2020). Thus, intergenerational relations have received heightened attention from psychological and sociological research in recent years (Bengtson, 2001; Coall & Hertwig, 2011). With regard to personality development, the transition to grandparenthood has been posited as an important developmental task in old age (Hutteman et al., 2014). However, empirical research into the psychological consequences of becoming a grandparent is sparse. Testing hypotheses derived from neo-socioanalytic theory (Roberts & Wood, 2006) in a prospective matched control-group design (see Luhmann et al., 2014), we investigate whether the transition to grandparenthood affects the Big Five personality traits and life satisfaction using data from two nationally representative panel studies.

Personality Development in Middle Adulthood and Old Age

The life span perspective characterizes aging as a lifelong process of development and adaptation (Baltes et al., 2006). In accordance with this perspective, research has found personality traits to be subject to change throughout the entire life span (Costa et al., 2019; Graham et al., 2020; Specht, 2017; Specht et al., 2014). Although a major portion of personality development takes place in adolescence and emerging adulthood (Bleidorn & Schwaba, 2017; Schwaba & Bleidorn, 2018), evidence has accumulated that personality traits also undergo changes in middle and old adulthood (e.g., Damian et al., 2019; Kandler et al., 2015; Lucas & Donnellan, 2011; Mõttus et al., 2012; Wagner et al., 2016; for a review, see Specht, 2017).

Here, we examine the Big Five personality traits—agreeableness, conscientiousness, extraversion, neuroticism, and openness to experiences—which constitute a broad categorization of universal patterns of thought, affect, and behavior (John et al., 2008). While the policy relevance of the Big Five personality traits has recently been emphasized (Bleidorn et al., 2019)—especially because of their predictive power regarding many important life outcomes (Ozer & Benet-Martínez, 2005; Roberts et al., 2007; Soto, 2019), we acknowledge that there are other viable taxonomies of personality (Ashton & Lee, 2007) and other levels of breadth and scope that could add valuable insights to personality development in middle adulthood and old age (Mõttus et al., 2017; Mõttus & Rozgonjuk, 2021).

Changes over time in the Big Five occur both in mean trait levels (i.e., mean-level change; Roberts et al., 2006) and in the relative ordering of people to each other on trait dimensions (i.e., rank-order stability; Anusic & Schimmack, 2016; Roberts & DelVecchio, 2000). No observed changes in mean trait levels do not necessarily mean that individual trait levels are stable over time, and perfect rank-order stability does not preclude mean-level changes. Mean-level changes in middle adulthood (ca. 30–60 years old; Hutteman et al., 2014) are typically characterized in terms of greater maturity as evidenced by increased agreeableness and conscientiousness, and decreased neuroticism (Damian et al., 2019; Roberts et al., 2006). In old age (ca. 60 years and older; Hutteman et al., 2014), research is generally more sparse but there is some evidence for a reversal of the maturity effect, especially following retirement (sometimes termed *la dolce vita* effect; Marsh et al., 2013; cf. Schwaba & Bleidorn, 2019) and at the end of life in ill health (Wagner et al., 2016).

In terms of rank-order stability, some prior studies have shown support for an inverted U-shape trajectory (Ardelt, 2000; Lucas & Donnellan, 2011; Specht et al., 2011; Wortman et al., 2012): Rank-order stability rises until reaching a plateau in midlife, and decreases, again, in old age. However, evidence is mixed whether rank-order stability actually decreases again in old age (see Costa et al., 2019). Nonetheless, the historical view that personality is stable, or "set like plaster" (Specht, 2017, p. 64) after one reaches adulthood (or leaves emerging adulthood behind; Bleidorn & Schwaba, 2017) can largely be abandoned (Specht et al., 2014).

Theories explaining the mechanisms of personality development in middle adulthood and old age emphasize both genetic influences and life experiences as interdependent sources of stability and change (Specht et al., 2014; Wagner et al., 2020). We focus on the latter[1] and conceptualize the transition to grandparenthood as a life experience that offers the adoption of a new social role according to the social investment principle of neo-socioanalytic theory (Lodi-Smith & Roberts, 2007; Roberts & Wood, 2006). According to the social investment principle, normative life events or transitions such as entering the work force or becoming a parent lead to personality maturation through the adoption of new social roles (Roberts et al., 2005). These new roles encourage or compel people to act in a more agreeable, conscientious, and emotionally stable (i.e., less neurotic) way, and the experiences in these roles as well as societal expectations towards them are hypothesized to drive long-term personality development (Lodi-Smith & Roberts, 2007; Wrzus & Roberts, 2017). Conversely, consistent social roles foster personality stability.

The paradoxical theory of personality coherence (Caspi & Moffitt, 1993) offers another explanation for personality development through role shifts stating that trait change is more likely whenever people transition into unknown environments where pre-existing behavioral responses are no longer appropriate and societal norms or social expectations give clear indications how to behave instead. On the other hand, stability is

[1] In a behavior-genetic twin study, Kandler et al. (2015) found that non-shared environmental factors were the main source of personality plasticity in old age.

favored in environments where no clear guidance how to behave is available. Thus, the finding that age-graded, normative life experiences such as possibly the transition to grandparenthood drive personality development would also be in line with the paradoxical theory of personality coherence (see Specht et al., 2014).

Certain life events such as the first romantic relationship (Wagner et al., 2015) or the transition from high school to university (Lüdtke et al., 2011) have (partly) been found to be accompanied by mean-level increases in line with the social investment principle (for a review, see Bleidorn et al., 2018). However, recent evidence regarding the transition to parenthood failed to empirically support the social investment principle (Asselmann & Specht, 2020; van Scheppingen et al., 2016). An analysis of monthly trajectories of the Big Five before and after nine major life events only found limited support for the social investment principle: small increases were found in emotional stability following the transition to employment but not for the other traits or for the other life events theoretically linked to social investment (Denissen et al., 2019). Recently, it has also been emphasized that effects of life events on the Big Five personality trends generally tend to be small and need to be properly analyzed using robust, prospective designs and appropriate control groups (Bleidorn et al., 2018; Luhmann et al., 2014).

Overall, much remains unknown regarding the environmental factors underlying personality development in middle adulthood and old age. One indication that age-graded, normative life experiences contribute to change following a period of relative stability in midlife is offered by recent research on retirement (Bleidorn & Schwaba, 2018; Schwaba & Bleidorn, 2019). These results were only partly in line with the social investment principle in terms of mean-level changes and displayed substantial individual differences in change trajectories. The authors discuss that as social role "divestment" (Schwaba & Bleidorn, 2019, p. ?) retirement functions differently compared to social investment in the classical sense which adds a role. The transition to grandparenthood could represent such an investment into a new role in middle adulthood and old age—given that grandparents have regular contact with their grandchild and actively take part in childcare to some degree (i.e., invest psychologically in the new grandparent role; Lodi-Smith & Roberts, 2007).

