Religious Upbringing and Adult Human Development: Longitudinal Evidence from 22 Countries

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Abstract

Does religious upbringing shape adult human development, or do observed differences primarily reflect continued participation later in life? I study this question using three waves of the Global Flourishing Study, covering 202,898 adults across 21 countries and Hong Kong. The data link religious-service attendance around age 12 to detailed measures of childhood conditions, current religious activity, and repeated adult outcomes. Childhood attendance is associated with higher adult flourishing, health, meaning, relationships, character, and selected economic outcomes, although these differences narrow substantially after accounting for family background and parental religiosity. Life-course comparisons sharpen the interpretation. Adults whose frequent participation ended after childhood display no broad advantage, while adults who currently participate frequently have similar profiles whether or not they attended frequently as children. Continued participation from childhood is associated with a modest additional advantage in aggregate nonfinancial flourishing, health, relationships, and some economic outcomes. These activation contrasts retain the same sign across contemporaneous, prospective, and within-person specifications, but they are small relative to the broader gap associated with current participation and cannot be separated from selection into persistence or correlated recall of childhood attendance. Individual fixed-effects estimates further show that income and flourishing move together within person, but childhood religious formation does not systematically moderate this relationship. Thus, religion functions as a continuing life-course institution rather than an independently identified childhood treatment.

Keywords: religion; childhood development; human capital; flourishing; income; panel data.
JEL Codes: J13, J24, I31, Z12.

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1 Introduction

Adult health, earnings, behavior, and well-being reflect investments and environments that begin long before labor-market entry.[1] Experimental and quasi-experimental studies show that enriched childhood environments can affect adult health, employment, earnings, social behavior, and outcomes through not only short-run gains, but also dynamic complementarities (Cunha et al., 2010; Heckman et al., 2013; Campbell et al., 2014; García et al., 2020, 2023).

Religious upbringing is a common but comparatively understudied part of that investment environment. For generations, theologians, developmental psychologists, and social scientists have debated whether religion promotes moral development or instead transmits norms whose content and consequences depend on family and social context. Religious families can organize repeated routines, parent–child conversations, relationships with nonparental adults, service to others, self-regulation, and ideas about obligation, purpose, and adversity. The same practices can also carry psychological control, exclusion, conflict, or rigid social boundaries. Reviews of religion and family life find small and heterogeneous associations with parenting and child adjustment rather than an unqualified advantage (Mahoney et al., 2001; Hardy et al., 2019). More focused developmental studies show that religious communities can shape the language through which children reason about autonomy and divinity, the scope they assign to religious norms, and the meaning of particular parenting practices (DiBianca Fasoli, 2018; Srinivasan et al., 2019; Davidov et al., 2021). The relevant economic object is therefore not belief alone. It is a bundle of repeated

[1]The economics of human development formalizes this process through self-productivity and dynamic complementarity: early capabilities raise the productivity of later investments, while developmental technologies and the skills they produce may vary across stages (Heckman, 2006; Heckman et al., 2006; Cunha and Heckman, 2007; Cunha et al., 2010; Heckman and Mosso, 2014; Heckman and Zhou, 2026; Heckman et al., 2026b,a).

inputs into the formation of social, emotional, moral, and meaning-related capabilities.

The economics of religion has studied several parts of this bundle, but usually after childhood. Religious organizations provide club goods, informal insurance, social networks, and norms that can shape labor supply, risk sharing, political preferences, and economic attitudes (Iannaccone, 1998; Dehejia et al., 2007; Chen, 2010; Guiso et al., 2003; Iyer, 2016). Historical evidence links Protestantism and missions to literacy and human-capital transmission, though results vary across settings and need not operate through doctrine itself (Becker and Woessmann, 2009; Valencia Caicedo, 2019). Contemporary causal studies likewise show that religious participation can respond to shocks and interventions and can affect some economic outcomes without moving others (Bentzen, 2019; Bryan et al., 2021). Most recently, Kashner and Pienkny (2026) distinguish access to congregational networks from the force of religious beliefs using pastor deaths and convictions in the Southern Baptist Convention. They find that both shocks reduce labor-force participation and raise unemployment, while credibility shocks also shift voting and opioid mortality; nearby housing values decline after both forms of disruption. While Kashner and Pienkny (2026) illustrate the role of congregational networks and norms, an open question is whether religious institutions enter the production of childhood human capital and shape later-in-life outcomes.

This paper examines that question using the Global Flourishing Study (GFS), a longitudinal study that begins with 202,898 adults across 21 countries and Hong Kong (Padgett et al., 2025; VanderWeele et al., 2025; Lomas et al., 2025). The survey records religious-service attendance around age 12, parental attendance, childhood affiliation, family financial conditions, health, parental structure, relationships with mothers and fathers, felt love, outsider status, and abuse. It also measures six domains of adult flourishing, education, employment, household income, current religious participation, volunteering, charitable giving, and assistance to strangers. Up to

three annual waves provide repeated adult outcomes, including financial well-being (Bialowolski et al., 2025). The combination permits three complementary analyses: an adult outcome profile, a comparison of childhood and adult religious trajectories, and a within-person test of whether childhood formation changes the relationship between material resources and well-being.

The first analysis treats religious-service attendance around age 12 as a marker of religious formation and estimates its association with adult human-development outcomes. The comparison is within country-by-five-year-birth-cohort cells and uses annual survey weights. I add observed childhood conditions in stages: finances, health, family structure, migration background, parental relationships and felt love, and parental religious attendance. Moving from never attending around age 12 to weekly attendance is associated with 0.244 standard deviations higher nonfinancial flourishing, including differences of 0.256 standard deviations in meaning and purpose, 0.206 in character, 0.172 in close relationships, and 0.169 in health. The corresponding differences are 2.3 percentage points in within-country income rank, 3.1 points in employment, and 5.0 points in college completion. Conditioning on the full childhood vector reduces the nonfinancial coefficient by 47 percent, to 0.129 standard deviations. The fully adjusted estimates remain largest for meaning and purpose (0.148) and character (0.132); they equal 0.089 for relationships and 0.070 for health. The economic estimates become 1.0 percentile point for income rank, 3.4 percentage points for employment, and 1.5 points for college completion. The attenuation is evidence that religious upbringing is substantially bundled with family and developmental circumstances, recognizing that the residual need not reflect only a causal direct effect.

Relative to adults who attended infrequently in both childhood and adulthood, childhood-and-current weekly participants have nonfinancial flourishing that is 0.277 standard deviations higher, while adults who became weekly participants differ by 0.234 standard deviations. Adults

who attended weekly only in childhood differ by 0.002 standard deviations. The broad profile is therefore concentrated among current participants, but it is not uniquely concentrated among those exposed in childhood: the childhood-and-current–adult-only point difference is 0.043 standard deviations. The same pattern appears across health, meaning, relationships, and character. These comparisons weigh against a broad durable association of childhood attendance after participation ends. The activation contrasts are positive and modest: they are statistically significant for the aggregate nonfinancial index, health, and relationships, and the nonfinancial estimate retains the same positive sign in contemporaneous, prospective, and within-person specifications. They are economically small relative to the common gap associated with current participation and cannot be separated from selection into persistent participation or correlated recall of childhood attendance.

The second analysis asks whether childhood religious formation changes how adult flourishing moves with material resources. A stylized capability framework distinguishes consumption from nonmaterial capabilities accumulated through childhood. If the two are substitutes in adult well-being production, people with greater stocks of nonmaterial capability should have a flatter income–well-being gradient. Using individual and country-by-wave fixed effects, identification comes from changes in a respondent's position across country-specific income brackets, net of common changes in the same study site and wave. A doubling of annualized household income is associated with a 0.066-point increase in nonfinancial flourishing on its 0–10 scale. The corresponding changes are 0.094 points for hedonic well-being and 0.076 points for meaning and purpose.

The proposed slope-shifting implication is not supported, although it may be due to a lack of variation. The interaction between log income and childhood religious formation is -0.003 with a standard error of 0.012. The implied income coefficient is 0.097 among respondents who never attended and 0.094 among weekly childhood attendees; for a doubling of income, the difference is

only −0.002 points. The interaction remains near zero among respondents with stable employment, after adding income interactions with the full set of observed childhood conditions, when formation is coded as a weekly indicator or a family religious environment, and when income is measured by within-site-wave rank, ordinal category, inverse hyperbolic sine, alternative top-code rules, or subjective adequacy. Domain-specific interactions are likewise close to zero except for the financial-stability domain, whose items are mechanically proximate to the income regressor.

The cross-country evidence points in the same direction. Site-specific interaction estimates range from −0.160 in Nigeria to 0.416 in Hong Kong, but no individual 95 percent confidence interval excludes zero. Twelve estimates are negative and ten are positive. An inverse-variance summary is −0.022 (standard error 0.013; 95 percent confidence interval $[-0.049, 0.004]$), with no estimated between-site variance beyond sampling error. A positive relation between the absolute interaction and observed selection into weekly childhood attendance is driven by Hong Kong and disappears when that site is excluded. The data therefore do not reveal a stable secularization or selection pattern in the income-slope result.

The paper contributes at the intersection of the economics of religion and the economics of human development. Most economic research on religion studies contemporaneous adult belief, participation, or shocks to religious institutions, emphasizing club goods, informal insurance, social networks, norms, and economic behavior (Iannaccone, 1998; Dehejia et al., 2007; Chen, 2010; Iyer, 2016; Bryan et al., 2021). Recent quasi-experimental work provides evidence that disruptions to congregational access and credibility propagate to local labor markets, voting, health, and housing (Kashner and Pienkny, 2026), while adult religious engagement is associated with more stable subjective well-being over the business cycle (Makridis et al., 2021). Historical studies also link religious institutions to literacy and human-capital transmission (Becker and Woessmann,

2009; Valencia Caicedo, 2019). I shift the unit of analysis to religious upbringing as a recurring childhood institution and distinguish childhood-only, adult-only, and persistent participation within a common measurement framework across 22 societies.

For the economics of human development, models of self-productivity and dynamic complementarity emphasize that early environments shape multidimensional capabilities and the productivity of later investments (Heckman, 2006; Cunha and Heckman, 2007; Cunha et al., 2010; Heckman and Mosso, 2014). Religious upbringing is a plausible developmental input because congregations and religious households bundle family routines, adult relationships, peer networks, moral instruction, and meaning-making (Mahoney, 2010; Hardy et al., 2019). Prior studies link childhood religious exposure to child development, later health and well-being, and young-adult moral formation (Bartkowski et al., 2008; Chen and VanderWeele, 2018; Smith, 2023). Closely related Wave 1 GFS research provides a cross-national benchmark on the associations between childhood conditions, including religious attendance around age 12, and adult financial well-being (Bialowolski et al., 2025). This paper examines a broader adult capability profile, separates childhood-only, adult-only, and persistent participation, and uses the panel to estimate within-person income–flourishing comovement and test whether childhood formation shifts that gradient. The 47 percent attenuation under childhood controls, the absence of a broad childhood-only profile, and the similarity of childhood-and-current and adult-only profiles indicate that religious upbringing is bundled with family circumstances and later participation rather than operating as a separable childhood input.

The empirical design does not resolve selection into religious upbringing. Childhood religion and family experiences are recalled retrospectively in the same survey, so unobserved family type, current well-being, and mood-congruent recall may contribute to the observed adult profile. Repeating time-invariant childhood measures across person–wave observations improves precision

but creates no within-person identifying variation for those associations. Individual fixed effects remove stable person-specific confounding from the income analysis, but income changes can still reflect health, employment, marriage, and other household shocks; income is also categorical and the panel contains only two or three waves. I therefore interpret the estimates as evidence on life-course patterns and within-person comovement, not as causal returns to religious upbringing.

The remainder of the paper proceeds as follows. Section 2 presents the capability-formation framework. Section 3 describes the GFS measures and empirical designs. Section 4 reports the adult profile, continuity, and panel results. Section 5 evaluates alternative samples, measures, and timing specifications. Section 6 concludes.

2 Religious Upbringing and Human Capital Production

The framework treats religious upbringing as one input into a broader childhood production process. It is not a structural model of religious choice, and the empirical analysis does not estimate its primitives. Its purpose is to distinguish three objects that are often combined: childhood inputs, adult continuity, and the interaction between material resources and nonmaterial capabilities.

2.1 A dynamic childhood production process

Let K_{is} denote the vector of capabilities held by child i at developmental stage s. The vector can include cognitive skills, health, self-regulation, social competence, moral identity, relationships, and the ability to construct purpose or meaning. Capabilities evolve according to

$$K_{i,s+1} = \Phi_s(K_{is}, I_{is}, R_{is}, F_{is}, \eta_{is}) \,, \tag{1}$$

where I_{is} denotes conventional investments such as parental time, material resources, and schooling; R_{is} denotes the child's religious environment; F_{is} denotes other family and community conditions; and η_{is} collects unmeasured shocks.

The term R_{is} is intentionally broader than an individual's belief, or perceptions. It can include service attendance, parent–child conversations, religious instruction, relationships with adults and peers, behavioral expectations, volunteering, prayer, ritual, and interpretations of suffering or obligation. The GFS measure of service attendance around age 12 is therefore a proxy for intensity of exposure, not a complete measure of the underlying input. The net marginal product of R_{is} is not assumed to be positive. A religious environment may support social and moral capabilities, but it may also introduce conflict, exclusion, or psychological control. Its content and its complementarity with family relationships can vary across traditions and societies.

The human-development literature emphasizes that the productivity of an input depends on the capability stock already accumulated. For a conventional investment I_{is}, dynamic complementarity can be written schematically as

$$\frac{\partial^2 \Phi_s}{\partial K_{is} \partial I_{is}} > 0. \tag{2}$$

An analogous interaction may apply to R_{is}: religious routines or instruction may have different consequences depending on prior language, self-regulation, parental warmth, or social trust. This feature matters for interpretation. The coefficient on childhood attendance is not the productivity of a separable input. It combines the content of religious exposure, the prior capability stock, complementary family inputs, and selection into the setting. Sequentially adding childhood controls in the empirical analysis therefore measures overlap with this bundle; it does not recover a direct production parameter, which would require plausibly exogenous variation and a structural model.

