**The Behavioral Ecology of Moral Dilemmas: Childhood Unpredictability, but Not Harshness, Predicts Less Deontological and Utilitarian Responding**

**Supplemental Materials**

**Process Dissociation**

Process dissociation (PD; Jacoby, 1991) is a content-agnostic procedure that can be applied to any domain in which conventional methods conflate the measurement of two response patterns underlain by largely dissociable psychological processes (for review, see Payne & Bishara, 2009; for examples of other applications, see Damian & Sherman, 2013; Ferreira, Garcia-Marques, Sherman, & Sherman, 2006; Payne, 2001). Conway and Gawronski (2013) adapted PD to quantify independently the strength of utilitarian and deontological response inclinations. To use PD, researches must collect responses to both *incongruent* trials where the two tendencies conflict (i.e., predict divergent responses) and *congruent* trials where the two tendencies correspond (i.e., predict identical responses). Incongruent dilemmas approximate conventional high-conflict dilemmas (Koenigs et al., 2007) because causing harm maximizes outcomes, such that responses indicate a relative contest between responses consistent with utilitarian versus deontological ethics. Examples include killing a baby to save a village from death, testing toxic chemicals on animals to cure AIDS, torturing a man to stop a deadly explosive, and killing a mother and child to save a group of schoolchildren. Congruent dilemmas are similar in structure and wording to incongruent dilemmas but they describe harm that is harder to justify on utilitarian grounds. Examples include killing a baby to save a village from hard labor, testing toxic chemicals on animals to cure the common cold, torturing a man to stop a nonlethal paint bomb, and killing a mother and child to save a single old lady. That is, to preclude trivial decision making, causing harm on congruent dilemmas causes some benefit, but not so much that the harm is worth the benefit in improving overall wellbeing, or maximizing total outcomes. Hence, congruent dilemmas allow researchers to distinguish between people who accept harm only when harm maximizes overall outcomes (i.e., utilitarian responding) from people who accept harm regardless of the consequences (who tend to score higher in psychopathy, egoism, and willingness to commit ethical violations, e.g., Conway et al., 2018).

PD allows researchers to algebraically estimate the strength of two parameters by applying participant responses on congruent and incongruent dilemmas to a processing tree (see Figure 1S). The deontology (D) parameter reflects the tendency to reject harm in all cases and the other utilitarian (U) parameter reflects the tendency to cause harm only when doing so maximizes outcomes. Although both parameters tend to correlate strongly with conventional relative dilemma judgments, the parameters are typically uncorrelated or mildly positively correlated with each other (for meta-analysis, see Friesdorf, Conway, & Gawronski, 2015), suggesting they reflect two independent response tendencies that jointly contribute to conventional dilemma judgments.

Findings using the deontological and utilitarian PD parameters roughly accord with tenets of dual process theory: the D parameter appears to primarily reflect affective reactions to harm and the U parameter appears to primarily reflect cognitive evaluations of outcomes, although there is more complexity as well (e.g., Byrd & Conway, 2019; Reynolds & Conway, 2018). For instance, empathic concern and perspective taking predict the deontology parameter and need for cognitive predicts the utilitarian parameter (Conway & Gawronski, 2013). Cognitive load reduces utilitarian but not deontological inclinations, whereas enhancing the salience of harm increases deontological but not utilitarian inclinations (Conway & Gawronski, 2013). Notably, consistent with the idea that although the dual process account is largely accurate, the picture is more complex: some reflective processing predicts deontological tendencies (e.g., Gamez-Djokic & Molden, 2016; McPhetres. Conway, Hughes, & Zuckerman, 2018), some emotional concerns predict utilitarian tendencies (e.g., Reynolds & Conway, 2018), and some higher-order processes, such as strategic self-presentation, predict both (Rom & Conway, 2018). Nonetheless, the preponderance of evidence suggests that deontological responses reflect relatively more affective processing focused on harmful actions, while utilitarian responses reflect relatively more deliberative cognitive processing focused on outcomes.

Underscoring the importance of pulling apart sacrificial dilemma response tendencies is the consistent finding that when both parameters correlate positively with another variable or are similarly affected by a manipulation, they cancel each other out on conventional relative measures—*suppression* occurs (e.g., moral identity internalization, Conway & Gawronski, 2013; aversion to witnessing harm, Reynolds & Conway, 2018; moral conviction about harm, Conway et al., 2018; Hayakawa et al., 2017; Muda et al., 2017). The degree to which such effects cancel out depends on the relative strength of the associations with each parameter: when these relationships are roughly equal, a null relationship with the conventional measure emerges. If the relationship with the D parameter is larger than that with the U parameter, relationship with conventional judgments will be negative—lower utilitarianism. If the relationship with the U parameter is larger than that with the D parameter, relationship with conventional judgments will be positive—higher utilitarianism.