Grandparenthood

The transition to grandparenthood, that is, the birth of the first grandchild, can be described as a time-discrete life event marking the beginning of one's status as a grandparent (Luhmann et al., 2012). In terms of characteristics of major life events (Luhmann et al., 2020), the transition to grandparenthood stands out in that it is externally caused (by one's own children; see also Arpino, Gumà, et al., 2018; Margolis & Verdery, 2019), while at the same time being predictable as soon as one's children reveal their pregnancy or family planning. The transition to grandparenthood has been labeled a countertransition due to this lack of direct control over if and when someone has their first grandchild (Hagestad & Neugarten, 1985; as cited in Arpino, Gumà, et al., 2018). Grandparenthood is also generally positive in valence and emotionally significant—given one maintains a good relationship with their child.

Grandparenthood can also be characterized as a developmental task (Hutteman et al., 2014) mostly associated with the period of (early) old age—although considerable variation in the age at the transition to grandparenthood exists both within and between cultures (Leopold & Skopek, 2015; Skopek & Leopold, 2017). Still, the period where parents on average experience the birth of their first grandchild coincides with the end of (relative) stability in terms of personality development in midlife (Specht, 2017), where retirement, shifting social roles, and initial cognitive and health declines can potentially be disruptive to life circumstances putting personality development into motion (e.g., Mueller et al., 2016; Stephan et al., 2014). As a developmental task, grandparenthood is expected to be part of a normative sequence of aging that is subject to societal expectations and values differing across cultures and historical time (Baltes et al., 2006; Hutteman et al., 2014).

Mastering developmental tasks (i.e., fulfilling roles and expectations to a high degree) is hypothesized to drive personality development towards maturation similarly to propositions by the social investment principle, that is, leading to higher levels of agreeableness and conscientiousness, and lower levels of neuroticism (Roberts et al., 2005; Roberts & Wood, 2006). In comparison to the transition to parenthood which has been found to be ambivalent in terms of both personality maturation and life satisfaction (Krämer & Rodgers, 2020; van Scheppingen et al., 2016), Hutteman et al. (2014) hypothesize that the transition to grandparenthood is generally seen as positive because it (usually) does not impose the stressful demands of daily childcare on grandparents. Grandparental investment in their grandchildren has been discussed as beneficial in terms of the evolutionary, economic, and sociological advantages it provides for the whole intergenerational family structure (Coall et al., 2018; Coall & Hertwig, 2011).

While we could not find prior studies investigating development of the Big Five over the transition to grandparenthood, there is some evidence on life satisfaction. We define life satisfaction as the general, cognitive appraisal of one's well-being in life based on subjective criteria (Eid & Larsen, 2008). Previous research on associations of grandparenthood with life satisfaction has often relied on cross-sectional designs (e.g., Mahne & Huxhold, 2014; Triadó et al., 2014). There are a few studies with longitudinal designs although with conflicting conclusions: Longitudinal studies utilizing panel data from the Survey of Health, Ageing and Retirement in Europe (SHARE) showed that the birth of a grandchild was followed by improvements to quality of life and life satisfaction only among women (Tanskanen et al., 2019), and only in first-time grandmothers via their daughters (Di Gessa et al., 2019). Several studies emphasized that grandparents actively involved in childcare experienced larger increases in life satisfaction (Arpino, Bordone, et al., 2018; Danielsbacka et al., 2019; Danielsbacka & Tanskanen, 2016). On the other hand, fixed effects regression models[2] using SHARE data did not find any effects of first-time

[2] Fixed effects regression models exclusively rely on within-person variance (see Brüderl & Ludwig, 2015; McNeish & Kelley, 2019).

grandparenthood on life satisfaction regardless of grandparental investment and only minor decreases of grandmothers' depressive symptoms (Sheppard & Monden, 2019).

In a similar vein, some prospective studies reported beneficial effects of the transition to grandparenthood and of grandparental childcare investment on various health measures, especially in women (Chung & Park, 2018; Condon et al., 2018; Di Gessa et al., 2016a, 2016b). Again, beneficial effects on self-rated health did not persevere in fixed effects analyses as reported in Ates (2017) who used longitudinal data from the German Aging Survey (DEAS).

Current Study

Three research questions motivate the current study which is the first to analyze personality development over the transition to grandparenthood with regards to the Big Five traits:

1. What are the effects of the transition to grandparenthood on mean-level trajectories of the Big Five traits and life satisfaction?
2. How large are interindividual differences in intraindividual change for the Big Five traits and life satisfaction over the transition to grandparenthood?
3. How does the transition to grandparenthood affect rank-order stability of the Big Five traits and life satisfaction?

To address these questions, we compare development over the transition to grandparenthood with that of matched participants who do not experience the transition during the study period (Luhmann et al., 2014). This is necessary because pre-existing differences between prospective grandparents and non-grandparents in variables related to the development of the Big Five or life satisfaction introduce confounding bias when estimating the effects of the transition to grandparenthood (VanderWeele et al., 2020). The impact of adjusting (or not adjusting) for pre-existing differences, or background characteristics, has recently been emphasized in the prediction of life outcomes from personality in a mega-analytic framework of ten large panel studies (Beck & Jackson, 2021). Propensity score matching is one technique to account for confounding bias by equating the groups in their estimated propensity to experience the event in question (Thoemmes & Kim, 2011). This propensity is calculated from regressing the so-called treatment variable (i.e., the group variable indicating whether someone experienced the event) on covariates related to the likelihood of experiencing the event and to the outcomes. Thereby, in addressing confounding bias balance between the groups in the covariates used to calculate the propensity score is also aimed for (Stuart, 2010).

We adopt a prospective design that tests effects of first-time grandparents separately against two propensity-score-matched control groups: first, a matched control group of parents (but not grandparents) with at least their oldest child in reproductive age, and, second, a matched control group of nonparents. This allows us to disentangle potential effects attributable to becoming a grandparent from effects attributable to being a parent already, thus addressing selection effects into grandparenthood and confounding more comprehensively than previous research. Thereby, we cover the first two of the three causal pathways to not experiencing grandparenthood pointed out by demographic research (Margolis & Verdery, 2019): one's own childlessness, childlessness of one's children during one's life, and (premature) death. Our comparative design also controls for average age-related and historical trends in the Big Five traits and life satisfaction (Luhmann et al., 2014), and enables us to report effects of the transition to grandparenthood unconfounded by instrumentation effects, which describe the tendency of reporting lower well-being scores with each repeated measurement (Baird et al., 2010).[3]

We improve upon previous longitudinal studies utilizing matched control groups (e.g., Anusic et al., 2014a, 2014b; Yap et al., 2012) in that we performed the matching at a

[3] Instrumentation effects caused by repeated assessments have only been described for life satisfaction but we assume similar biases exist for certain Big Five items.

specific time point preceding the transition to grandparenthood (at least two years beforehand) and not based on individual survey years. This design choice ensures that the covariates involved in the matching procedure are not already influenced by the event or anticipation of it (Greenland, 2003; Rosenbaum, 1984; VanderWeele, 2019; VanderWeele et al., 2020), thereby also reducing the risk of confounding through collider bias (Elwert & Winship, 2014). Similar approaches in the study of life events have recently been adopted (Balbo & Arpino, 2016; Krämer & Rodgers, 2020; van Scheppingen & Leopold, 2020).