Adult outcome d is generated by the accumulated capability stock and the adult environment:

$$Y_{id} = H_d(K_{iA}, A_i, X_{iA}, \nu_{id}) \,, \tag{3}$$

where A_i denotes adult religious participation, X_{iA} denotes adult material and social conditions, and ν_{id} is an outcome-specific disturbance. Equation (3) clarifies why the adult profile alone cannot establish a childhood effect. Childhood religious formation may affect K_{iA}, but it may also predict adult participation, family persistence, or other unobserved inputs. The continuity analysis is therefore a timing diagnostic rather than an identified decomposition.

2.2 Material resources and nonmaterial capabilities

A complementary question concerns how accumulated nonmaterial capabilities relate to adult material resources. Let adult flourishing be

$$W_{it} = f(C_{it}, M_i), \qquad C_{it} = c(y_{it}), \tag{4}$$

where consumption C_{it} increases with household income y_{it} and M_i is a durable stock of nonmaterial capability. Childhood religious formation may contribute to M_i, but M_i also reflects family inputs, personality, education, adult religion, and other experiences.

Differentiating Equation (4) with respect to log income gives

$$\frac{\partial W_{it}}{\partial \log y_{it}} = f_C(C_{it}, M_i) c'(y_{it}) y_{it}. \tag{5}$$

If consumption and nonmaterial capability are substitutes in the production of flourishing,

$$f_{CM}(C, M) < 0, \tag{6}$$

then the marginal flourishing return to income declines with M_i. If childhood religious formation is positively associated with M_i, the empirical income-by-formation interaction should be negative. Complementarity, $f_{CM} > 0$, implies the opposite sign.

This is the framework's only signed panel prediction. In the estimating equation

$$W_{ict} = \beta x_{ict} + \theta \left(x_{ict} \times R_i^{\text{child}} \right) + \alpha_i + \delta_{ct} + \varepsilon_{ict}, \tag{7}$$

x_{ict} is log household income, α_i is an individual fixed effect, and δ_{ct} is a country-by-wave fixed effect. Under the substitution interpretation, $\theta < 0$. The coefficient is not a structural estimate of f_{CM}. Childhood attendance is an imperfect proxy for M_i; adult religious participation may change over time; and income shocks may differ systematically by childhood background. A null estimate can therefore mean either that the average cross-partial is small or that heterogeneity and measurement prevent the reduced-form interaction from capturing it.

3 Data, Measurement, and Empirical Design

3.1 The Global Flourishing Study panel

The Global Flourishing Study is an annual longitudinal survey designed to measure multiple dimensions of human flourishing in nationally representative samples (Padgett et al., 2025; VanderWeele

et al., 2025; Lomas et al., 2025). The initial cohort contains 202,898 adults in 21 countries and Hong Kong, which I treat as 22 study sites. Wave 1 was fielded in 2022–2023, and the release used here contains up to three annual observations per respondent. Retrospective childhood measures are time invariant and therefore repeat on each observed wave, while adult flourishing, income, employment, marital status, and religious participation can vary over time.

The GFS uses site-specific sampling frames and provides annual survey weights. I normalize those weights to mean one within site and wave. Cross-sectional adult-profile regressions use the annual weights and cluster standard errors by respondent because the same childhood exposure appears on multiple person–wave records. Wave-1 analyses of recalled childhood conditions use the Wave-1 weight. The panel regressions are unweighted in the primary specification because person fixed effects identify the coefficients from within-person changes and longitudinal weights are not consistently available for every wave in the release. Appendix Table A.2 compares weighted and unweighted specifications where the design permits them.

The sample size varies with the outcome and control set. The baseline adult-profile estimates use between 395,333 and 428,576 person–wave observations; the fully adjusted estimates use between 296,076 and 318,194. The principal within-person income regression uses 324,447 observations, and the income-by-childhood-formation specification uses 321,963. Across the 22 study-site slope regressions, 92,091 respondents report a change in income category across waves and provide the clearest identifying variation for the interaction estimates. Table 1 reports summary statistics for Wave-1 respondents, respondents observed in at least two outcome waves, and income movers.

Figure 1 documents weekly religious-service attendance around age 12 across study sites and five-year birth cohorts. The cross-site variation is substantial, ranging from uncommon exposure in Japan and Sweden to a majority of respondents in several African and Southeast Asian sites.

Across cohorts, recalled weekly attendance shows a modest decline among the earliest birth cohorts followed by a comparably large or larger rise among later cohorts. Nonetheless, I recognize that childhood religion is endogenous: families select into religious participation, and both the content and meaning of attendance differ across societies and generations.

3.2 Childhood religious formation and family environment

The principal childhood measure is `SVCS_12YRS`, which asks how often the respondent attended religious services or worshiped at a temple, mosque, shrine, church, or other religious building around age 12. The four responses are at least weekly, one to three times per month, less than monthly, and never. I map the responses to

$$R_i^{\text{child}} = \frac{4 - \texttt{SVCS_12YRS}_i}{3}, \tag{8}$$

so that the scale equals 0 for never, 1/3 for less than monthly, 2/3 for one to three times per month, and 1 for weekly attendance. A coefficient on this scale therefore compares weekly with never attendance. Robustness specifications use a weekly indicator, a monthly-or-more indicator, the original categories, and a family religious-environment index that averages the respondent's and parents' attendance when at least two components are observed. Parental attendance comes from `SVCS_MOTHER` and `SVCS_FATHER`; childhood affiliation comes from `REL1`.

The observed childhood environment includes subjective family finances around age 12, childhood health, whether the parents were married to one another, foreign-born status, relationships with each parent, whether the respondent felt loved by each parent, whether the respondent felt like an outsider in the family, and reported physical or sexual abuse. The abuse measure enters only

the adverse-childhood index; it is not included as a separate covariate in the adult-profile control vector, the propensity-score model, or the observed-selection index. The exact variable names and codebook wording appear in Appendix Table A.1. I construct a supportive-childhood index from health, parental structure, parental relationships, felt love, and outsider status, and an adversity index from the reverse-coded components plus difficult childhood finances and abuse. Each index is standardized within study site. These retrospective reports are analytically valuable but subject to common recall and mood-congruent measurement error because childhood religion and childhood experiences are reported in the same adult survey.

3.3 Adult human-development outcomes

The GFS measures six flourishing domains with two 0–10 items each. Hedonic well-being averages happiness and life satisfaction. Health averages self-rated mental and physical health. Meaning and purpose averages understanding one's purpose and feeling that one's activities are worthwhile. Character and virtue averages promoting good in difficult circumstances and willingness to defer present happiness for greater future happiness. Close relationships averages contentment and satisfaction with relationships. Financial and material stability averages freedom from worry about ordinary expenses and safety, food, or housing.

For the adult outcome profile, I standardize each domain within study site and wave and average the five nonfinancial domains. This scaling removes site–wave differences in response levels and makes the coefficients comparable across domains. Income rank is the respondent's position in the household-income distribution within site and wave, scaled from zero to one. Employment equals one for respondents employed by an employer or self-employed. College completion follows

the harmonized three-level education variable and equals one for completion of four years beyond high school or a four-year degree. Additional outcomes include subjective income adequacy and indicators for volunteering, helping a stranger, and donating to charity; the behavioral indicators are coded so that one denotes the activity. For the panel, I retain the original 0–10 outcome scales. The principal outcome is the mean of the five nonfinancial domains. I also report a full flourishing index including financial stability, hedonic well-being, meaning and purpose, each remaining domain, and the financial domain separately. This distinction matters because the financial items are conceptually and mechanically close to household income.

3.4 Harmonized household income

The GFS records household income in site-specific brackets; they are monthly in most sites and annual in Australia and the United States. I convert every bracket to an annual local-currency amount. For a closed interval, the representative value is the geometric midpoint. For a bottom open interval, I assign one-half of the upper bound; for a top open interval, I assign 1.5 times the lower bound. The main regressor is the log of this annualized midpoint. Site-by-wave fixed effects absorb exchange rates, inflation, and all other common differences in the level of local-currency income, so the panel coefficient is identified only from changes within respondents relative to others in the same site and wave.

The cardinal conversion is necessarily approximate. Robustness specifications use site-by-wave income rank, the ordinal bracket number, the inverse hyperbolic sine of the midpoint, alternative top-bracket multipliers of 1.25 and 2.0, and subjective income adequacy. The GFS release used here measures the number of adults in the household only in Wave 1. Equivalized income is

therefore not available longitudinally, and carrying baseline household size forward would subtract a person-specific constant that is absorbed by the individual fixed effect. I omit that algebraically redundant diagnostic and document the data limitation in Appendix Section 7.1.

3.5 Adult outcome profile

The first design studies the association between childhood religious formation and adult outcome d:

$$Y_{ict}^{d} = \tau_d R_i^{\text{child}} + X_i' \Gamma_d + \lambda_{ct} + \mu_{c,b(i)} + u_{ict}^{d}, \tag{9}$$

where λ_{ct} denotes country-by-wave fixed effects and $\mu_{c,b(i)}$ denotes country-by-five-year-birth-cohort fixed effects. Equation (9) compares adults from the same society and approximate generation who report different religious-service attendance around age 12. The identifying variation is cross-sectional, not within person. The coefficient τ_d is therefore an adjusted association.

I estimate four control sets. The first includes gender and the two fixed-effect sets. The second adds childhood finances, health, family structure, and migration background. The third adds parental relationships, felt love, and outsider status. The fourth adds maternal and paternal religious attendance. Sequential controls provide a decomposition of observed overlap, but they do not cleanly divide confounders from mechanisms. Parental attendance, for example, may capture family selection, transmit religion, or organize the environment through which development occurs. Standard errors are clustered by respondent.

3.6 Religious continuity

Let childhood-and-Wave-1 participation denote weekly attendance around age 12 and weekly attendance in Wave 1, childhood-only participation denote weekly attendance only around age 12, Wave-1-only participation denote weekly attendance only in Wave 1, and the omitted category denote less-than-weekly attendance in both periods. The adult classification is fixed from Wave 1 and repeated across the respondent's person–wave observations. Because the GFS does not observe attendance continuously between age 12 and the adult panel, the childhood-and-Wave-1 group does not establish uninterrupted participation. I estimate

$$Y_{ict}^{d} = \sum_{g \in \mathcal{G} \setminus \{0\}} \pi_{gd} \mathbf{1}\{G_i = g\} + X_i' \rho_d + \lambda_{ct} + \mu_{c,b(i)} + v_{ict}^{d}. \tag{10}$$

The contrast between childhood-only and infrequent-both respondents describes whether a childhood association remains visible among adults who were not weekly participants in Wave 1. Let CW, WO, and CO denote the childhood-and-Wave-1, Wave-1-only, and childhood-only groups. The incremental childhood association among Wave-1 weekly participants is $\pi_{CW,d} - \pi_{WO,d}$. Their difference,

$$\kappa_d = (\pi_{CW,d} - \pi_{WO,d}) - \pi_{CO,d}, \tag{11}$$

is the life-course complementarity or activation contrast. A positive κ_d means that childhood exposure is more strongly associated with the outcome among adults who attended weekly in Wave 1. Separate extensions estimate the corresponding interaction with contemporaneous adult attendance, use Wave-1 attendance to predict later-wave outcomes, and exploit within-person changes in adult attendance. All comparisons remain descriptive because adult participation is

selected and persistent unobserved family traits can affect both childhood and adult religion.

3.7 Within-person income gradients

The panel analysis begins with the average within-person income–well-being association:

$$W_{ict}^{d} = \beta_d \log y_{ict} + \alpha_i + \lambda_{ct} + \varepsilon_{ict}^{d}, \tag{12}$$

where α_i is an individual fixed effect. The identifying variation is a respondent's deviation from his or her own average income, net of the common deviation in the same country and wave. Standard errors are clustered by respondent. Individual fixed effects remove stable reporting style, childhood background, permanent income, and all time-invariant characteristics. They do not make income changes exogenous. Health deterioration, job loss, marriage, household dissolution, or other shocks can change income and well-being jointly. Equation (12) is therefore a within-person association rather than a causal income effect.

The signed comparative-static test adds the time-invariant childhood measure as a slope shifter:

$$W_{ict}^{d} = \beta_d \log y_{ict} + \theta_d \left(\log y_{ict} \times R_i^{\text{child}} \right) + \alpha_i + \lambda_{ct} + e_{ict}^{d}. \tag{13}$$

The level of R_i^{child} is absorbed by α_i. Under the substitution condition in Equation (6), the reduced-form prediction is $\theta_d < 0$. The implied income slope is β_d for respondents who never attended and $\beta_d + \theta_d$ for weekly childhood attendees. I examine whether the estimate changes among respondents with stable employment, after adding interactions between income and the full observed childhood vector, under alternative codings of formation and income, and across

flourishing domains.

3.8 Developmental and cross-country diagnostics

To describe how religious formation is bundled with other childhood inputs, I estimate

$$E_{ic}^{k} = \pi_k R_i^{\text{child}} + Z_i' \Lambda_k + \mu_{c,b(i)} + \xi_{ic}^{k} \tag{14}$$

using Wave-1 records. The coefficients show whether religious upbringing co-occurs with supportive relationships, family structure, health, or adversity. They do not identify effects of religion because family and community characteristics jointly determine the variables and all childhood measures are recalled in adulthood. As a companion design, I interact childhood religious formation with the adverse-childhood index in Equation (9). The interaction asks whether the adult association of adversity differs with religious formation. It is not an identified treatment-effect heterogeneity parameter: both variables are selected childhood characteristics, both are recalled in adulthood, and the same family environment may jointly determine them.

I also add income interactions with the childhood support and adversity indices and then with the complete childhood component vector. Because the baseline religion interaction is already near zero, these models cannot support a meaningful mediated-share calculation. They instead ask whether controlling for the measured childhood bundle reveals a materially different interaction.

Finally, I estimate Equation (13) separately by study site and plot the resulting $\widehat{\theta}_c$ estimates with 95 percent confidence intervals. I report an inverse-variance pooled estimate and the sign distribution across sites. An observed-selection index summarizes standardized differences in childhood finances, health, family structure, parental relationships, felt love, outsider status,

migration background, and gender between weekly and nonweekly childhood attendees within each site. Relating $|\widehat{\theta}_c|$ to that index is a descriptive test of whether the most selected settings generate the largest slope differences. With only 22 sites and imprecise site-level estimates, the exercise is explicitly descriptive and lacks a causal interpretation.