To calculate the PD parameters, we plugged responses on both congruent and incongruent dilemmas into a processing tree (see Figure 1S) to algebraically derive two parameters that independently track utilitarian response inclinations (consistently maximizing outcomes) and deontological inclinations (consistently rejecting causing harm). Specifically, we used the following formulas: *U* describes the case where utilitarianism drive judgments, and *D* describes the case where deontology drives judgments. Therefore, 1 – U describes the case where utilitarianism does not drive judgments and 1 – D describes the case where deontology does not drive judgments. Thus, the probability of judging harm as unacceptable on congruent dilemmas is represented by the case where either (a) utilitarianism drives the response or (b) if utilitarianism does not, deontology drives the response. This can be solved for algebraically:

*p*(unacceptable | congruent) = U + [(1 – U) × D] (1)

Conversely, the probability of judging harm as acceptable on congruent dilemmas can be solved for algebraically, as well, by considering the cases where neither utilitarianism nor deontology drives judgments:

*p*(acceptable | congruent) = (1 – U) × (1 – D) (2)

Furthermore, we can solve for the probability of rejecting harm on incongruent dilemmas, which reflects deontological but not utilitarian tendencies:

*p*(unacceptable | incongruent) = (1 – U) × D (3)

Finally, the probability of accepting harm on incongruent dilemmas reflects either utilitarian tendencies, or the absence of both utilitarian and deontological tendencies:

*p*(acceptable | incongruent) = U + [(1 – U) × (1 – D)] (4)

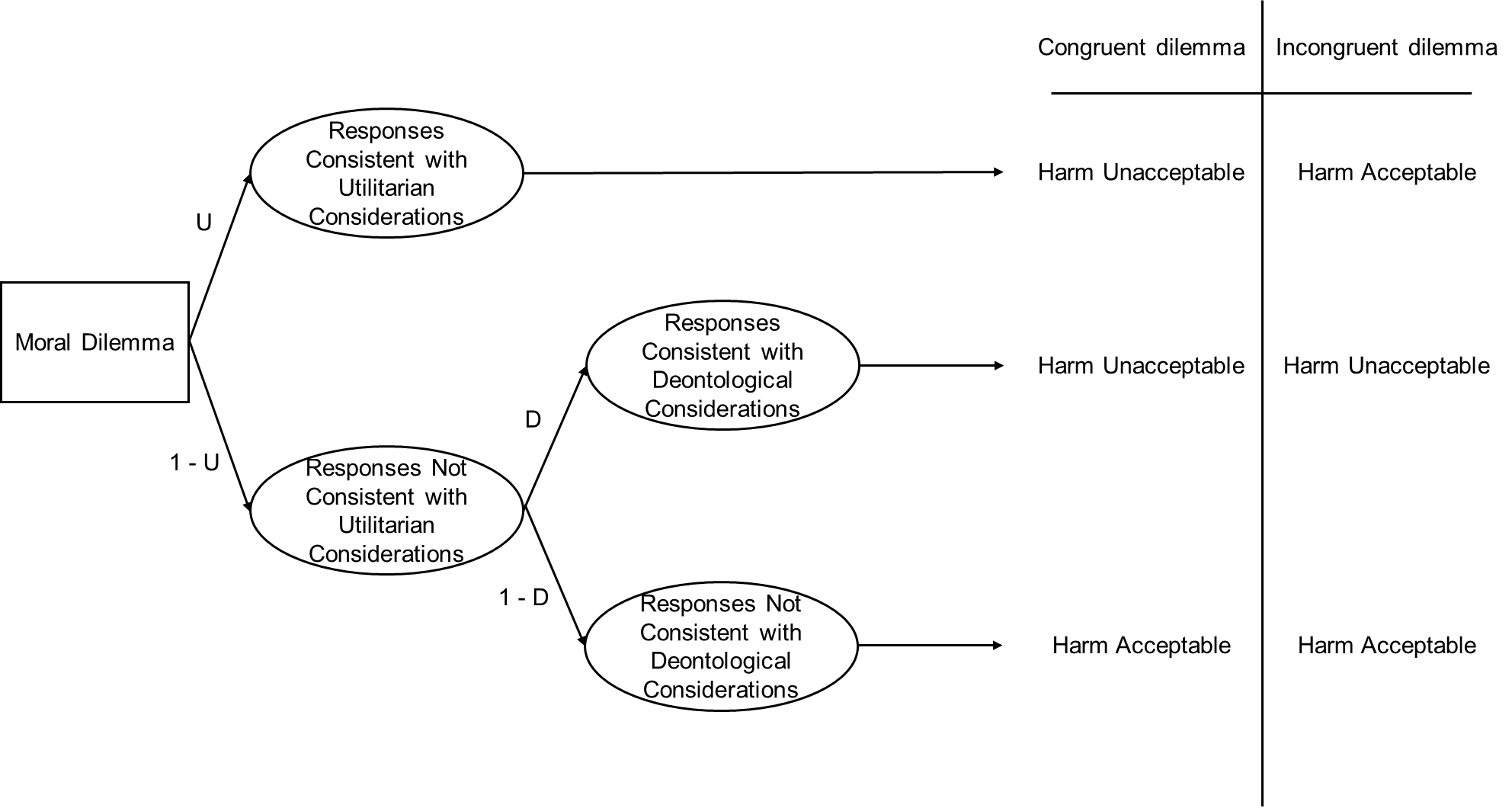
Therefore, by calculating the probability of accepting and rejecting harm on congruent and incongruent dilemmas, we can combine formulas to solve for utilitarian and deontological response tendencies using formulas (5) and (6). In formula (5), subtracting the probability of rejecting harm on incongruent dilemmas from the probability of rejecting harm on congruent dilemmas indicates the tendency to always maximize outcomes regardless of whether doing so entails causing harm— the utilitarian parameter (U).