Informed by the social investment principle and previous research on personality development in middle adulthood and old age, we preregistered the following hypotheses (prior to data analysis; osf.io/):

- H1a: Following the birth of their first grandchild, grandparents increase in agreeableness and conscientiousness, and decrease in neuroticism as compared to the matched control groups of parents (but not grandparents) and nonparents, but do not differ in their trajectories of extraversion and openness to experience.
- H1b: Grandparents' post-transition increases in agreeableness and conscientiousness, and decreases in neuroticism are more pronounced among those who provide substantial grandchild care.
- H1c: Grandmothers increase in life satisfaction following the transition to grandparenthood as compared to the matched control groups but grandfathers do not.
- H2: Individual differences in intraindividual change in the Big Five and life satisfaction are larger in the grandparent group than the control groups.
- H3a: Compared to the matched control groups, grandparents' rank-order stability of the Big Five traits over the transition to grandparenthood is smaller.
- H3b: Grandparents' rank-order stability of life satisfaction is comparatively stable over the transition to grandparenthood.

Exploratorily, we further probe the moderator performing paid work which could constitute a potential role conflict among grandparents.

Methods

Samples

To evaluate these hypotheses, we used data from two population-representative panel studies: the Longitudinal Internet Studies for the Social Sciences (LISS) panel from the Netherlands and the Health and Retirement Study (HRS) from the United States.

The LISS panel is a representative sample of the Dutch population initiated in 2008 with data collection still ongoing (Scherpenzeel, 2011; van der Laan, 2009). It is administered by CentERdata (Tilburg University, The Netherlands). Included households are a true probability sample of households drawn from the population register (Scherpenzeel & Das, 2010). While originally roughly half of invited households consented to participate, refreshment samples were drawn in order to oversample previously underrepresented groups using information about response rates and their association with demographic variables (household type, age, ethnicity; see https://www.lissdata.nl/about-panel/sample-and-recruitment/). Data collection was carried out online and participants lacking the necessary technical equipment were outfitted with it. We included yearly assessments from 2008 to 2020 from several different modules (see *Measures*) as well as data on basic demographics which was assessed on a monthly rate. For later coding of covariates from these monthly demographic data we used the first available assessment in each year.

The HRS is an ongoing longitudinal population-representative study of older adults in the US (Sonnega et al., 2014) administered by the Survey Research Center (University of Michigan, United States). Initiated in 1992 with a first cohort of individuals aged 51-61 and their spouses, the study has since been extended with additional cohorts in the 1990s (see https://hrs.isr.umich.edu/documentation/survey-design/). In addition to the HRS core interview every two years (in-person or as a telephone survey), the study has since

2006 included a leave-behind questionnaire covering a broad range of psychosocial topics including the Big Five personality traits and life satisfaction. These topics, however, were only administered every four years starting in 2006 for one half of the sample and in 2008 for the other half. We included personality data from 2006 to 2018, all available data for the coding of the transition to grandparenthood from 1996 to 2018, as well as covariate data from 2006 to 2018 including variables drawn from the Imputations File and the Family Data (only available up to 2014).

These two panel studies provided the advantage that they contained several waves of personality data as well as information on grandparent status and a broad range of covariates at each wave. While the HRS provided a large sample with a wider age range, the LISS panel was smaller and younger[4] but provided more frequent personality assessments spaced every one to two years. Note that M. van Scheppingen has previously used the LISS panel to analyze ???. B. Chopik has previously used the HRS to analyze ???. These publications do not overlap with the current study in the central focus of grandparenthood.[5] The present study used de-identified archival data in the public domain, and, thus, it was not necessary to obtain ethical approval from an IRB.

Measures

Personality

In the LISS panel, the Big Five personality traits were assessed using the 50-item version of the IPIP Big-Five Inventory scales (Goldberg, 1992). For each Big Five trait, ten 5-point Likert-scale items were answered (1 = *very inaccurate*, 2 = *moderately inaccurate*, 3 = *neither inaccurate nor accurate*, 4 = *moderately accurate*, 5 = *very accurate*). Example

[4] The reason for the included grandparents from the LISS panel being younger was that grandparenthood questions were part of the *Work and Schooling* module and—for reasons unknown to us—filtered to participants performing paid work. Thus, older, retired first-time grandparents from the LISS panel could not be identified.

[5] Publications using LISS panel data can be searched at https://www.dataarchive.lissdata.nl/publications/. Publications using HRS data can be searched at https://hrs.isr.umich.edu/publications/biblio/.

items included “Like order” (conscientiousness), “Sympathize with others’ feelings” (agreeableness), “Worry about things” (neuroticism), “Have a vivid imagination” (openness to experience), and “Start conversations” (extraversion). At each wave, we took a participant’s mean of each subscale as their trait score. Internal consistencies at the time of matching, as indicated by McDonald’s ω (McNeish, 2018), averaged $\omega = 0.83$ over all traits ranging from $\omega = 0.76$ (conscientiousness in the nonparent control group) to $\omega = 0.90$ (extraversion in the nonparent control group). Another study has shown measurement invariance for these scales across time and age groups (Schwaba & Bleidorn, 2018). The Big Five (and life satisfaction) were contained in the *Personality* module which was administered yearly but with planned missingness in some years for certain cohorts (see Denissen et al., 2019). Thus, there are one to two years between included assessments, given no other sources of missingness.

In the HRS, the Midlife Development Inventory (MIDI) scales were administered to measure the Big Five (Lachman & Weaver, 1997). This instrument was constructed for use in large-scale panel studies of adults and consisted of 26 adjectives (five each for conscientiousness, agreeableness, and extraversion, four for neuroticism, and seven for openness to experience). Participants were asked to rate on a 4-point scale how well each item described them (1 = *a lot*, 2 = *some*, 3 = *a little*, 4 = *not at all*). Example adjectives included “Organized” (conscientiousness), “Sympathetic” (agreeableness), “Worrying” (neuroticism), “Imaginative” (openness to experience), and “Talkative” (extraversion). For better comparability with the LISS panel, we reverse scored all items so that higher values corresponded to higher trait levels and, at each wave, took the mean of each subscale as the trait score. Big Five trait scores showed satisfactory internal consistencies at the time of matching which averaged $\omega = 0.75$ over all traits ranging from $\omega = 0.66$ (conscientiousness in the nonparent control group) to $\omega = 0.81$ (agreeableness in the nonparent control group).

Life Satisfaction

In both samples, life satisfaction was assessed using the 5-item Satisfaction with Life Scale (SWLS; Diener et al., 1985) which participants answered on a 7-point Likert scale (1 = *strongly disagree*, 2 = *somewhat disagree*, 3 = *slightly disagree*, 4 = *neither agree or disagree*, 5 = *slightly agree*, 6 = *somewhat agree*, 7 = *strongly agree*)[6]. An example item was "I am satisfied with my life". Internal consistency at the time of matching was ω = 0.89 in the LISS panel with the parent control sample (ω = 0.88 with the nonparent control sample), and ω = 0.91 in the HRS with the parent control sample (ω = 0.90 with the nonparent control sample).