4 Religious Upbringing and Adult Human Development

4.1 Religious upbringing is associated with a broad adult profile

Figure 2 and Table 2 report the adult outcome profile. In the baseline specification, moving from never attending religious services around age 12 to weekly attendance is associated with 0.244 standard deviations higher nonfinancial flourishing (standard error 0.011; 95 percent confidence interval $[0.221, 0.266]$).[2] The association is broad rather than confined to a single domain: 0.256 standard deviations for meaning and purpose, 0.206 for character, 0.172 for close relationships, and 0.169 for health. The corresponding differences are 0.023 on the zero-to-one income-rank scale, 3.1 percentage points in employment, and 5.0 percentage points in college completion. All estimates in the baseline profile are statistically distinguishable from zero at the 1 percent level.

The childhood controls reduce most of the profile, but they do not eliminate it. With childhood finances, health, family structure, migration background, parental relationships, felt love, outsider status, and parental religious attendance included, the nonfinancial coefficient is 0.129 standard deviations (standard error 0.015; 95 percent confidence interval $[0.100, 0.157]$). This is a 47.1 percent reduction from the baseline estimate. The fully adjusted coefficients are 0.148 for meaning

[2]The confidence intervals reported here and in the following paragraph are calculated from the unrounded coefficient and standard-error estimates.

and purpose, 0.132 for character, 0.089 for relationships, and 0.070 for health. The income-rank and college differences fall to 0.010 and 1.5 percentage points, respectively, while the employment difference is 3.4 percentage points. The intermediate columns of Table 2 show that attenuation occurs as both the basic childhood conditions and the parental-relationship blocks enter.[3]

These coefficient paths are consistent with religious upbringing being embedded in a larger developmental environment for childhood human capital accumulation. They are not estimates of a direct effect net of all alternative childhood inputs. The remaining association may reflect unmeasured family characteristics, other features of religious communities, or parts of the developmental bundle not captured by the GFS battery. Conversely, controlling for parental religious attendance or family relationships may remove variation through which religious upbringing operates. The appropriate interpretation is therefore empirical overlap, not mediation.

Several additional adult behaviors exhibit the same broad pattern. In the fully adjusted specification, weekly rather than never childhood attendance is associated with 6.0 percentage points more volunteering, 7.4 points more help to a stranger, and 9.8 points more charitable giving. Subjective income adequacy differs by only 0.007 on its zero-to-one scale. Appendix Table A.9 reports these estimates. The behavioral profile is consistent with the social-capital and norm channels emphasized in the economics of religion, including recent evidence that congregational disruptions affect economic and social behavior (Iyer, 2016; Kashner and Pienkny, 2026).

[3]A Bonferroni adjustment for the eight profile outcomes uses a two-sided threshold of $0.05/8 = 0.00625$, approximately $|t| > 2.73$. All eight baseline estimates exceed this threshold; the smallest baseline absolute t-statistic is 7.67. In the fully adjusted specification, nonfinancial flourishing, health, meaning and purpose, close relationships, character, and employment survive the correction. Income rank and college completion do not: using the reported coefficients and standard errors, their absolute t-statistics are 2.50, compared with 8.60, 5.00, 9.87, 6.36, 9.43, and 5.67 for the six estimates that survive.

4.2 Religious formation is bundled with other childhood inputs

Table 3 turns from adult outcomes to the recalled childhood environment. A one-unit movement from never to weekly attendance is associated with a 0.214-standard-deviation higher supportive-childhood index and a 0.203-standard-deviation lower adversity index. In addition, weekly attendance is associated with 0.040 points on the zero-to-one childhood-finance scale, 0.036 points on the zero-to-one childhood-health scale, and an 8.0-percentage-point difference in having parents married to one another around age 12. The corresponding differences are 0.035 and 0.047 on the zero-to-one parental-relationship measures, and 1.6 and 3.1 percentage points for feeling loved by the mother and father. The estimate for not feeling like an outsider is 0.9 percentage point and is not statistically distinguishable from zero.

These estimates illuminate the production-function interpretation. Childhood religious participation does not appear as an isolated input. It co-occurs with parental religion, family structure, economic conditions, health, and relationships—precisely the kinds of inputs whose productivity may interact dynamically over childhood. The correlations could arise because religious participation changes family routines or networks, because particular families select into religion, or because adult respondents jointly reconstruct multiple childhood experiences. The data do not separate these channels, but they reject an interpretation of service attendance as a stand-alone treatment.

4.3 Adversity moderation is selective rather than broad

Appendix Table A.10 interacts childhood religious formation with the adverse-childhood index. The interaction is 0.010 (standard error 0.012) for nonfinancial flourishing, 0.000 for meaning and purpose, and −0.001 for character. These estimates do not support broad buffering of the adult

profile. The interaction is positive for health and close relationships, 0.029 in each case, and equals 0.008 for income rank, 0.008 for employment, and 0.011 for college completion. The domain pattern is therefore heterogeneous. Since adversity and religious formation are both recalled and neither is exogenous, the estimates describe conditional associations rather than a causal protective effect.

4.4 The broad adult profile is concentrated among current participants

Table 4 compares childhood participation with adult participation measured in Wave 1. Relative to respondents who attended less than weekly in both periods, childhood-and-Wave-1 weekly participants have nonfinancial flourishing that is 0.277 standard deviations higher, while Wave-1-only weekly participants differ by 0.234 standard deviations. The childhood-only coefficient is 0.002 (standard error 0.012). It is also close to zero for health (−0.012), meaning (0.011), and relationships (−0.001). The exceptions are character, at 0.039 sd, and employment, at 1.4 pp.

These estimates do not support a broad durable association of childhood attendance after adult participation ends. The activation contrasts are positive and modest: they are statistically significant for the aggregate nonfinancial index, health, and relationships, and the nonfinancial estimate retains the same positive sign in contemporaneous, prospective, and within-person specifications. They are economically small relative to the common gap associated with current participation and cannot be separated from selection into persistent participation or correlated recall of childhood attendance. In the life-course specification, the activation contrast in Equation (11) is 0.041 (standard error 0.016; $p = 0.011$) for nonfinancial flourishing. The corresponding contrasts are 0.048 for health (0.016), 0.024 for meaning (0.016), 0.031 for relationships (0.015), and 0.010 for character (0.016). Appendix Table A.5 reports the full set of contrasts. The contemporaneous

interaction is 0.032 (0.015); using Wave-1 adult attendance to predict Wave-2 and Wave-3 outcomes yields 0.030 (0.020); and the interaction among respondents who change weekly-attendance status is 0.025 (0.012). The contemporaneous and within-person estimates are statistically significant at the 5 percent level, whereas the prospective estimate is not. Appendix Tables A.6, A.7, and A.8 report these extensions.

This pattern admits several interpretations. Current congregational participation may supply contemporaneous relationships, routines, and meaning-related inputs. Alternatively, unmeasured traits may select adults into continued or newly adopted participation, and correlated recall of childhood attendance or current religious reporting may share response tendencies with current flourishing. The trajectory design cannot distinguish these channels. It does, however, weigh against a simple reading of the baseline coefficient as a broad adult advantage that persists after people drift away from frequent participation.

At the level of the trajectory groups, childhood-and-Wave-1 and Wave-1-only participants have lower adjusted income rank and lower college completion than the omitted group, and neither current-participation group has a statistically distinguishable employment difference. The strong nonfinancial profile among current participants is therefore not a general socioeconomic advantage; it is concentrated in meaning, relationships, health, and character. The economic activation contrasts nevertheless reveal a related but distinct pattern. The life-course activation contrast is 0.020 on the within-site income-rank scale (standard error 0.005; $p < 0.001$) and 0.049 log points for household income (standard error 0.016; $p = 0.002$); the contemporaneous childhood-by-current-attendance interaction for log income is 0.063 (standard error 0.015; $p < 0.001$). These positive contrasts coexist with lower adjusted income rank among both current-participation groups relative to the omitted group: -0.021 for childhood-and-Wave-1 weekly participants and -0.035

for Wave-1-only participants. One possibility is that early religious formation complements adult congregational networks in labor-market outcomes even when the current-participation groups have lower adjusted economic levels. Another is differential selection by family socioeconomic type or other unmeasured traits into both persistence and the relative income pattern; correlated recall of childhood attendance is an additional concern.

4.5 Within-person income changes and flourishing

Figure 3 plots residualized changes in nonfinancial flourishing across residualized log-income bins. The relationship is increasing over most of the distribution, and the paths for higher and lower childhood formation substantially overlap. The figure therefore anticipates both parts of the regression evidence: income and flourishing move together within person, but the gradient does not visibly differ by childhood formation.

Table 5 quantifies the first relationship. A one-unit increase in log annual household income is associated with a 0.095-point increase in nonfinancial flourishing on its 0–10 scale (standard error 0.005). A doubling of income corresponds to 0.066 points, with a 95 percent confidence interval of $[0.059, 0.073]$. For a doubling of income, the estimated change is 0.081 points for the full flourishing index, 0.094 for hedonic well-being, 0.076 for meaning and purpose, and 0.152 for financial stability. The larger financial estimate is expected because that domain directly asks about material worry.

These magnitudes are precisely estimated but small relative to the ten-point outcome scale. They are also not causal income effects. A respondent whose income rises may simultaneously experience a job change, marriage, or other event that affects flourishing. The fixed effects remove stable personal differences and common site–wave shocks, but they do not isolate an exogenous

income innovation, so the estimates cannot be interpreted as causal effects.

4.6 Childhood formation does not materially shift the income gradient

Table 6 reports the paper's principal comparative-static test. In the baseline model, the coefficient on log income is 0.097 (standard error 0.010), while the interaction with childhood religious formation is −0.003 (standard error 0.012; 95 percent confidence interval $[-0.027, 0.021]$). The implied income slope is therefore 0.097 among respondents who never attended religious services around age 12 and 0.094 among weekly attendees. For a doubling of income, the implied changes are 0.067 and 0.065 points, a difference of −0.002 points.

The estimate remains close to zero under specifications that address observed differences in income shocks. Among respondents with stable employment, the interaction is −0.006 (standard error 0.014). When income is interacted with the 11-variable primary slope-control set—childhood finances and health, parental structure, foreign-born status, parental relationships and felt love, outsider status, and maternal and paternal attendance—the religion interaction is 0.028 (standard error 0.019). The point estimate changes sign, but its confidence interval still includes zero. Appendix Figure A.2 presents the implied slope at each original childhood-attendance category. The estimates are nearly flat across the four categories and are imprecise relative to the small differences between them.

The baseline estimate therefore does not support the substitution prediction in Equation (6). This is a substantive null rather than an absence of an income gradient: the main income coefficient is precisely positive, while the interaction is small and estimated with enough precision to rule out a large average flattening. The confidence interval nevertheless permits modest heterogeneity, and

measurement error in both categorical income and recalled formation can attenuate the interaction.

4.7 The null interaction is broadly shared across domains and societies

Appendix Table A.14 and Figure A.3 decompose the interaction across outcomes. The estimates are −0.006 for hedonic well-being, −0.008 for meaning and purpose, 0.016 for health, −0.013 for character, and −0.013 for relationships. None is statistically distinguishable from zero. The financial-stability interaction is −0.083 (standard error 0.026), but this outcome is constructed from questions about meeting expenses and material security and is thus close to the income regressor. I treat it as a secondary result rather than evidence for a broad nonmaterial buffering mechanism.

Figure 4 reports the site-specific estimates. They range from −0.160 in Nigeria to 0.416 in Hong Kong. Twelve of the 22 point estimates are negative and ten are positive, and no site-specific 95 percent confidence interval excludes zero. The inverse-variance pooled estimate is −0.022 (standard error 0.013; 95 percent confidence interval $[-0.049, 0.004]$). Cochran's Q is 20.35 on 21 degrees of freedom, and the estimated between-site variance is zero. The apparent variation in point estimates is therefore consistent with sampling error.

The observed-selection exercise also provides little evidence of a systematic pattern. Across all 22 sites, the slope relating the absolute interaction to the observed-selection index is 1.020 (standard error 0.835). Excluding Hong Kong reduces it to 0.218 (standard error 0.314). Appendix Table A.17 and Figure A.5 show that the positive all-site relationship is driven by one high-leverage observation. The cross-site results do not reveal a stable secularization or selection gradient in the income-slope interaction.

5 Robustness and Identification Limits

5.1 Adult-profile sensitivity

Appendix Table A.2 examines the nonfinancial adult profile across samples and estimators. The fully adjusted coefficient is 0.129 standard deviations. Re-estimating the baseline specification on the fully adjusted model's complete-case sample yields 0.242, nearly identical to the unrestricted baseline estimate of 0.244. The attenuation after adding childhood controls therefore does not arise mechanically from changing observations. The unweighted full-control estimate is 0.121. Estimating the model only with Wave-1 outcomes yields 0.132, while using only later-wave outcomes yields 0.126. A Wave-1 specification using the supplied primary-sampling-unit and stratum variables produces 0.132 (standard error 0.017). These estimates are close to the fully adjusted result despite different weighting and variance assumptions (Appendix Table A.3).

Controlling for adult attendance reduces the childhood-formation coefficient to 0.079 standard deviations. This 39 percent reduction relative to the fully adjusted estimate is consistent with the continuity results: the common gap associated with current participation is substantially larger than the activation contrasts. The activation estimates remain positive in contemporaneous, prospective, and within-person specifications, but they cannot be separated from selection into persistent participation or correlated recall of childhood attendance. Conditioning on adult attendance is not a mediation estimate because adult participation is endogenous and may itself reflect unmeasured capabilities or family persistence.

Appendix Table A.4 applies a propensity-score reweighting exercise to the binary contrast between weekly and less-frequent childhood attendance. I estimate the score in Wave 1 on complete

cases using a survey-weighted logit containing gender; childhood financial difficulty and health; whether the parents were married around age 12; foreign-born status; relationships with and felt love from each parent; outsider status; maternal and paternal religious formation; and study-site and five-year-birth-cohort indicators. I define common support as the intersection of the observed score ranges for weekly and less-frequent attendees. The inverse-probability weight is $D_i/\widehat{p}_i + (1 - D_i)/(1 - \widehat{p}_i)$, where D_i denotes weekly childhood attendance. I multiply it by the normalized annual survey weight, winsorize the combined weight at the first and ninety-ninth percentiles, carry the Wave-1 score across the respondent's observed waves, absorb study-site-by-wave and study-site-by-cohort fixed effects, and cluster standard errors by respondent.