Transition to Grandparenthood

The procedure to obtain information on grandparents' transition to grandparenthood generally followed the same steps in both samples. The items this coding was based on, however, differed slightly: In the LISS panel, participants were asked "Do you have children and/or grandchildren?" with "children", "grandchildren", and "no children or grandchildren" as possible answer categories. This question was part of the *Work and Schooling* module and filtered to participants performing paid work. In the HRS, all participants were asked for the total number of grandchildren: "Altogether, how many grandchildren do you (or your husband / wife / partner, or your late husband / wife / partner) have? Include as grandchildren any children of your (or your [late] husband's / wife's / partner's) biological, step- or adopted children".[7]

In both samples, we tracked grandparenthood status (0 = *no grandchildren*, 1 = *at least one grandchild*) over time. Due to longitudinally inconsistent data in some cases, we included in the grandparent group only participants with exactly one transition from 0 to 1 in this grandparenthood status variable, and no transitions backwards (see Fig. SX). We

[6] In the LISS panel, the "somewhat" was omitted and instead of "or" "nor" was used.

[7] The listing of biological, step-, or adopted children has been added since wave 2006.

marked participants who continually indicated that they had no grandchildren as potential members of the control groups.

Covariates

For propensity score matching, we used a broad set of covariates (VanderWeele et al., 2020) covering participants' demographics (e.g., education), economic situation (e.g., income), and health (e.g., mobility difficulties). We also included the pre-transition outcome variables as covariates—as recommended in the literature (Cook et al., 2020; Hallberg et al., 2018; Steiner et al., 2010; VanderWeele et al., 2020), as well as the panel wave participation count and assessment year in order to control for instrumentation effects and historical trends (e.g., 2008/2009 financial crisis; Baird et al., 2010; Luhmann et al., 2014). For matching grandparents with the parent control group we additionally included as covariates variables containing information on fertility and family history (e.g., number of children, age of first three children) which were causally related to the timing of the transition to grandparenthood (i.e., entry into treatment; Arpino, Gumà, et al., 2018; Margolis & Verdery, 2019).

Covariate selection has seldom been explicitly discussed in previous longitudinal studies estimating treatment effects of life events (e.g., in matching designs). We see two (in part conflicting) traditions that address covariate selection: First, classical recommendations from psychology argue to include all available variables that are associated with both the treatment assignment process (i.e., selection into treatment) and the outcome (e.g., Steiner et al., 2010; Stuart, 2010). Second, recommendations from a structural causal modeling perspective (see Elwert & Winship, 2014; Rohrer, 2018) are more cautious aiming to avoid pitfalls such as conditioning on a pre-treatment collider (collider bias) or a mediator (overcontrol bias). Structural causal modeling, however, requires advanced knowledge of the causal structures underlying all involved variables (Pearl, 2009).

In selecting covariates, we followed guidelines laid out by VanderWeele et al. (2019; 2020) which reconcile both views and offer practical guidance[8] when complete knowledge of the underlying causal structures is unknown: These authors propose a "modified disjunctive cause criterion" (VanderWeele, 2019, p. 218) recommending to select all available covariates which are assumed to be causes of the outcomes, treatment exposure (i.e., the transition to grandparenthood), or both, as well as any proxies for an unmeasured common cause of the outcomes and treatment exposure. To be excluded from this selection are variables assumed to be instrumental variables (i.e., assumed causes of treatment exposure that are unrelated to the outcomes except through the exposure) and collider variables (Elwert & Winship, 2014). Because all covariates we used for matching were measured at least two years before the birth of the grandchild, we judge the risk of introducing collider bias or overcontrol bias by controlling for these covariates to be relatively small. In addition, as mentioned in the *Introduction*, the event transition to grandparenthood is not planned by or under direct control of grandparents which further reduces the risk of bias introduced by controlling for pre-treatment colliders.

An overview of the variables we used to compute the propensity scores for matching can be found in the Supplemental Material (see also Tables S2 & S3). Critically, we also provide justification for each covariate on whether we assume it to be causally related to treatment assignment, the outcomes, or both. We tried to find substantively equivalent covariates in both samples but had to compromise in a few cases (e.g., children's educational level only in HRS vs. children living at home only in LISS).

Estimating propensity scores requires complete covariate data. Therefore, before computing propensity scores, we performed multiple imputations in order to account for missingness in our covariates (Greenland & Finkle, 1995). Using five imputed data sets computed by classification and regression trees (CART; Burgette & Reiter, 2010) in the

[8] Practical considerations of covariate selection when using large archival datasets (i.e., with no direct control over data collection) are discussed in VanderWeele et al. (2020).

mice R package (van Buuren & Groothuis-Oudshoorn, 2011), we predicted treatment assignment (i.e., the transition to grandparenthood) five times per observation in logistic regressions with a logit link function.[9] We averaged these five scores per observation to compute the final propensity score to be used for matching (Mitra & Reiter, 2016). We used imputed data only for propensity score computation and not in later analyses because missing data in the outcome variables due to nonresponse was negligible.

Moderators

Based on insights from previous research, we tested three variables as potential moderators of the mean-level trajectories of the Big Five and life satisfaction over the transition to grandparenthood: First, we analyzed whether gender acted as a moderator as indicated by research on life satisfaction (see Tanskanen et al., 2019; Di Gessa et al., 2019). We coded a dummy variable indicating female gender (0 = *male*, 1 = *female*).

Second, we tested whether performing paid work or not was associated with divergent trajectories of the Big Five and life satisfaction (see Schwaba & Bleidorn, 2019). Since the LISS subsample of grandparents we identified was based exclusively on participants performing paid work, we performed these analyses only in the HRS subsample. This served two purposes: to test how participants involved in the workforce (even if officially retired) differed from those not working, which might shed light on role conflict. As a robustness check this also allowed us to assess whether potential differences in the main results between the LISS and HRS samples could be accounted for by including performing paid work as a moderator in analyses of the HRS sample. The LISS grandparent sample had already been conditioned on this variable through filtering in the questionnaire.

Third, we examined how involvement in grandchild care moderated trajectories of

[9] In these logistic regressions we included all covariates listed above as predictors except for *female* which was later used for exact matching and health-related covariates in LISS-wave 2014 which were not assessed in that wave.

the Big Five and life satisfaction in grandparents after the transition to grandparenthood (see Arpino, Bordone, et al., 2018; Danielsbacka et al., 2019; Danielsbacka & Tanskanen, 2016). We coded a dummy variable (0 = *provided less than 100 hours of grandchild care*, 1 = *provided 100 or more hours of grandchild care*) as a moderator based on the question "Did you (or your [late] husband / wife / partner) spend 100 or more hours in total since the last interview / in the last two years taking care of grand- or great grandchildren?".[10] This information was only available for grandparents in the HRS; in the LISS panel too few participants answered follow-up questions on intensity of care (>50 in the final analysis sample).

Procedure

Drawing on all available data, three main restrictions defined the final analysis samples of grandparents (see Fig. SX for participant flowcharts): First, we identified participants who indicated having grandchildren for the first time during study participation (see *Measures*; $N_{LISS} = 337$; $N_{HRS} = 3272$, including HRS waves 1996-2004 before personality assessments were introduced). Second, we restricted the sample to participants with at least one valid personality assessment (valid in the sense that at least one of the six outcomes was non-missing; $N_{LISS} = 335$; $N_{HRS} = 1702$).[11] Third, we included in the analysis samples only participants with both a valid personality assessment before and one after the transition to grandparenthood ($N_{LISS} = 253$; $N_{HRS} = 859$). Lastly, few participants were excluded because of inconsistent or missing information regarding their children[12] resulting in the final analysis samples of first-time grandparents,

[10] Although dichotomization of a continuous construct (hours of care) is not ideal for moderation analysis (MacCallum et al., 2002), there were too many missing values in the variable assessing hours of care continuously (variables *E063).

[11] For the HRS subsample, we also excluded $N = 30$ grandparents in a previous step who reported unrealistically high numbers of grandchildren (> 10) in their first assessment following the transition to grandparenthood.

[12] We opted not to use multiple imputation for these child-related variables such as number of children which defined the control groups and were also later used for computing the propensity scores.

$N_{LISS} = 250$ (53.60% female; age at transition to grandparenthood $M = 57.94$, $SD = 4.87$) and $N_{HRS} = 846$ (54.85% female; age at transition to grandparenthood $M = 61.80$, $SD = 6.88$).

To disentangle effects of the transition to grandparenthood from effects of being a parent, we defined two pools of potential control subjects to be involved in the matching procedure: The first pool of potential control subjects comprised parents who had at least one child in reproductive age (defined as $15 \leq age_{firstborn} \leq 65$) but no grandchildren throughout the observation period ($N_{LISS} = 844$ with 3040 longitudinal observations; $N_{HRS} = 1485$ with 2703 longitudinal observations). The second pool of potential matches comprised participants who reported being childless throughout the observation period ($N_{LISS} = 1077$ with 4337 longitudinal observations; $N_{HRS} = 1340$ with 2346 longitudinal observations). The two control groups were, thus, by definition mutually exclusive.

In order to match each grandparent with the control participant who was most similar in terms of the included covariates we utilized propensity score matching. Propensity score matching was performed in a grandparent's survey year which preceded the year when the transition was first reported by at least two years (aside from that choosing the smallest available gap between matching and transition). This served the purpose to ensure that the covariates used for matching were not affected by the event itself or its anticipation (i.e., when one's child was already pregnant with their first child; Greenland, 2003; Rosenbaum, 1984; VanderWeele et al., 2020). Propensity score matching was performed using the *MatchIt* R package (Ho et al., 2011) with exact matching on gender combined with Mahalanobis distance matching on the propensity score. In total, four matchings were performed; two per sample (LISS; HRS) and two per control group (parents but not grandparents; nonparents). We matched 1:1 with replacement because of the relatively small pools of available non-grandparent controls. This meant that control observations were allowed to be used multiple times for matching (i.e., duplicated in the analysis samples[13]). We did not specify a caliper because our goal was to find matches for all grandparents, and because we achieved satisfactory covariate balance this way.

We evaluated the matching procedure in terms of covariate balance and, graphically, in terms of overlap of the distributions of the propensity scores and (non-categorical) covariates (Stuart, 2010). Covariate balance as indicated by the standardized difference in means between the grandparent and the controls after matching was satisfactory (see Tables S2 & S3) lying below 0.25 as recommended in the literature (Stuart, 2010). Graphically, differences between the distributions of the propensity score and the covariates were also small and indicated no missing overlap (see Fig. SX).

After matching, each matched control observation received the same value as their matched grandparent in the *time* variable describing the temporal relation to treatment, and the control subject's other longitudinal observations were centered around this matched observation. Thereby, we coded a counterfactual transition time frame for each control subject. Due to left- and right censored longitudinal data (i.e., panel entry or attrition), we restricted the final analysis samples to six years before and six years after the transition as shown in Table S1. We analyzed unbalanced panel data where not every participant provided all person-year observations. The final LISS analysis samples, thus, contained 250 grandparents with 1368 longitudinal observations, matched with 250 control subjects with either 1257 (parent control group) or 1355 longitudinal observations (nonparent control group). The final HRS analysis samples contained 846 grandparents with 2262 longitudinal observations, matched with 846 control subjects with either 2091 (parent control group) or 2039 longitudinal observations (nonparent control group; see Table S1. In the HRS, there were a few additional missing values in the outcomes ranging from 13 to 53 longitudinal

[13] In the LISS data, 250 grandparent observations were matched with 250 control observations; these control observations corresponded to 186 unique person-year observations stemming from 130 unique participants for the parent control group, and to 174 unique person-year observations stemming from 107 unique participants for the nonparent control group. In the HRS data, 846 grandparent observations were matched with 846 control observations; these control observations corresponded to 568 unique person-year observations stemming from 482 unique participants for the parent control group, and to 485 unique person-year observations stemming from 401 unique participants for the nonparent control group.

observations which will be listwise deleted in the respective analyses.

Analytical Strategy

Our design can be referred to as an interrupted time-series with a "nonequivalent no-treatment control group" (Shadish et al., 2002, p. 182) where treatment, that is, the transition to grandparenthood, is not deliberately manipulated.

First, to analyze mean-level changes, we used linear piecewise regression coefficients in multilevel regression models with person-year observations nested within participants and households (Hoffman, 2015). To model change over time in relation to the birth of the first grandchild, we coded three piecewise regression coefficients: a *before-slope* representing linear change in the years leading up to the transition to grandparenthood, an *after-slope* representing linear change in the years after the transition, and a *jump* coefficient shifting the intercept directly after the transition was first reported, thus representing sudden changes that go beyond changes already modeled by the *after-slope* (see Table @ref(tab:piecewise-coding-scheme for the coding scheme of these coefficients; Hoffman, 2015). Other studies of personality development have recently adopted similar piecewise growth-curve models (e.g., Bleidorn & Schwaba, 2018; Krämer & Rodgers, 2020; Schwaba & Bleidorn, 2019; van Scheppingen & Leopold, 2020).

All effects of the transition to grandparenthood on the Big Five and life satisfaction were modeled as deviations from patterns in the matched control groups by interacting the three piecewise coefficients with the binary treatment variable (0 = *control*, 1 = *grandparent*). In additional models, we interacted these coefficients with the binary moderator variables resulting in two- or three-way interactions. To test differences in the growth parameters between two groups in cases where these differences were represented by multiple fixed-effects coefficients, we defined linear contrasts using the *linearHypothesis* command from the *car* R package (Fox & Weisberg, 2019). All models of mean-level changes were estimated using maximum likelihood and included random intercepts but no random slopes of the piecewise regression coefficients.

Second, to assess interindividual differences in intraindividual change in the Big Five and life satisfaction we added random slopes to the models assessing mean-level changes (see Denissen et al., 2019 for a similar approach). In other words, we allowed for differences between individuals in their trajectories of change to be modeled, that is, differences in the *before-slope*, *after-slope*, and *jump* coefficients. Because multiple simultaneous random slopes are often not computationally feasible, we added random slopes one at a time and used likelihood ratio test to determine whether the addition of the respective random slope led to a significant improvement in model fit. We plotted distributions of random slopes (for a similar approach, see Denissen et al., 2019; Doré & Bolger, 2018). To statistically test differences in the random slope variance between the grandparent group and each control group, we respecified the multilevel models as multi-group latent growth curve models (LGCM; Preacher et al., 2008) using the *lavaan* R package (Rosseel, 2012). Next, we tested a LGCM with an equality constraint on the grandparents' and control groups' variances of the latent slope against an unconstrained LGCM. This was also done separately for the parent and nonparent control groups.