On common support, the weekly-attendance coefficient is 0.072 standard deviations under inverse-probability weighting and 0.055 in a separate fully adjusted outcome model estimated on the same support. The reweighted estimate remains positive but is much smaller than the parsimonious weekly-attendance estimate of 0.145. These are not doubly robust estimates: the third column uses the propensity weights without the childhood controls in the outcome equation, while the fourth uses the full outcome model with the ordinary survey weight. Both address only observed selection and leave unobserved parental traits, local religious supply, and retrospective reporting unresolved; the estimator deals with selection on observables, albeit a rich set of them.

5.2 Income measurement and transition direction

Appendix Table A.13 changes both sides of the interaction. The baseline estimate is −0.003 (standard error 0.012). The interaction is −0.002 using a weekly-attendance indicator and −0.023 using the family religious-environment index. It is 0.043 when income is measured by within-

site-wave rank, 0.016 for the ordinal income category, and −0.003 for the inverse hyperbolic sine of income. Alternative top-bracket rules produce estimates of −0.005 and 0.000, while the subjective-income-adequacy interaction is −0.012. None is statistically distinguishable from zero. The conclusion therefore does not depend on assigning a particular local-currency midpoint to the income brackets.

Figure A.1 shows that income movements are common in both childhood-attendance groups. Among respondents who attended less than weekly at age 12, 42.9 percent of adjacent observations show no bracket change, 18.6 percent move up one bracket, 14.4 percent move up at least two, 11.9 percent move down one, and 12.3 percent move down at least two. The corresponding shares among weekly childhood attendees are 40.1, 17.6, 17.6, 11.5, and 13.2 percent. Panel B shows that adjusted changes in nonfinancial flourishing rise with upward income transitions and fall with downward transitions in both childhood-attendance groups; the confidence intervals overlap throughout. The identifying support for the interaction is therefore not concentrated in one childhood group, and the descriptive transition pattern is consistent with the near-zero interaction estimate.

Appendix Table A.12 estimates adjacent-wave first-difference models by transition direction and size. The interaction is −0.011 (standard error 0.019) for income increases, −0.002 (0.022) for income decreases, −0.004 (0.015) for movements of at least two brackets, and −0.013 (0.014) among all nonzero income moves. The four estimates are uniformly close to zero and provide no evidence that the null depends on the direction or size of the income transition.

5.3 Time-varying shocks and timing

The principal concern for Equation (13) is that income changes coincide with other adult shocks. Appendix Table A.11 adds time-varying employment status, marital status, and the number of children in the household. The interaction changes from −0.003 to −0.004. Restricting the sample to respondents whose employment status does not change yields −0.006. Using lagged rather than contemporaneous income produces 0.009 (standard error 0.018). These checks do not identify exogenous income variation, but they show that the null is not driven by the most immediate employment, marriage, or child-composition changes captured in the data.

The household-size measure needed for equivalized income is available only in Wave 1. A respondent fixed-effects model with observed equivalized income consequently has no person with two usable observations. Carrying Wave-1 household size forward would not solve the problem substantively: the fixed household-size adjustment is a person-specific constant and is absorbed by the respondent fixed effect. I therefore omit equivalized income and a stable-household restriction rather than present algebraically redundant or unidentified specifications.

5.4 Outcome domains and measured childhood overlap

The domain decomposition in Appendix Table A.14 shows that the aggregate null is not concealing a consistent offset between hedonic and eudaimonic outcomes. The interaction is close to zero for both, as well as for health, character, and relationships. The only precisely negative estimate is for financial stability, which includes items about expenses and material security. Because the dependent variable partly restates the material margin measured by income, this result is not a clean test of durable meaning-related capability.

Appendix Table A.15 adds interactions between income and the observed childhood environment. The religion interaction is −0.003 in the baseline, −0.003 after adding the support index, −0.002 after adding adversity, −0.003 after adding both, and 0.012 after adding the eight component measures used to construct the support and adversity indices: childhood finances and health, parental structure, parental relationships and felt love, and outsider status. Figure A.4 displays the corresponding confidence intervals. Since the starting coefficient is already effectively zero, a mediated-share calculation would be undefined or misleading. The exercise instead shows that measured childhood conditions do not mask a large negative interaction.

5.5 Attrition, recall, and common-method measurement

Appendix Table A.18 relates childhood formation to availability of the complete nonfinancial-flourishing outcome in later waves after conditioning on the full childhood vector. The coefficients are 0.004 (standard error 0.007) for Wave 2 and 0.003 (0.008) for Wave 3. Differential later-wave outcome availability by childhood religious formation is therefore small conditional on the observed covariates. Attrition and item nonresponse can still be selective on unmeasured characteristics, and the number of available waves differs across respondents and sites.

Recall remains the principal limitation of the childhood analysis. Childhood attendance, finances, health, relationships, love, outsider status, and abuse are all reported retrospectively in adulthood. A stable reporting tendency can generate correlations among these measures even if the underlying childhood experiences were less strongly related. Individual fixed effects do not address this concern for the adult profile because the childhood measures do not vary over time. In the slope model, fixed effects remove the direct level association between recall and adult flourishing,

but differential reporting of income or changes in well-being by recalled childhood background could still affect the interaction.

Common survey administration creates a related concern. Adult religion, flourishing, and economic circumstances are measured in the same instrument, so mood, social desirability, or response style can induce covariance. Site-by-wave standardization and fixed effects remove common level shifts but not person-specific reporting changes. The continuity and activation analyses help by showing that the broad profile is not present among childhood-only participants, but they cannot distinguish current religious inputs or life-course complementarity from current response style and correlated recall of childhood attendance.

Taken together, the robustness exercises strengthen a narrow conclusion: the adult profile is positive, broad, and partly explained by observed childhood circumstances, whereas the within-person income-slope interaction is consistently close to zero. They do not transform either result into a causal estimate of religious upbringing.

6 Conclusion

Religious upbringing is a widespread childhood institution, yet economics has usually studied religion through adult belief, participation, or congregational life. The human-development literature, by contrast, emphasizes that adult capabilities emerge from sequences of early inputs whose productivity can depend on previously accumulated skills. This paper brings those perspectives together. It asks whether childhood religious formation is associated with a broad adult capability profile, whether that profile persists without adult participation, and whether formation changes the relationship between later material resources and flourishing.

The framework places religious exposure inside a multistage capability-production process. Attendance around age 12 proxies for a larger environment that can include family routines, moral instruction, adult and peer relationships, service, identity, and interpretations of adversity. Dynamic complementarity implies that the consequences of any such input depend on prior capabilities and complementary family conditions. A separate adult production function yields one signed implication: if consumption and durable nonmaterial capability are substitutes, respondents with greater childhood formation should have a flatter within-person income–well-being gradient. The model therefore helps organize the empirical predictions that can be tested in the data.

The empirical results support the broad developmental framing but not a simple independent childhood effect. Weekly rather than never childhood attendance is associated with 0.244 standard deviations higher nonfinancial flourishing and with differences across meaning, character, relationships, health, income rank, employment, and college completion. Adding recalled childhood finances, health, family structure, parental relationships, felt love, and parental attendance reduces the nonfinancial coefficient to 0.129 standard deviations. Childhood formation is itself associated with a 0.214-standard-deviation higher support index and a 0.203-standard-deviation lower adversity index. Religious upbringing therefore appears as part of a bundled developmental environment rather than a separable input. A companion adversity interaction is essentially zero for aggregate nonfinancial flourishing, although it is positive for health and relationships.

Religious continuity matters for interpretation. Childhood-and-current weekly participants differ from adults who participate infrequently in both periods by 0.277 standard deviations in nonfinancial flourishing, while adult-only weekly participants differ by 0.234 and childhood-only participants by 0.002. The activation contrasts are positive and modest: they are statistically significant for the aggregate nonfinancial index, health, and relationships, and the nonfinancial estimate retains the

same positive sign in contemporaneous, prospective, and within-person specifications. They are economically small relative to the common gap associated with current participation and cannot be separated from selection into persistent participation or correlated recall of childhood attendance. Positive activation contrasts also appear for income rank and log household income even though both current-participation groups have lower adjusted income rank than adults who participate infrequently in both periods, a pattern consistent with either complementarity or differential selection. Frequent childhood attendance alone leaves no broad adjusted profile after participation ends, while limited complementarity with adult participation remains possible.

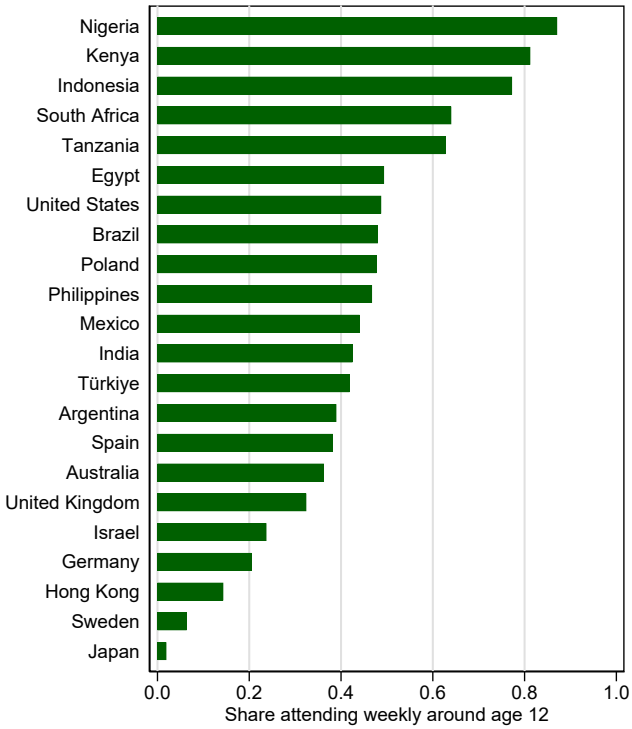
The panel evidence rejects the paper's main signed comparative-static prediction on average. A doubling of household income is associated with a 0.066-point increase in nonfinancial flourishing on a 0–10 scale. The income-by-childhood-formation interaction is -0.003 with a standard error of 0.012, implying only a -0.002-point difference in the response to a doubling of income between weekly and never childhood attendees. The estimate remains near zero across formation measures, income codings, time-varying controls, employment-stable and mover samples, nonfinancial domains, and study sites. The data therefore show within-person comovement between income and flourishing, but not a distinct average buffering role for childhood religious formation.

Three limitations motivate further research. First, the childhood measures are retrospective and jointly recalled, so prospective data following children, parents, and congregations would separate developmental inputs from adult reconstruction. Second, service attendance is an intensity measure rather than a measure of content. Dyadic data on parenting, mentoring, peer networks, moral instruction, and institutional quality would identify which parts of religious environments are developmentally relevant and which are harmful or neutral. Third, the short income panel cannot isolate exogenous material shocks. Longer panels linked to administrative earnings, employment,

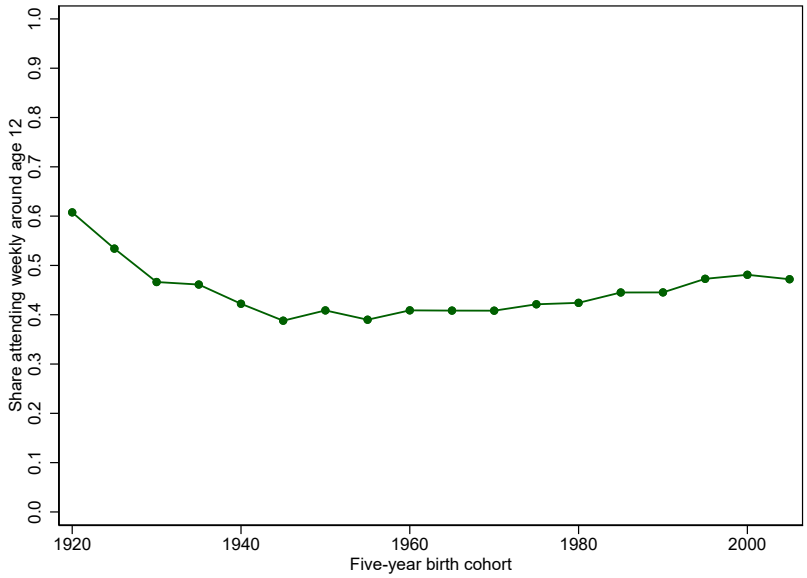
education, and health records—or designs exploiting plausibly exogenous changes in access to religious institutions—could distinguish durable childhood formation from current congregational effects. Such evidence would more directly connect the economics of religion to the technology of human development while preserving the distinction between association and causation.

Tables and Figures

Figure 1: Weekly Religious-Service Attendance Around Age 12



(a) Across study sites



(b) Across five-year birth cohorts

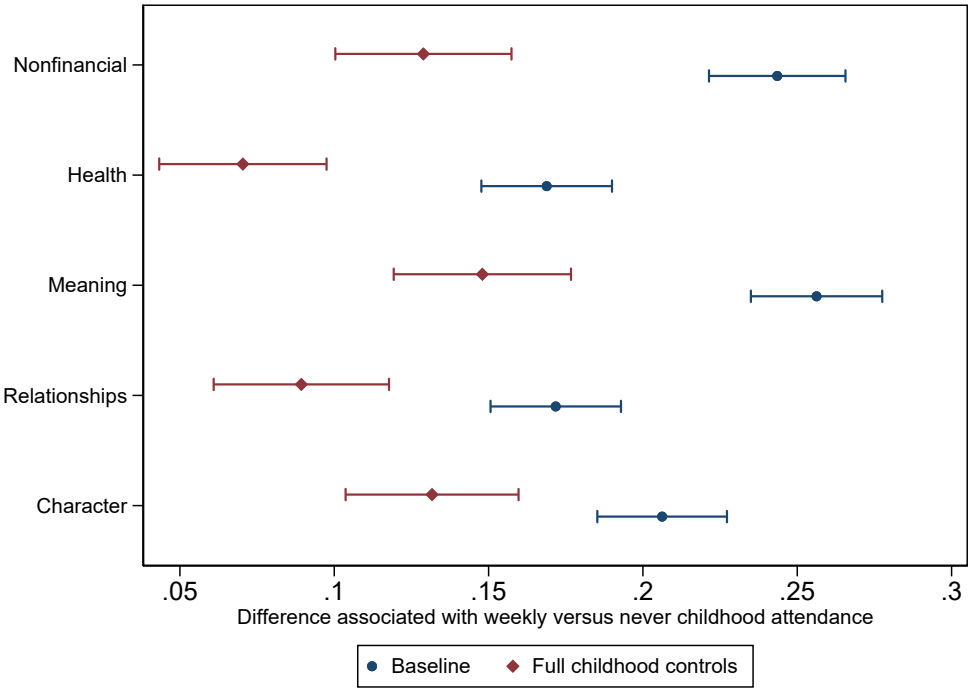
Notes.—Source: Global Flourishing Study, Wave 1. Panel A reports the weighted share of respondents in each study site who recall attending religious services or worshiping at least weekly around age 12. Hong Kong is treated as a separate study site. Panel B reports the weighted share by five-year birth cohort using equal-site weights, so each study site contributes the same total weight. The figure is descriptive and does not adjust for differences in cohort or site composition.

Table 1: Summary Statistics

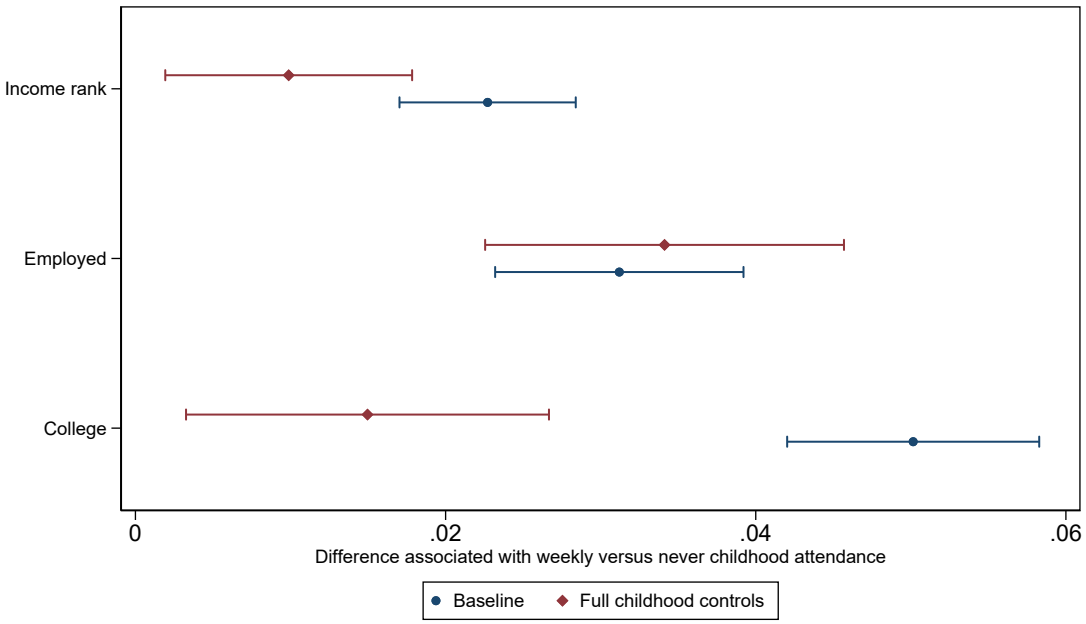
	Wave 1 respondents		Observed in 2+ waves		Income movers	
	mean	sd	mean	sd	mean	sd
Nonfinancial flourishing (site-wave z-score)	-0.033	1.036	-0.035	1.031	-0.034	1.015
Health (site-wave z-score)	-0.042	1.035	-0.040	1.027	-0.036	1.015
Meaning and purpose (site-wave z-score)	-0.029	1.033	-0.029	1.033	-0.033	1.024
Close relationships (site-wave z-score)	-0.011	1.023	-0.012	1.018	-0.012	1.010
Character and virtue (site-wave z-score)	-0.023	1.033	-0.026	1.023	-0.016	1.009
Within-country-by-wave household-income rank	0.485	0.291	0.492	0.291	0.489	0.283
Employed	0.570	0.495	0.573	0.495	0.589	0.492
Completed four-year college degree	0.213	0.410	0.236	0.425	0.218	0.413
Childhood religious formation	0.586	0.406	0.580	0.409	0.587	0.406
Supportive childhood index (country z-score)	-0.024	1.021	-0.018	1.017	-0.023	1.021
Adverse childhood index (country z-score)	0.033	1.020	0.030	1.017	0.034	1.021

Notes.—Source: Global Flourishing Study. The Wave-1 sample contains one observation per baseline respondent. The repeated-outcome sample contains respondents observed on the nonfinancial index in at least two waves. Income movers report different income categories across at least two observed waves. Flourishing domains and childhood indices are standardized within study site and wave or study site, as appropriate. Income rank is scaled from zero to one within study site and wave.

Figure 2: Childhood Religious Formation and Adult Human-Development Outcomes



(a) Flourishing domains



(b) Economic outcomes

Notes.—Source: Global Flourishing Study. Points compare weekly with never religious-service attendance around age 12. Circles report the baseline specification and diamonds report the specification with the full observed childhood controls. Bars show 95 percent confidence intervals based on respondent-clustered standard errors. Flourishing outcomes are standardized within study site and wave; economic outcomes retain their natural zero-to-one scales.

Table 2: Sequential Controls for Childhood Religious Formation and Adult Outcomes

Outcome	Baseline	+ childhood conditions	+ relationships	+ parental religion
Nonfinancial flourishing	0.244*** [0.011]	0.205*** [0.011]	0.193*** [0.011]	0.129*** [0.015]
Health	0.169*** [0.011]	0.130*** [0.011]	0.121*** [0.011]	0.070*** [0.014]
Meaning and purpose	0.256*** [0.011]	0.226*** [0.011]	0.222*** [0.011]	0.148*** [0.015]
Close relationships	0.172*** [0.011]	0.141*** [0.011]	0.127*** [0.011]	0.089*** [0.014]
Character and virtue	0.206*** [0.011]	0.185*** [0.011]	0.169*** [0.011]	0.132*** [0.014]
Income rank	0.023*** [0.003]	0.012*** [0.003]	0.012*** [0.003]	0.010** [0.004]
Employed	0.031*** [0.004]	0.025*** [0.004]	0.025*** [0.004]	0.034*** [0.006]
College completion	0.050*** [0.004]	0.039*** [0.004]	0.037*** [0.005]	0.015** [0.006]

Notes.—Source: Global Flourishing Study. Each cell reports the coefficient comparing weekly with never religious-service attendance around age 12, followed by the respondent-clustered standard error in brackets. Flourishing outcomes are standardized within study site and wave; income rank, employment, and college completion run from zero to one. All specifications include gender, study-site-by-wave fixed effects, and study-site-by-five-year-birth-cohort fixed effects. Column 2 adds childhood finances and health, parental structure, and foreign-born status; Column 3 adds parental relationships, felt love, and outsider status; Column 4 adds maternal and paternal attendance. Annual survey weights are used. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Childhood Religious Formation and the Recalled Childhood Environment

Childhood measure	Coefficient	Standard error	Wave-1 respondents
Supportive childhood index	0.214***	0.018	194,271
Adverse childhood index	-0.203***	0.017	197,933
Favorable childhood finances	0.040***	0.004	197,567
Favorable childhood health	0.036***	0.004	197,635
Parents married around age 12	0.080***	0.008	189,381
Good relationship with mother	0.035***	0.004	191,452
Good relationship with father	0.047***	0.005	183,507
Felt loved by mother	0.016***	0.003	189,342
Felt loved by father	0.031***	0.004	180,741
Did not feel like an outsider	0.009	0.007	194,905

Notes.—Source: Global Flourishing Study, Wave 1. Each row reports a separate weighted regression of the listed childhood outcome on the childhood religious-formation scale, gender, foreign-born status, and study-site-by-five-year-birth-cohort fixed effects. The support and adversity indices are standardized within study site. Component outcomes run from zero to one, with positive values indicating the favorable condition named in the row. Standard errors are clustered by study-site-by-cohort cell. These retrospective associations do not identify whether religious participation shaped the childhood environment or family type generated both. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Childhood and Wave-1 Adult Religious Participation

Panel A. Flourishing outcomes

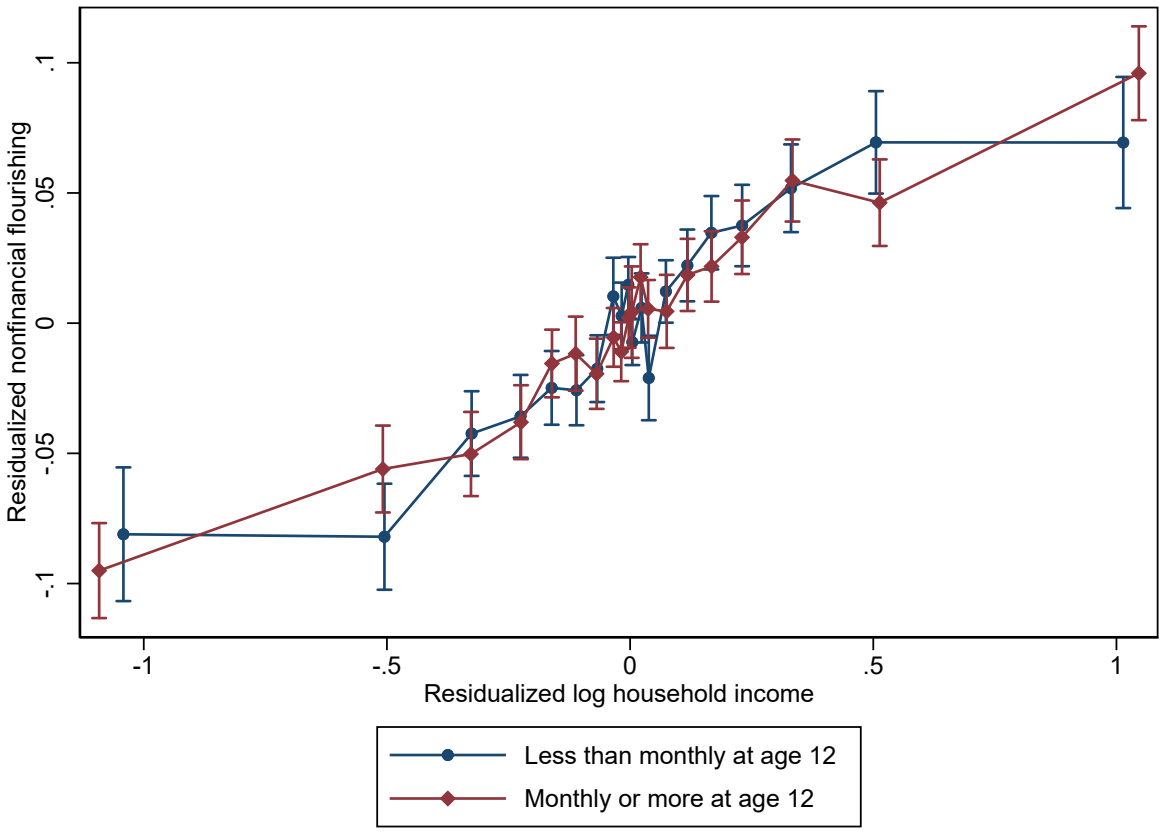
	Nonfinancial	Health	Meaning	Relationships	Character
Weekly at age 12 and Wave 1	0.277***	0.195***	0.306***	0.190***	0.198***
	[0.012]	[0.012]	[0.011]	[0.011]	[0.011]
Weekly at age 12 only	0.002	-0.012	0.011	-0.001	0.039***
	[0.012]	[0.012]	[0.012]	[0.012]	[0.011]
Weekly in Wave 1 only	0.234***	0.158***	0.271***	0.160***	0.149***
	[0.013]	[0.012]	[0.012]	[0.012]	[0.013]
Person–wave observations	310507	315872	315899	315686	315814
R-squared	0.132	0.130	0.105	0.084	0.051

Panel B. Economic outcomes

	Income rank	Employed	College
Weekly at age 12 and Wave 1	-0.021***	0.006	-0.024***
	[0.004]	[0.005]	[0.005]
Weekly at age 12 only	-0.005	0.014***	-0.005
	[0.003]	[0.005]	[0.005]
Weekly in Wave 1 only	-0.035***	-0.004	-0.016***
	[0.004]	[0.006]	[0.006]
Person–wave observations	295348	316181	317358
R-squared	0.101	0.302	0.160

Notes.—Source: Global Flourishing Study. The omitted group attended less than weekly both around age 12 and in Wave 1. Frequent childhood participation means weekly attendance around age 12; frequent adult participation means attendance at least weekly in Wave 1. The Wave-1 adult classification is fixed across the respondent's person–wave observations. The childhood-and-Wave-1 group does not establish uninterrupted participation between age 12 and adulthood. All specifications include the full observed childhood controls, gender, study-site-by-wave fixed effects, and study-site-by-five-year-birth-cohort fixed effects. Annual survey weights are used. Standard errors, in brackets, are clustered by respondent. The coefficients are adjusted group differences, not causal effects of entering, leaving, or continuing religious participation. The activation contrast defined in Equation (11) equals the childhood-and-Wave-1 coefficient minus the Wave-1-only and childhood-only coefficients. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3: Within-Person Income Changes and Nonfinancial Flourishing



Notes.—Source: Global Flourishing Study. The figure uses the baseline fixed-effects estimation sample, forms 20 pooled ventiles of residualized log household income, and plots mean residualized nonfinancial flourishing separately for respondents who attended less than monthly and at least monthly around age 12. Both variables are residualized with respondent and study-site-by-wave fixed effects. Bars show 95 percent confidence intervals based on respondent-clustered standard errors for each bin mean. The figure is descriptive; Table 5 reports the corresponding fixed-effects regression.