Third, to examine rank-order stability in the Big Five and life satisfaction over the transition to grandparenthood, we computed the test-retest correlation of measurements prior to the transition to grandparenthood (at the time of matching) with the first available measurement after the transition. To test the difference in test-retest stability between grandparents and either of the control groups, we then entered the pre-treatment measure as well as the treatment variable (0 = *control*, 1 = *grandparent*) and their interaction into multiple regression models predicting the Big Five and life satisfaction. The interaction tested for significant differences in the test-retest stability between those who experienced the transition to grandparenthood and those who did not (for a similar approach, see Denissen et al., 2019; McCrae, 1993).

We used R (Version 4.0.4; R Core Team, 2021) and the R-packages *lme4* (Version

1.1.26; Bates et al., 2015), and *lmerTest* (Version 3.1.3; Kuznetsova et al., 2017) for multilevel modeling, as well as *tidyverse* (Wickham et al., 2019) for data wrangling, and *papaja* (Aust & Barth, 2020) for reproducible manuscript production. Additional modeling details and a list of all software we used is provided in the Supplemental Material. In line with Benjamin et al. (2018), we set the α-level for all confirmatory analyses to .005.

Results

Discussion

Based on

- personality maturation cross-culturally: (Bleidorn et al., 2013; Chopik & Kitayama, 2018)
- facets / nuances (Mõttus & Rozgonjuk, 2021)
- arrival of grandchild associated with retirement decisions (Lumsdaine & Vermeer, 2015); pers X WB interaction over retirement (Henning et al., 2017);
- Does the Transition to Grandparenthood Deter Gray Divorce? A Test of the Braking Hypothesis (Brown et al., 2021)
- prolonged period of grandparenthood? (Margolis & Wright, 2017)
- subjective experience of aging (Bordone & Arpino, 2015)
- policy relevance of personality (Bleidorn et al., 2019), e.g., health outcomes (Turiano et al., 2012), but not really evidence for healthy neuroticism (Turiano et al., 2020)
- mortality & grandparenthood(Christiansen, 2014); moderated by race? (Choi, 2020); but see HRS -> "Grandparenthood overall was unassociated with mortality risk in both women and men" (Ellwardt et al., 2021) –> (Hilbrand et al., n.d.): "Survival analyses based on data from the Berlin Aging Study revealed that mortality hazards

for grandparents who provided non-custodial childcare were 37% lower than for grandparents who did not provide childcare and for non-grandparents. These associations held after controlling for physical health, age, socioeconomic status and various characteristics of the children and grandchildren.”

- “Older grandparents tended to provide financial assistance and more strongly identified with the role. When their grandchildren were younger, grandparents tended to interact more with them, share more activities, provide baby-sitting, and receive more symbolic rewards from the grandparent role.” (Silverstein & Marenco, 2001)

- “refutes the central claim of role theory according to which salient roles are more beneficial to the psychological well-being of the individual than are other roles, especially in old age. It also questions the theoretical framework of grandparent role meaning that is commonly cited in the literature” (Muller & Litwin, 2011) –> see also (Condon et al., 2019): First-Time Grandparents’ Role Satisfaction and Its Determinants

- “maternal grandmothers tend to invest the most in their grandchildren, followed by maternal grandfathers, then paternal grandmothers, with paternal grandfathers investing the least“ -> also: call for causally informed designs! (Coall & Hertwig, 2011) –> discusses grandparental role investment from an evolutionary perspective

- factors determining grandparental investement: (Coall et al., 2014)

- relation to well-being: (Danielsbacka & Tanskanen, 2016)

- “Over the last two decades, the share of U.S. children under age 18 who live in a multigenerational household (with a grandparent and parent) has increased dramatically“ (Pilkauskas et al., 2020)

- differences in Big Five assessment: HRS adjectives vs. LISS statements

Limitations

Despite

Conclusions

Our

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Supplemental Material

Supplemental Tables

Table S1

Longitudinal sample size in the analysis samples and coding scheme for the piecewise regression coefficients

	Pre-transition years						Post-transition years						
	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6
LISS: Analysis samples													
Grandparents: obs.	92	105	108	121	156	116	133	138	108	108	69	62	52
Grandparents: % women	51.09	48.57	52.78	51.24	56.41	62.93	47.37	52.90	51.85	50.00	56.52	66.13	53.85
Parent controls: obs.	91	108	101	131	184	88	105	120	76	87	79	43	44
Parent controls: % women	61.54	49.07	55.45	51.15	56.52	53.41	55.24	52.50	57.89	51.72	56.96	60.47	50.00
Nonparent controls: obs.	89	110	96	141	181	83	116	142	84	122	105	34	52
Nonparent controls: % women	47.19	54.55	54.17	54.61	54.70	50.60	47.41	55.63	55.95	58.20	57.14	38.24	50.00
LISS: Coding scheme													
Before-slope	0	1	2	3	4	5	5	5	5	5	5	5	5
After-slope	0	0	0	0	0	0	1	2	3	4	5	6	7
Jump	0	0	0	0	0	0	1	1	1	1	1	1	1
HRS: Analysis samples													
Grandparents: obs.	162		388		461		380		444		195		232
Grandparents: % women	57.41		54.12		55.53		53.95		55.41		56.41		53.45
Parent controls: obs.	159		385		461		321		378		172		215
Parent controls: % women	54.72		54.03		55.53		54.21		56.61		57.56		60.93
Nonparent controls: obs.	170		385		461		298		352		169		204
Nonparent controls: % women	54.12		54.03		55.53		54.36		59.66		52.66		58.82
HRS: Coding scheme													
Before-slope	0		1		2		2		2		2		2
After-slope	0		0		0		1		2		3		4
Jump	0		0		0		1		1		1		1

Note. obs. = observations. *time* = 0 marks the first year where the transition to grandparenthood has been reported. The number of participants is $N_{LISS} = 250$ and $N_{HRS} = 846$.

Table S2

Standardized Difference in Means for Covariates Used in Propensity Score Matching and the Propensity Score in the LISS panel