Table 5: Within-Person Household Income and Adult Flourishing

	Nonfinancial	Full	Hedonic	Eudaimonic	Financial
Log household income	0.095***	0.117***	0.135***	0.110***	0.220***
	[0.005]	[0.005]	[0.008]	[0.007]	[0.011]
Person–wave observations	324447	323357	332611	332341	333211
Within R-squared	0.003	0.005	0.003	0.002	0.003

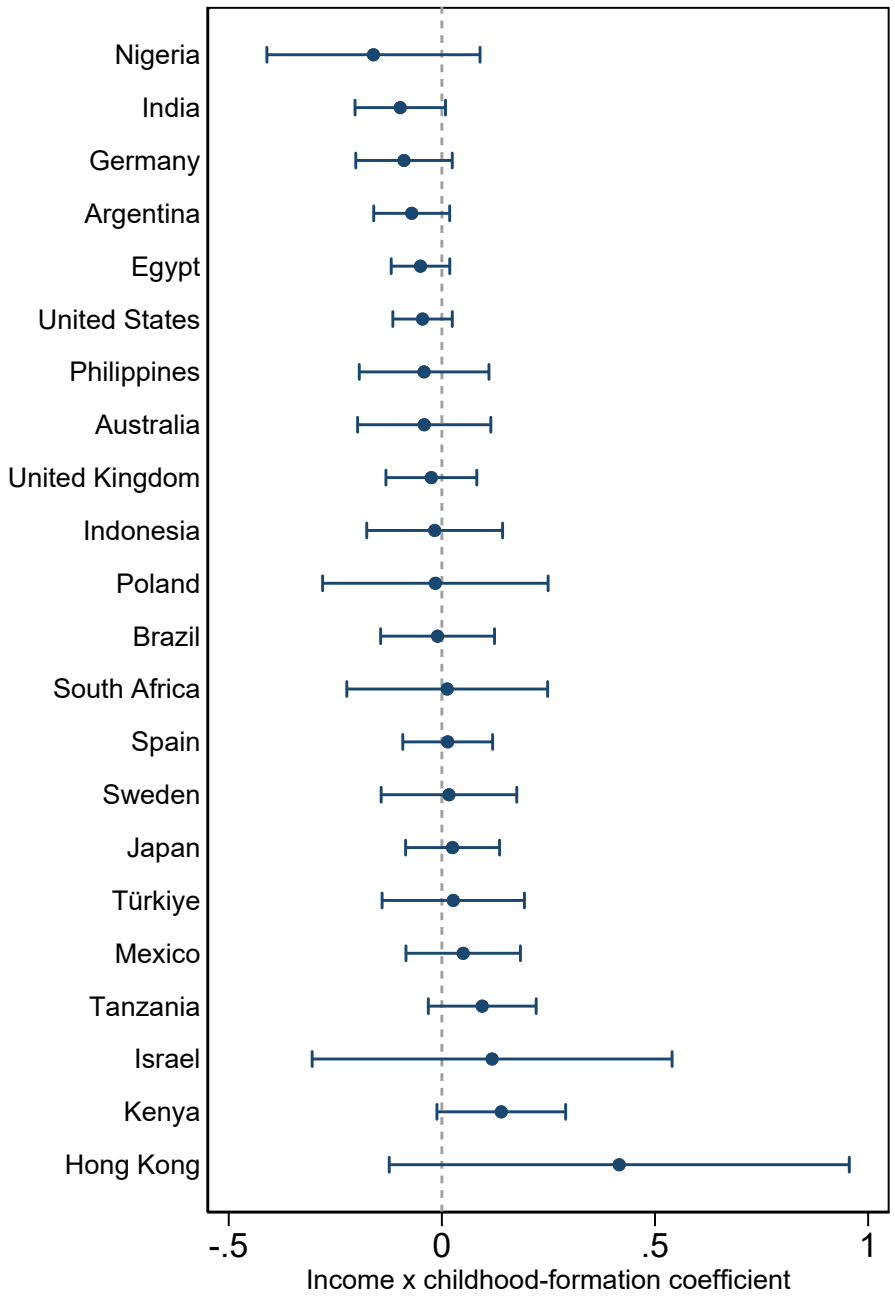
Notes.—Source: Global Flourishing Study. Each column reports a separate regression of the 0–10 flourishing measure on log annualized household income. All specifications include respondent and study-site-by-wave fixed effects. Standard errors, in brackets, are clustered by respondent. The nonfinancial index averages health, hedonic well-being, meaning and purpose, character, and close relationships; the full index additionally includes financial stability. The coefficients are within-person associations and do not isolate exogenous income changes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Childhood Religious Formation as an Income-Slope Shifter

	Baseline	Stable employment	Childhood interactions
Log household income	0.097***	0.102***	0.165***
	[0.010]	[0.011]	[0.035]
Income x childhood formation	-0.003	-0.006	0.028
	[0.012]	[0.014]	[0.019]
Person–wave observations	321963	256574	245188
Within R-squared	0.003	0.003	0.003

Notes.—Source: Global Flourishing Study. The dependent variable is nonfinancial flourishing on its 0–10 scale. Childhood religious formation runs from zero for never attending around age 12 to one for weekly attendance. All specifications include respondent and study-site-by-wave fixed effects; the level of childhood formation is absorbed by the respondent fixed effect. Column 2 restricts the sample to respondents whose employment status is unchanged across at least two observed waves. Column 3 adds interactions between income and the 11-variable primary slope-control set: childhood finances and health, parental structure, foreign-born status, parental relationships and felt love, outsider status, and maternal and paternal attendance. Standard errors, in brackets, are clustered by respondent. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 4: Income-Slope Interactions Across Study Sites



Notes.—Source: Global Flourishing Study. Each point is the coefficient on log household income interacted with childhood religious formation from a separate study-site regression with respondent and wave fixed effects. Bars show 95 percent confidence intervals based on respondent-clustered standard errors. The vertical dashed line is zero. The inverse-variance pooled estimate is −0.022 (standard error 0.013). Site-level estimates are descriptive and imprecise because each regression is identified by only two or three waves of income movements.

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7 Additional Measurement, Results, and Robustness

This appendix reports the construction details and supporting estimates that are useful for evaluating the main results but are not required to follow the paper's argument. The analysis uses only the long person–wave file. Childhood measures and baseline characteristics are repeated across a respondent's observed waves; no separate person-level analysis file is created.

7.1 Variable construction and panel conventions

Table A.1 summarizes the principal measures. Responses coded as saw/skipped, don't know, or refused are set to missing. Parent-specific "does not apply" responses are not coded as adverse experiences. The childhood religious-formation scale reverses and rescales the four categories of `SVCS_12YRS`, so zero denotes never attending and one denotes attending at least weekly. The adult outcome profiles use study-site-by-wave standardized outcomes; the fixed-effects income analyses retain the underlying 0–10 flourishing scales to make within-person magnitudes interpretable.

Household income is constructed from the country-specific `INCOME` brackets. Monthly brackets are annualized, closed brackets receive their midpoint, and open upper categories use a transparent tail rule examined below. Income rank is computed within study site and wave. Adult household size is observed only in Wave 1, so equivalized income is omitted from respondent fixed-effects models; carrying baseline household size forward would subtract a person-specific constant already absorbed by those fixed effects.

Table A.1: Construction of Principal Variables

Construct	Source variable(s)	Construction
Childhood religious formation	SVCS_12YRS	Reverse-rescaled four-category attendance measure: never = 0, less than monthly = 1/3, one to three times monthly = 2/3, and at least weekly = 1.
Parental religious formation	SVCS_MOTHER, SVCS_FATHER	Reverse-rescaled using the same four substantive categories; does-not-apply responses remain missing.
Nonfinancial flourishing	Ten GFS items	Equal-weight mean of happiness/life satisfaction, health, meaning/purpose, character/virtue, and close-relationship domains; the financial domain is excluded.
Hedonic flourishing	HAPPY, LIFE_SAT	Mean of the two 0–10 items.
Eudaimonic flourishing	LIFE_PURPOSE, WORTHWHILE	Mean of the two 0–10 items.
Supportive childhood environment	Childhood health, parents' marriage, parental relationships, felt love, outsider status	Favorably oriented mean of available components, requiring at least four of seven, standardized within study site.
Adverse childhood environment	Childhood financial difficulty, poor health, family disruption, poor parental relationships, lack of felt love, outsider status, abuse	Adversely oriented mean of available components, requiring at least four of nine, standardized within study site.
Annual household income	INCOME	Country-specific geometric bracket midpoint, annualized when the source category is monthly; bottom-open brackets use one-half of the upper bound and top-open brackets use 1.5 times the lower bound. Logged after excluding nonpositive values.
Income rank	INCOME	Percentile rank of the harmonized income category within study site and wave, scaled from zero to one.
Income mover	Longitudinal INCOME	Respondent observed with valid income in at least two waves whose harmonized income category changes across observed waves.
Stable employment	EMPLOYMENT	Respondent observed with a valid employed/non-employed indicator in at least two waves whose indicator does not change.
Religious continuity	SVCS_12YRS, ATTEND_SVCS	Four mutually exclusive groups formed from weekly childhood attendance and weekly adult attendance in Wave 1. The less-than-weekly in both periods group is omitted.

Notes.—Variable names follow the Global Flourishing Study release. Adult household size equals the number of adults plus the number of children in the household, but the adult count is observed only in Wave 1 and is therefore used only descriptively. All time-invariant childhood variables are repeated within person in the long file.

7.2 Adult-profile sensitivity

Table 2 reports the sequential coefficient path for the principal adult outcomes. The estimates below vary weighting, outcome timing, observed-selection adjustment, and contemporaneous adult participation. These comparisons test whether the broad profile depends on a particular sample or estimator; they do not identify selection on unobserved family or community characteristics.

For the propensity-score exercise, the treatment is weekly rather than less-frequent attendance around age 12. The Wave-1 survey-weighted logit is estimated on complete cases and includes gender; childhood financial difficulty and favorable health; parents' marital status; foreign-born status; good relationships with each parent; felt love from each parent; outsider status; maternal and paternal attendance scales; and study-site and five-year-birth-cohort indicators. Common support is the overlap between the minimum and maximum predicted probabilities observed in the two treatment groups. Average-treatment-effect weights are multiplied by the annual survey weight and winsorized at the first and ninety-ninth percentiles before being applied to the long person–wave file.

Table A.2: Robustness of the Adult Human-Development Profile

Specification	Coefficient	Standard error	Person–waves
Baseline	0.244***	0.011	418,164
Full observed childhood controls	0.129***	0.015	311,290
Baseline, full-control sample	0.242***	0.012	311,290
Unweighted, full-control sample	0.121***	0.009	311,290
Wave 1 outcome	0.132***	0.017	142,542
Later-wave outcomes	0.126***	0.017	168,735
Add current weekly attendance	0.079***	0.015	310,319

Notes.—The outcome is study-site-by-wave standardized nonfinancial flourishing. The "Baseline, full-control sample" row estimates the baseline model on the complete-case sample used by the full-control model. The unweighted row estimates the full-control model on that same sample without survey weights. Wave 1 and later-wave rows separate outcome timing. The final row conditions on contemporaneous adult attendance and is descriptive because adult participation is endogenous. All other rows use the available annual survey weight. Standard errors are clustered by respondent. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Complex-Survey Inference for the Adult Profile

	Nonfinancial flourishing (site-wave z-score)
Childhood religious formation	0.132***
	[0.017]
Wave-1 respondents	142555
R-squared	0.135

Notes.—The Wave-1 model uses the supplied annual weight and survey-design variables. The specification otherwise matches the fully adjusted adult-profile regression. The estimate is close to the respondent-clustered result reported in the main analysis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Observed-Selection Adjustment for Weekly Childhood Attendance

Specification	Coefficient	Standard error	Person–waves
Baseline	0.145***	0.008	418,164
Full observed childhood controls	0.055***	0.009	311,290
IPW, common support	0.072***	0.009	310,182
Outcome model, common support	0.055***	0.009	310,182

Notes.—The Wave-1 propensity score is estimated on complete cases with the survey-weighted logit described above. Common support is the intersection of the predicted-score ranges for weekly and less-frequent childhood attendees. The IPW row uses $D/\widehat{p} + (1 - D)/(1 - \widehat{p})$, multiplies this weight by the normalized annual survey weight, and winsorizes the combined weight at the first and ninety-ninth percentiles. The outcome-model row instead uses the full childhood-control vector on the common-support sample with the ordinary annual survey weight. Study-site-by-wave and study-site-by-cohort fixed effects are included; standard errors are clustered by respondent. The estimates adjust only for observed selection. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7.3 Childhood exposure and adult religious participation

Table A.5: Life-Course Complementarity Contrasts

Outcome	Contrast	Standard error	Person–waves
Nonfinancial flourishing	0.041**	0.016	310,507
Health	0.048***	0.016	315,872
Meaning and purpose	0.024	0.016	315,899
Close relationships	0.031**	0.015	315,686
Character and virtue	0.010	0.016	315,814
Income rank	0.020***	0.005	295,348
Log household income	0.049***	0.016	294,531
Employed	-0.004	0.007	316,181
College completion	-0.004	0.007	317,358

Notes.—Each row reports (childhood-and-Wave-1 − Wave-1-only) − (childhood-only − less-than-weekly in both). Adult participation is measured in Wave 1 and the classification is fixed across person–wave observations. Models include the full observed childhood controls, gender, study-site-by-wave fixed effects, and study-site-by-five-year-birth-cohort fixed effects, use annual survey weights, and cluster standard errors by respondent. The contrasts are descriptive. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Childhood Formation and Contemporaneous Adult Participation

Panel A. Flourishing outcomes

	Nonfinancial	Health	Meaning	Relationships	Character
Weekly attendance around age 12	0.007	-0.007	0.017	0.001	0.038***
	[0.012]	[0.011]	[0.012]	[0.012]	[0.011]
Currently attends weekly	0.261***	0.171***	0.296***	0.190***	0.172***
	[0.012]	[0.012]	[0.011]	[0.011]	[0.012]
Childhood x current weekly attendance	0.032**	0.042***	0.012	0.024*	0.008
	[0.015]	[0.015]	[0.015]	[0.014]	[0.015]
Person–wave observations	310319	315669	315654	315440	315572
R-squared	0.134	0.131	0.107	0.086	0.052

Panel B. Economic outcomes

	Income rank	Log income	Employed	College
Weekly attendance around age 12	-0.006*	-0.014	0.014***	-0.008
	[0.003]	[0.011]	[0.005]	[0.005]
Currently attends weekly	-0.036***	-0.107***	-0.008	-0.019***
	[0.003]	[0.012]	[0.005]	[0.005]
Childhood x current weekly attendance	0.021***	0.063***	-0.003	0.003
	[0.004]	[0.015]	[0.006]	[0.006]
Person–wave observations	295141	294321	315934	317107
R-squared	0.101	0.765	0.302	0.160

Notes.—Each column estimates the interaction between weekly attendance around age 12 and weekly attendance in the contemporaneous adult wave. All models include the full observed childhood controls, gender, study-site-by-wave fixed effects, and study-site-by-five-year-birth-cohort fixed effects, use annual survey weights, and cluster standard errors by respondent. The interaction is the direct activation contrast. Adult participation is endogenous, so the estimates are descriptive. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Wave-1 Adult Participation and Later-Wave Outcomes