			Parent control group		Nonparent control group	
Covariate	Description	Raw variable	Before PSM	After PSM	Before PSM	After PSM
pscore	Propensity score	/	1.14	0.01	1.34	0.01
female	Gender (f.=1, m.=0)	geslacht	0.05	0.00	0.05	0.00
age	Age	gebjaar	0.85	-0.05	4.05	-0.09
degreehighersec	Higher secondary/preparatory university education	oplmet	0.07	0.00	-0.07	0.08
degreevocational	Intermediate vocational education	oplmet	-0.20	-0.11	-0.02	0.05
degreecollege	Higher vocational education	oplmet	0.00	0.04	0.02	-0.14
degreeuniversity	University degree	oplmet	-0.08	0.15	-0.15	-0.03
religion	Member of religion/church	cr*012	0.10	0.10	0.33	0.06
speakdutch	Dutch spoken at home (primarily)	cr*089	-0.02	-0.11	0.00	0.04
divorced	Divorced (marital status)	burgstat	0.02	0.00	0.29	0.10
widowed	Widowed (marital status)	burgstat	0.09	0.05	0.13	0.12
livetogether	Live together with partner	cf*025	-0.08	-0.11	1.05	-0.02
rooms	Rooms in dwelling	cd*034	-0.03	0.02	0.63	-0.22
logincome	Personal net monthly income in Euros (logarithm)	nettoink	-0.01	0.12	0.59	-0.21
rental	Live for rent (vs. self-owned dwelling)	woning	-0.08	-0.10	-0.47	-0.08
financialsit	Financial situation of household (scale from 1-5)	ci*252	0.08	0.02	-0.03	-0.08
jobhours	Average work hours per week	cw*127	0.02	0.15	0.11	0.00
mobility	Mobility problems (walking, staircase, shopping)	ch*023/027/041	0.07	-0.12	0.09	-0.04
dep	Depression items from Mental Health Inventory	ch*011 - ch*015	-0.01	0.02	-0.22	0.03
betterhealth	Poor/moderate health status (ref.: good)	ch*004	0.00	0.01	-0.26	-0.01
worsehealth	Very good/excellent health status (ref.: good)	ch*004	0.04	-0.19	0.11	0.04
totalchildren	Number living children	cf*455 / cf*036	0.25	-0.02	NA	NA
totalresidentkids	Number of living-at-home children in household	aantalki	-0.71	0.00	NA	NA
secondkid	Has two or more children	cf*455 / cf*036	0.20	-0.01	NA	NA
thirdkid	Has three or more children	cf*455 / cf*036	0.26	0.00	NA	NA
kid1female	Gender of first child (f.=1, m.=0)	cf*068	0.04	-0.01	NA	NA
kid2female	Gender of second child (f.=1, m.=0)	cf*069	0.01	-0.06	NA	NA
kid3female	Gender of third child (f.=1, m.=0)	cf*070	0.17	-0.04	NA	NA
kid1age	Age of first child	cf*456 / cf*037	1.70	-0.12	NA	NA
kid2age	Age of second child	cf*457 / cf*038	0.87	0.00	NA	NA
kid3age	Age of third child	cf*458 / cf*039	0.40	-0.01	NA	NA
kid1home	First child living at home	cf*083	-1.56	0.11	NA	NA

Table S2 continued

			Parent control group		Nonparent control group	
Covariate	Description	Raw variable	Before PSM	After PSM	Before PSM	After PSM
kid2home	Second child living at home	cf*084	-1.05	0.03	NA	NA
kid3home	Third child living at home	cf*085	-0.05	0.01	NA	NA
swls	Satisfaction with Life Scale	cp*014 - cp*018	0.10	-0.05	0.25	0.00
agree	Agreeableness	cp*021 - cp*066	0.05	-0.03	0.13	-0.12
con	Conscientiousness	cp*022 - cp*067	-0.06	0.03	0.16	0.04
extra	Extraversion	cp*020 - cp*065	0.05	0.06	0.02	-0.10
neur	Neuroticism	cp*023 - cp*068	-0.02	-0.10	-0.26	-0.01
open	Openness	cp*024 - cp*069	0.06	0.09	-0.16	-0.05
participation	Waves participated	/	-0.27	-0.24	0.09	-0.10
year	Year of assessment	wave	-0.23	-0.15	0.08	-0.15

Note. PSM = propensity score matching, ref. = reference category, f. = female, m. = male, NA = covariate not used in this sample. The standardized difference in means between the grandparent and the two control groups (parent and nonparent) was computed by $(\bar{x}_{gp} - \bar{x}_c)/(\hat{\sigma}_{gp})$. A rule of thumb says that this measure should ideally be below .25 (Stuart, 2010).

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Table S3

Standardized Difference in Means for Covariates Used in Propensity Score Matching and the Propensity Score in the HRS

			Parent control group		Nonparent control group	
Covariate	Description	Raw variable	Before PSM	After PSM	Before PSM	After PSM
pscore	Propensity score	/	0.92	0.00	1.45	0.00
female	Gender (f.=1, m.=0)	RAGENDER	-0.07	0.00	0.01	0.00
age	Age	RABYEAR	-0.46	-0.03	-1.02	0.06
schlyrs	Years of education	RAEDYRS	0.11	0.07	0.25	-0.08
religyear	Religious attendance: yearly	*B082	0.04	0.00	0.13	-0.02
religmonth	Religious attendance: monthly	*B082	0.01	0.00	0.10	0.10
religweek	Religious attendance: weekly	*B082	0.06	0.01	0.04	0.04
religmore	Religious attendance: more	*B082	0.09	-0.08	0.06	-0.03
notusaborn	Not born in the US	*Z230	-0.05	0.06	0.13	-0.05
black	Race: black/african american (ref.: white)	RARACEM	-0.13	-0.15	-0.22	0.07
raceother	Race: other (ref.: white)	RARACEM	-0.09	-0.07	0.01	-0.09
divorced	Divorced (marital status)	R*MSTAT	-0.06	0.00	0.01	0.00
widowed	Widowed (marital status)	R*MSTAT	-0.31	0.02	-0.41	0.08
livetogether	Live together with partner	*A030 / *XF065_R	0.25	-0.04	1.05	-0.04
roomslessthree	Number of rooms (in housing unit)	*H147 / *066	-0.15	-0.10	-0.59	-0.08
roomsfourfive	Number of rooms (in housing unit)	*H147 / *066	0.00	0.04	-0.25	0.04
roomsmoreeight	Number of rooms (in housing unit)	*H147 / *066	0.07	-0.07	0.28	0.01
loghhincome	Household income (logarithm)	*ITOT	0.03	0.08	0.41	0.03
loghhwealth	Household wealth (logarithm)	*ATOTB	0.07	0.03	0.34	-0.04
renter	Live for rent (vs. self-owned dwelling)	*H004	-0.10	-0.09	-0.51	-0.03
jobhours	Hours worked/week main job	R*JHOURS	0.25	0.09	0.59	-0.02
paidwork	Working for pay	*J020	0.28	0.09	0.62	-0.02
mobilitydiff	Difficulty in mobility rated from 0-5	R*MOBILA	-0.16	-0.01	-0.52	0.02
cesd	CESD score (depression)	R*CESD	-0.13	-0.06	-0.26	-0.01
conde	Sum of health conditions	R*CONDE	-0.22	0.01	-0.51	0.04
healthexcellent	Self-report of health - excellent (ref: good)	R*SHLT	0.05	0.00	0.15	-0.02
healthverygood	Self-report of health - very good (ref: good)	R*SHLT	0.23	0.06	0.31	-0.07
healthfair	Self-report of health - fair (ref: good)	R*SHLT	-0.16	-0.05	-0.29	-0.01
healthpoor	Self-report of health - poor (ref: good)	R*SHLT	-0.07	-0.01	-0.24	0.03
totalnonresidentkids	Number of nonresident kids	*A100	0.66	-0.08	NA	NA
totalresidentkids	Number of resident children	*A099	-0.22	-0.02	NA	NA
secondkid	Has two or more children	KIDID	0.52	-0.03	NA	NA