Panel A. Flourishing outcomes

	Nonfinancial	Health	Meaning	Relationships	Character
Weekly attendance around age 12	0.007	0.006	0.012	0.003	0.028**
	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]
Weekly adult attendance in Wave 1	0.228***	0.150***	0.272***	0.160***	0.137***
	[0.016]	[0.016]	[0.016]	[0.016]	[0.016]
Childhood x Wave 1 adult attendance	0.030	0.034*	0.020	0.015	0.014
	[0.020]	[0.020]	[0.020]	[0.019]	[0.020]
Later-wave observations	168319	171094	171517	171433	171463
R-squared	0.125	0.124	0.102	0.083	0.049

Panel B. Economic outcomes

	Income rank	Log income	Employed	College
Weekly attendance around age 12	-0.003	-0.003	0.013**	-0.004
	[0.004]	[0.013]	[0.006]	[0.006]
Weekly adult attendance in Wave 1	-0.034***	-0.088***	-0.002	-0.014**
	[0.005]	[0.016]	[0.007]	[0.007]
Childhood x Wave 1 adult attendance	0.019***	0.042**	-0.010	-0.007
	[0.006]	[0.019]	[0.008]	[0.008]
Later-wave observations	161017	160199	171228	172333
R-squared	0.106	0.747	0.314	0.166

Notes.—The sample contains Wave-2 and Wave-3 outcomes. Weekly adult attendance is fixed at its Wave-1 value. All models include the full observed childhood controls, gender, study-site-by-wave fixed effects, and study-site-by-five-year-birth-cohort fixed effects, use annual survey weights, and cluster standard errors by respondent. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Within-Person Changes in Adult Religious Participation

Panel A. Flourishing outcomes

	Nonfinancial	Health	Meaning	Relationships	Character
Currently attends weekly	0.065***	0.031***	0.064***	0.066***	0.064***
	[0.009]	[0.010]	[0.010]	[0.010]	[0.010]
Current x childhood weekly attendance	0.025**	0.037***	0.003	0.007	0.001
	[0.012]	[0.013]	[0.013]	[0.013]	[0.014]
Person–wave observations	64110	65408	65354	65317	65333
Attendance switchers	24120	24434	24415	24399	24396
Within R-squared	0.004	0.002	0.003	0.003	0.002

Panel B. Economic outcomes

	Income rank	Log income	Employed
Currently attends weekly	-0.004*	-0.016*	-0.001
	[0.003]	[0.009]	[0.004]
Current x childhood weekly attendance	0.001	0.008	0.005
	[0.003]	[0.013]	[0.006]
Person–wave observations	58423	57949	65259
Attendance switchers	22384	22235	24361
Within R-squared	0.000	0.000	0.000

Notes.—The sample is restricted to respondents whose weekly-attendance status changes across observed waves. Models include respondent and study-site-by-wave fixed effects and cluster standard errors by respondent. The level of childhood weekly attendance is absorbed by respondent fixed effects. These unweighted estimates describe within-person comovement and do not identify an exogenous change in religious participation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7.4 Additional adult outcomes and income transitions

The additional adult outcomes in Table A.9 extend the adult profile beyond the six flourishing domains. Childhood formation is only weakly associated with subjective income adequacy after full adjustment, but it is positively associated with volunteering, helping a stranger, and charitable giving. These behaviors are self-reported and may differ in meaning across societies, so they are treated as supplementary rather than incorporated into the principal human-development index.

Table A.9: Additional Adult Outcomes

	Income adequacy	Volunteered	Helped stranger	Donated
Childhood religious formation	0.007*	0.060***	0.074***	0.098***
	[0.004]	[0.005]	[0.006]	[0.006]
Person–wave observations	316635	317319	317111	317135
R-squared	0.237	0.119	0.146	0.152

Notes.—Each column reports the coefficient on childhood religious formation from the fully adjusted adult-profile model. Binary outcomes are in probability units. Standard errors are clustered by respondent. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7.5 Childhood adversity as a moderator

A companion specification interacts childhood religious formation with the adverse-childhood index in the adult-profile model. Appendix Table A.10 provides selective rather than broad evidence of moderation. The interaction is 0.010 (standard error 0.012) for aggregate nonfinancial flourishing and is effectively zero for meaning and character. It is positive for health and close relationships, 0.029 in each case, and small but positive for income rank, employment, and college completion. Because adversity and religious upbringing are jointly recalled in adulthood and both are selected family characteristics, these coefficients are descriptive buffering patterns rather than causal protection from adversity.

Table A.10: Childhood Religious Formation, Adversity, and Adult Outcomes

Outcome	Adversity	Formation × adversity	Person–waves
Nonfinancial flourishing	-0.137*** [0.018]	0.010 [0.012]	311,290
Health	-0.161*** [0.017]	0.029*** [0.011]	316,703
Meaning and purpose	-0.071*** [0.018]	0.000 [0.013]	316,725
Close relationships	-0.118*** [0.017]	0.029** [0.013]	316,504
Character and virtue	-0.040** [0.017]	-0.001 [0.012]	316,630
Income rank	-0.023*** [0.005]	0.008*** [0.003]	296,076
Employed	0.007 [0.007]	0.008* [0.005]	317,006
College completion	-0.010 [0.007]	0.011** [0.005]	318,194

Notes.—Source: Global Flourishing Study. Cells report coefficients with respondent-clustered standard errors in brackets. Childhood religious formation runs from zero for never attendance around age 12 to one for weekly attendance. The adverse-childhood index is standardized within study site. All specifications include the full observed childhood controls, gender, study-site-by-wave fixed effects, and study-site-by-five-year-birth-cohort fixed effects. Annual survey weights are used. Positive interaction coefficients indicate that the negative association between adversity and the outcome is less pronounced at higher levels of childhood religious formation; they do not identify a causal buffering effect. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Time-Varying Economic and Household Controls

Specification	Interaction	Standard error	Person–waves
Baseline	-0.003	0.012	321,963
Time-varying controls	-0.004	0.013	288,251
Stable employment	-0.006	0.014	256,574
Lagged income	0.009	0.018	155,810

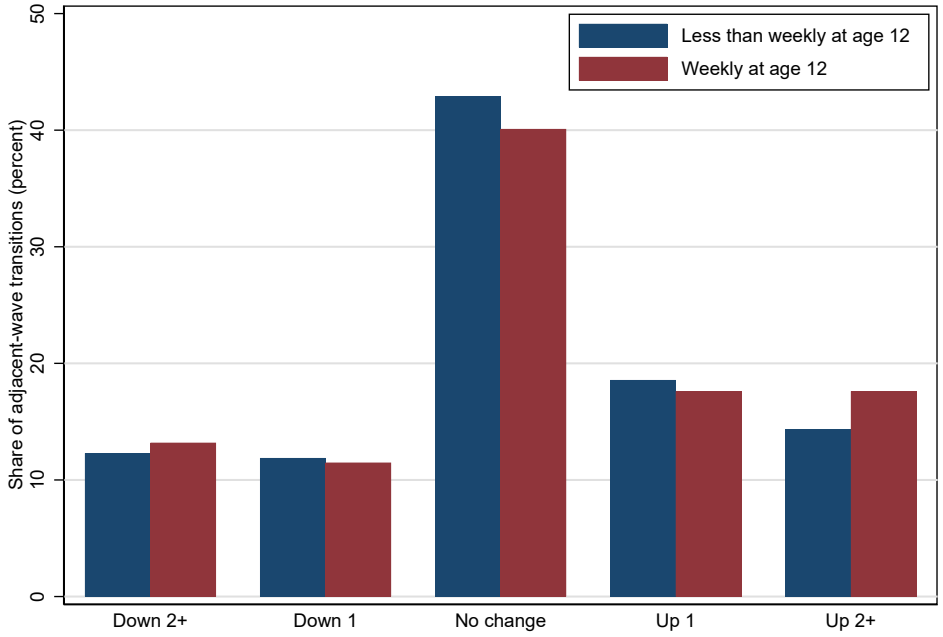
Notes.—All specifications include respondent and study-site-by-wave fixed effects. The time-varying-controls row adds employment status, marital status, and the number of children under age 18 in the household. The stable-employment row requires at least two valid employment observations. The final row uses one-wave-lagged log household income. Standard errors are clustered by respondent.

Table A.12: Income-Transition Restrictions

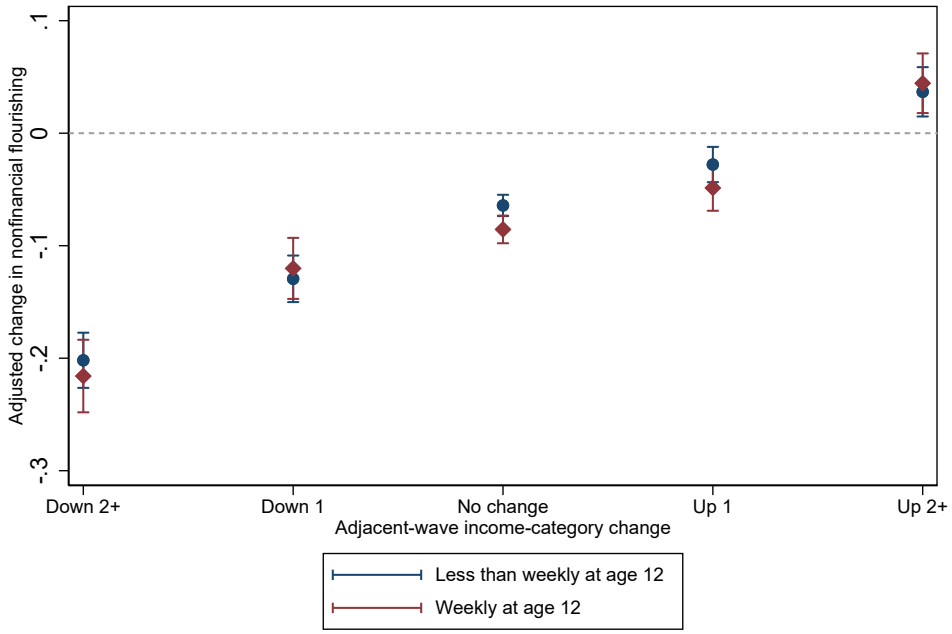
Identifying sample	Interaction	Standard error	Adjacent-wave transitions
Income increases	-0.011	0.019	61,478
Income decreases	-0.002	0.022	44,598
Moves of two or more brackets	-0.004	0.015	51,521
All nonzero income moves	-0.013	0.014	106,076

Notes.—The dependent variable is the adjacent-wave change in nonfinancial flourishing, and the income measure is the adjacent-wave change in log annual household income. Rows restrict transitions by the direction or size of the observed income-category change. All models include study-site-by-wave fixed effects and cluster standard errors by respondent. The final column reports the number of adjacent-wave transitions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.1: Within-Person Income-Category Transitions and Changes in Flourishing



(a) Distribution of income transitions



(b) Adjusted changes in nonfinancial flourishing

Notes.—Source: Global Flourishing Study. Panel A reports the share of adjacent-wave observations with an observed income transition and an observed change in nonfinancial flourishing in each harmonized income-transition category, separately by weekly childhood attendance. Panel B reports adjusted mean changes in nonfinancial flourishing from a first-difference model with study-site-by-wave fixed effects, by the same transition categories and childhood-attendance groups. Bars show 95 percent confidence intervals based on respondent-clustered standard errors. Positive categories indicate upward income moves and negative categories indicate downward moves.

7.6 Alternative income measures and domain decomposition

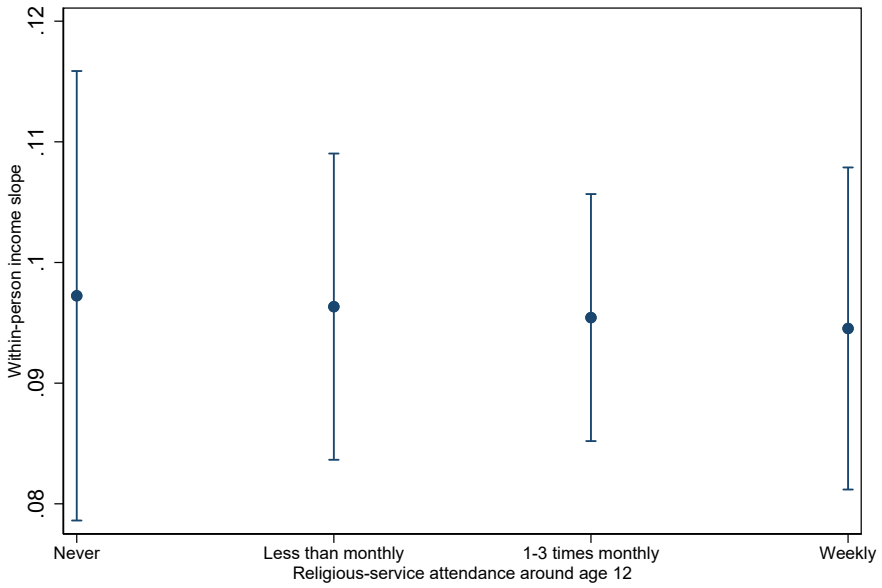
Table A.13 shows that the null slope-shifter estimate is not an artifact of the preferred midpoint conversion or linear childhood-formation scale. The interaction remains small when childhood formation is coded as weekly attendance, when the broader own-parent religious environment is used, and when income is represented by rank, ordinal category, the inverse hyperbolic sine, alternative top-bracket values, or subjective adequacy.

Table A.13: Alternative Religious-Formation and Income Measures

Alternative specification	Interaction	Standard error	Person–waves
Formation scale	-0.003	0.012	321,963
Weekly childhood attendance	-0.002	0.010	321,963
Family religious environment	-0.023	0.015	298,016
Within-site-wave income rank	0.043	0.040	323,202
Log ordinal income category	0.016	0.021	323,202
Inverse hyperbolic sine income	-0.003	0.012	321,963
Top bracket: 1.25 times lower bound	-0.005	0.013	321,963
Top bracket: 2.0 times lower bound	0.000	0.012	321,963
Subjective income adequacy	-0.012	0.033	352,407

Notes.—Each row modifies either the childhood religious-formation measure or the income measure in Equation (13). All specifications include respondent and study-site-by-wave fixed effects; standard errors are clustered by respondent. Tail-rule rows assign the open top income bracket 1.25 or 2 times its lower bound.