Table S3 continued

Covariate	Description	Raw variable	Parent control group		Nonparent control group	
			Before PSM	After PSM	Before PSM	After PSM
thirdkid	Has three or more children	KIDID	0.38	-0.05	NA	NA
kid1female	Gender of first child (f.=1, m.=0)	KAGENDERBG	0.11	0.00	NA	NA
kid2female	Gender of second child (f.=1, m.=0)	KAGENDERBG	0.17	0.01	NA	NA
kid3female	Gender of third child (f.=1, m.=0)	KAGENDERBG	0.24	0.05	NA	NA
kid1age	Age of first child	KABYEARBG	-0.35	-0.06	NA	NA
kid2age	Age of second child	KABYEARBG	0.36	-0.06	NA	NA
kid3age	Age of third child	KABYEARBG	0.35	-0.05	NA	NA
kid1educ	Education of first child (years)	KAEDUC	0.30	0.05	NA	NA
kid2educ	Education of second child (years)	KAEDUC	0.57	-0.01	NA	NA
kid3educ	Education of third child (years)	KAEDUC	0.40	-0.03	NA	NA
childrenclose	Children live within 10 miles	*E012	0.14	0.02	NA	NA
siblings	Number of living siblings	R*LIVSIB	0.05	-0.08	0.21	0.04
swls	Satisfaction with Life Scale	*LB003*	0.17	0.05	0.30	0.05
agree	Agreeableness	*LB033*	0.06	0.00	0.11	0.06
con	Conscientiousness	*LB033*	0.14	-0.02	0.26	0.00
extra	Extraversion	*LB033*	0.04	-0.04	0.18	0.08
neur	Neuroticism	*LB033*	-0.06	0.01	-0.04	0.03
open	Openness	*LB033*	0.04	0.10	0.05	0.04
participation	Waves participated (2006-2018)	/	-0.36	0.00	-0.26	-0.05
interviewyear	Date of interview - year	*A501	-0.33	-0.03	-0.18	-0.07

Note. PSM = propensity score matching, ref. = reference category, f. = female, m. = male, NA = covariate not used in this sample. The standardized difference in means between the grandparent and the two control groups (parent and nonparent) was computed by $(\bar{x}_{gp} - \bar{x}_c)/(\hat{\sigma}_{gp})$. A rule of thumb says that this measure should ideally be below .25 (Stuart, 2010).

Supplemental Figures

Complete Software and Session Information

We used R (Version 4.0.4; R Core Team, 2021) and the R-packages *car* (Version 3.0.10; Fox et al., 2020a, 2020b; Yentes & Wilhelm, 2018), *carData* (Version 3.0.4; Fox et al., 2020b), *careless* (Version 1.1.3; Yentes & Wilhelm, 2018), *citr* (Version 0.3.2; Aust, 2019), *corrplot2017* (Wei & Simko, 2017), *cowplot* (Version 1.1.0; Wilke, 2020), *dplyr* (Version 1.0.2; Wickham, François, et al., 2020), *effects* (Version 4.2.0; Fox & Weisberg, 2018; Fox, 2003; Fox & Hong, 2009), *forcats* (Version 0.5.0; Wickham, 2020a), *foreign* (Version 0.8.81; R Core Team, 2020), *ggplot2* (Version 3.3.3; Wickham, 2016), *GPArotation* (Version 2014.11.1; Bernaards & I.Jennrich, 2005), *interactions* (Version 1.1.3; Long, 2019), *jtools* (Version 2.1.1; Long, 2020), *knitr* (Version 1.30; Xie, 2015), *lme4* (Version 1.1.26; Bates et al., 2015), *lmerTest* (Version 3.1.3; Kuznetsova et al., 2017), *magick* (Version 2.6.0; Ooms, 2021), *MatchIt* (Version 4.1.0; Ho et al., 2020), *Matrix* (Version 1.3.2; Bates & Maechler, 2021), *papaja* (Version 0.1.0.9997; Aust & Barth, 2020), *patchwork* (Version 1.1.0.9000; Pedersen, 2020), *png* (Version 0.1.7; Urbanek, 2013), *psych* (Version 2.0.9; Revelle, 2020), *purrr* (Version 0.3.4; Henry & Wickham, 2020), *readr* (Version 1.4.0; Wickham & Hester, 2020), *robustlmm* (Version 2.3; Koller, 2016), *scales* (Version 1.1.1; Wickham & Seidel, 2020), *stringr* (Version 1.4.0; Wickham, 2019), *tibble* (Version 3.0.4; Müller & Wickham, 2020), *tidyr* (Version 1.1.2; Wickham, 2020b), *tidyverse* (Version 1.3.0; Wickham, Averick, et al., 2019), and *tinylabels* (Version 0.1.0; Barth, 2020) for data wrangling, analyses, and plots.

The following is the output of R's *sessionInfo()* command, which shows information to aid analytic reproducibility of the analyses.

R version 4.0.4 (2021-02-15) Platform: x86_64-apple-darwin17.0 (64-bit) Running under: macOS Big Sur 10.16

Matrix products: default BLAS: /Library/Frameworks/R.framework/Versions/4.0/Resources/lib/libRblas.dylib LAPACK:

/Library/Frameworks/R.framework/Versions/4.0/Resources/lib/libRlapack.dylib

locale: [1] en_US.UTF-8/en_US.UTF-8/en_US.UTF-8/C/en_US.UTF-8/en_US.UTF-8

attached base packages: [1] stats graphics grDevices utils datasets methods base

other attached packages: [1] forcats_0.5.0 stringr_1.4.0 dplyr_1.0.2 purrr_0.3.4
[5] readr_1.4.0 tidyr_1.1.2 tibble_3.0.4 ggplot2_3.3.3
[9] tidyverse_1.3.0 citr_0.3.2 papaja_0.1.0.9997 tinylabels_0.1.0

loaded via a namespace (and not attached): [1] Rcpp_1.0.6 lattice_0.20-41 lubridate_1.7.9.2
[4] psych_2.0.9 assertthat_0.2.1 digest_0.6.27
[7] mime_0.9 R6_2.5.0 cellranger_1.1.0
[10] backports_1.2.0 reprex_0.3.0 evaluate_0.14
[13] httr_1.4.2 pillar_1.4.7 rlang_0.4.9
[16] readxl_1.3.1 rstudioapi_0.13 miniUI_0.1.1.1
[19] blob_1.2.1 rmarkdown_2.5 munsell_0.5.0
[22] shiny_1.5.0 broom_0.7.6 GPArotation_2014.11-1 [25] compiler_4.0.4 httpuv_1.5.4 modelr_0.1.8
[28] xfun_0.19 pkgconfig_2.0.3 base64enc_0.1-3
[31] mnormt_2.0.2 tmvnsim_1.0-2 htmltools_0.5.0
[34] tidyselect_1.1.0 bookdown_0.21 fansi_0.4.1
[37] withr_2.3.0 crayon_1.3.4 dbplyr_1.4.4
[40] later_1.1.0.1 grid_4.0.4 nlme_3.1-152
[43] jsonlite_1.7.1 xtable_1.8-4 gtable_0.3.0
[46] lifecycle_0.2.0 DBI_1.1.0 magrittr_2.0.1
[49] scales_1.1.1 cli_2.2.0 stringi_1.5.3
[52] fs_1.5.0 promises_1.1.1 xml2_1.3.2

[55] ellipsis_0.3.1 generics_0.1.0 vctrs_0.3.5

[58] tools_4.0.4 glue_1.4.2 hms_0.5.3

[61] parallel_4.0.4 fastmap_1.0.1 yaml_2.2.1

[64] colorspace_2.0-0 rvest_0.3.6 knitr_1.30

[67] haven_2.3.1

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