Figure A.2: Implied Within-Person Income Slopes by Childhood Attendance



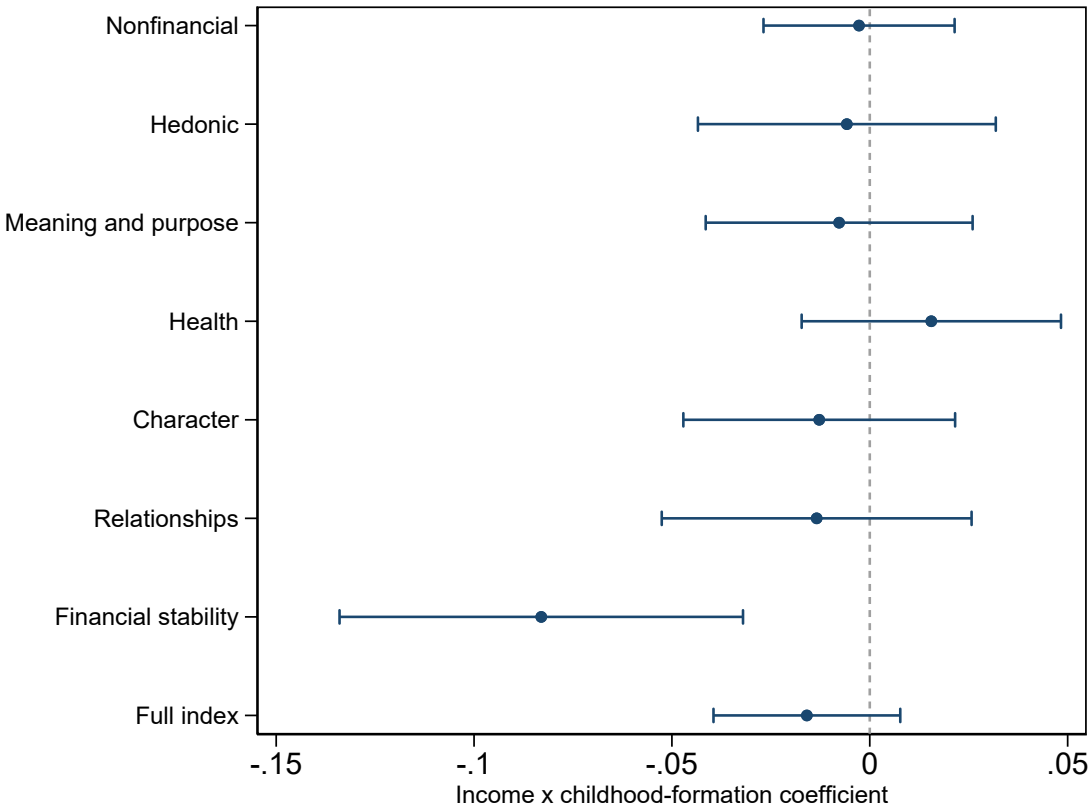
Notes.—Source: Global Flourishing Study. The points plot the income slopes implied by the baseline model in Table 6 at the four original childhood-attendance categories. Bars show delta-method 95 percent confidence intervals.

Table A.14: Income-Slope Interactions Across Flourishing Domains

Outcome	Interaction	Standard error	Person–waves
Nonfinancial flourishing	-0.003	0.012	321,963
Hedonic well-being	-0.006	0.019	330,039
Meaning and purpose	-0.008	0.017	329,779
Health	0.016	0.017	329,686
Character and virtue	-0.013	0.018	329,706
Close relationships	-0.013	0.020	329,340
Financial stability	-0.083***	0.026	330,636
Full flourishing index	-0.016	0.012	320,888

Notes.—Outcomes retain their original 0–10 scales. All models include respondent and study-site-by-wave fixed effects and respondent-clustered standard errors. The financial domain directly asks about material security and monthly expenses, so its negative interaction should not be generalized to the nonfinancial domains. *** $p < 0.01$.

Figure A.3: Income-by-Childhood-Formation Interactions by Flourishing Domain



Notes.—Points report the interaction between log household income and childhood religious formation from respondent fixed-effects models. Bars show 95 percent confidence intervals based on respondent-clustered standard errors.

7.7 Childhood-environment overlap

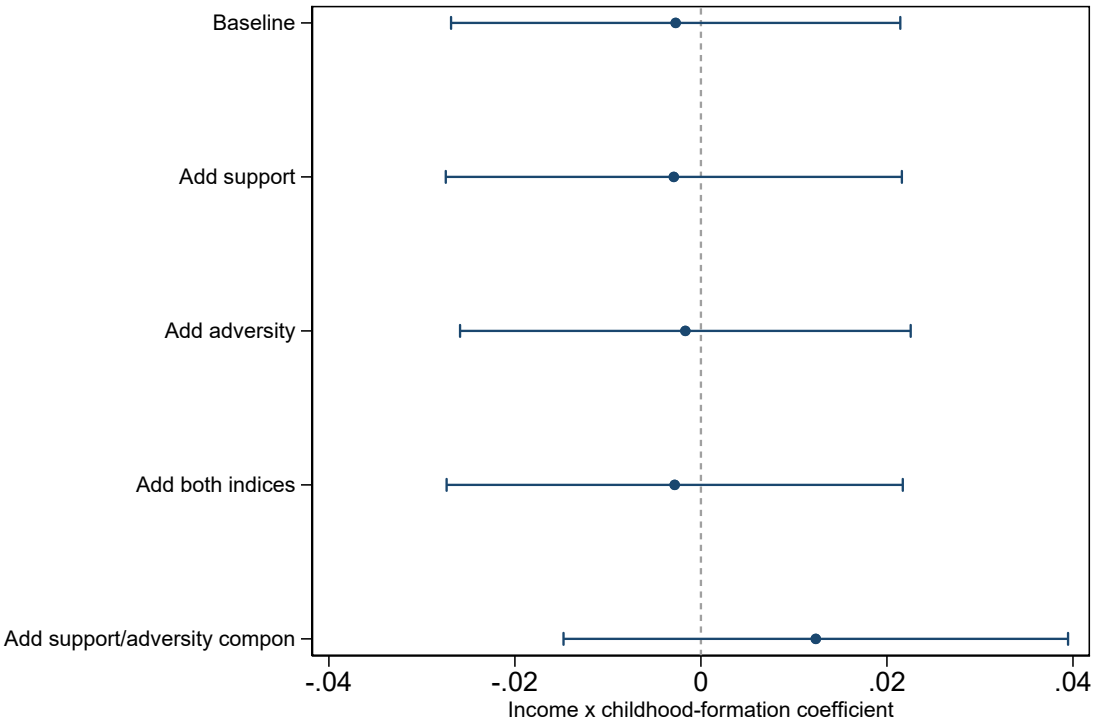
The overlap exercise in Table A.15 asks whether the income interaction changes when the recalled supportive and adverse childhood environments are allowed to shift the income slope. It does not: the interaction is nearly unchanged when the indices enter separately or together. Adding the eight component measures used to construct the support and adversity indices yields a small positive estimate, but the sample changes and the coefficient remains statistically indistinguishable from zero. Figure A.4 summarizes this pattern. Because religion and childhood experiences are recalled in the same survey, these estimates are not identified mediation effects.

Table A.15: Childhood-Environment Controls and the Income Interaction

Specification	Interaction	Standard error	Person–waves
Baseline	-0.003	0.012	321,963
Add supportive-childhood index	-0.003	0.013	316,965
Add adverse-childhood index	-0.002	0.012	321,771
Add both indices	-0.003	0.013	316,965
Add support/adversity components	0.012	0.014	278,310

Notes.—Each row includes respondent and study-site-by-wave fixed effects. The additional childhood variables enter as interactions with log household income because their levels are absorbed by respondent fixed effects. Rows 2–4 add the supportive-childhood index, the adverse-childhood index, or both. The final row instead adds the eight component measures used to construct those indices: childhood finances and health, parental structure, parental relationships and felt love, and outsider status. It does not add foreign-born status or parental attendance, which enter the separate 11-variable slope-control specification in Table 6. Standard errors are clustered by respondent.

Figure A.4: Sensitivity of the Income Interaction to Childhood-Environment Controls



Notes.—The figure plots the income-by-childhood-formation coefficient across the specifications reported in Table A.15. Bars show 95 percent confidence intervals based on respondent-clustered standard errors.

7.8 Study-site estimates and observed selection

Table A.16: Study-Site-Specific Income-by-Childhood-Formation Interactions

Study site	Interaction	Standard error	Person–wave observations	Income movers
Nigeria	-0.160	[0.128]	7,916	3,097
India	-0.098	[0.054]	17,629	6,522
Germany	-0.089	[0.058]	15,146	4,253
Argentina	-0.071	[0.045]	7,550	2,696
Egypt	-0.050	[0.035]	6,306	2,532
United States	-0.045	[0.036]	84,170	16,772
Philippines	-0.042	[0.078]	8,259	2,789
Australia	-0.041	[0.080]	7,293	1,783
United Kingdom	-0.025	[0.054]	9,640	2,725
Indonesia	-0.017	[0.081]	7,480	2,475
Poland	-0.015	[0.135]	13,673	4,696
Brazil	-0.010	[0.068]	11,411	3,677
South Africa	0.013	[0.120]	2,041	739
Spain	0.014	[0.054]	7,869	2,535
Sweden	0.017	[0.081]	30,413	8,329
Japan	0.025	[0.056]	34,269	10,262
Türkiye	0.027	[0.085]	1,265	483
Mexico	0.050	[0.069]	5,435	1,834
Tanzania	0.095	[0.065]	15,475	5,165
Israel	0.118	[0.215]	5,498	837
Kenya	0.139	[0.077]	21,471	7,493
Hong Kong	0.416	[0.275]	1,754	397
Inverse-variance pooled	-0.022	[0.013]	321,963	92,091

Notes.—Each row estimates Equation (13) within a study site, with respondent and wave fixed effects and respondent-clustered standard errors. Income movers are respondents observed in at least two waves whose harmonized income category changes. The pooled row uses inverse sampling-variance weights and does not account for cross-site heterogeneity.

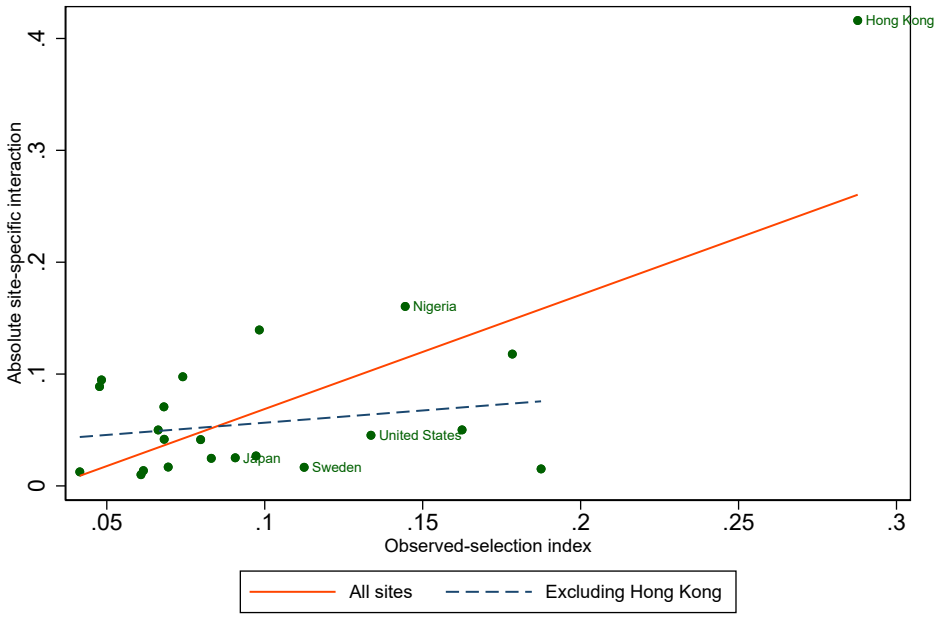
Table A.16 reports the study-site estimates underlying Figure 4. The estimates range from -0.160 in Nigeria to 0.416 in Hong Kong; none individually excludes zero at the 5 percent level. An inverse-variance weighted average equals -0.022 with a standard error of 0.013. This pooled summary is slightly negative but remains modest in magnitude and should not be interpreted as a common causal effect.

Table A.17: Observed Selection and Cross-Site Interaction Magnitudes

	All sites	Exclude Hong Kong
Observed-selection index	1.020	0.218
	[0.835]	[0.314]
Study sites	22	21
R-squared	0.465	0.045

Notes.—The dependent variable is the absolute value of the study-site-specific income-by-childhood-formation interaction. The regressor is the mean absolute standardized difference in the observed childhood covariates between weekly and less-frequent childhood attendees. Brackets contain heteroskedasticity-robust standard errors. With only 22 study sites, the exercise is descriptive and highly sensitive to leverage.

Figure A.5: Observed Selection and the Magnitude of Study-Site-Specific Interactions



Notes.—The horizontal axis is the study-site observed-selection index and the vertical axis is the absolute income-by-childhood-formation interaction. The fitted relationship is sensitive to Hong Kong, which combines the largest observed-selection index with the largest interaction magnitude.

7.9 Later-wave outcome availability

Finally, Table A.18 tests whether childhood religious formation predicts availability of the complete nonfinancial-flourishing outcome in Waves 2 or 3 among Wave-1 respondents. The estimated differences are 0.4 and 0.3 percentage points, respectively, and neither is statistically distinguishable from zero. These indicators combine panel retention with item availability, so they should not be interpreted as pure response-status measures. Attrition and item nonresponse can still depend on unobserved characteristics, but differential later-wave outcome availability by the focal childhood measure is small after conditioning on the observed baseline covariates.

Table A.18: Childhood Religious Formation and Later-Wave Outcome Availability

	Wave 2 outcome observed	Wave 3 outcome observed
Childhood religious formation	0.004	0.003
	[0.007]	[0.008]
Wave-1 respondents	145546	145546
R-squared	0.117	0.103

Notes.—The sample is Wave-1 respondents. The dependent variables indicate whether the respondent has a nonmissing complete nonfinancial-flourishing outcome in Wave 2 or Wave 3; they therefore combine panel retention and item availability. Models use the normalized Wave-1 survey weight and include the full childhood-control vector, gender, and study-site-by-five-year-birth-cohort fixed effects. Brackets contain standard errors clustered by study-site-by-cohort cell.