U = *p*(unacceptable| congruent) *– p*(unacceptable | incongruent) (5)

Having obtained U, we can divide the probability of rejecting harm on incongruent dilemmas by the inverse of U, to estimate the tendency to always reject causing harm regardless of whether doing so maximizes outcomes or not—the deontology parameter (D).

D = *p*(unacceptable| incongruent)/(1 – U) (6)

Employing these formulae provides independent estimates of utilitarian and deontological response tendencies for each participant while also allowing measurement of conventional dilemma judgments where higher scores reflect more utilitarian responses and lower scores reflect more deontological responses. That is, conventional dilemma responses are the proportion of decisions to accept harm across the incongruent dilemmas, where causing harm maximizes outcomes, as is the standard practice (e.g., Kahane et al., 2015). Such responses reflect the relative strength of tendencies to reject harm versus maximize outcomes. Hence, higher acceptance of sacrificial harm may reflect either lower concern about causing harm or more concerns about maximizing outcomes, or both. Moreover, such analyses are insensitive to cases of suppression, where a predictor is associated with increases or decreases in both harm-rejection (deontological) and outcome-maximizing (utilitarian) response tendencies. Cases of suppression are common in dilemmas (e.g., Reynolds & Conway, 2018; Conway et al., 2018). Hence, PD analyses typically provide additional nuance to clarify ambiguous results produced by conventional responses. The deontology and utilitarian parameters typically correlate strongly (negatively and positively, respectively) with the conventional relative dilemma judgments, whereas they are uncorrelated or weakly correlated with one another (see meta-analysis by Friesdorf et al., 2015).



*Figure 1S.*

Processing tree illustrating components leading to harm-rejection and harm-acceptance judgments in congruent and incongruent moral dilemmas.The paths from left to right depict the cases that (a) responses are consistent with utilitarianism, (b) responses are consistent with deontology, or (c) responses are consistent with neither utilitarianism nor deontology.

**Supplemental Analyses**

**Study 1**

**Conventional Sacrificial Moral Dilemma Analyses**

Conventional judgments correlated positively with childhood unpredictability, *r* = .26, *p* < .001 but not harshness, *r* = -.03, *p* = .677. However, a process dissociation analysis revealed a more nuanced pattern (see main manuscript).

**Life History Strategy Analyses**

The effects of childhood unpredictability and harshness on downstream social processes have largely been documented through the lens of life history theory (e.g., Belsky et al., 1991; Ellis et al., 2009), so we included existing measures of life history strategies in the first few studies. Life history theory suggests that the constellations of biological, cognitive, and social strategies that emerge from early calibration processes fall on a continuum from *fast* to *slow*. Unpredictable and harsh environments give rise to relatively fast strategies that facilitate expedient extraction of resources from the physical and social environment, whereas predictability and resource abundance gives rise to relatively slower strategies that facilitate long term investment in the physical and social environment.

Prior research suggests that faster strategies are associated with higher levels of self-focused individual differences, such as desire for revenge, avoidant attachment (Figueredo et al., 2005, 2018), and lower levels of other-focused individual differences, such as communitarian beliefs (Figueredo et al., 2007), and emotional intelligence (Figueredo, Andrzejczak, Jones, Smith-Castro, & Montero-Rojas, 2011). Accordingly, people with faster, versus slower, life history strategies give less support to, get less support from, and experience lower quality relationships with their friends and family (Figueredo et al., 2007; Olderbak & Figueredo, 2009). In extreme cases, fast life history strategies can predict intimate partner violence (Barbaro & Shackelford, 2019; Szepsenwol, Zamir, & Simpson, 2019), largely mediated by selfish tendencies and aggression (Figueredo et al., 2018).

People with fast, versus slow, life history strategies less often consider future consequences of expedient resource extraction, which adversely affects social partners and the group: they are more likely to lie, cheat, steal, and commit violent crimes, largely due to lower self-control (Dunkel et al., 2013; Figueredo & Jacobs, 2010; Rushton & Whitney, 2002). In contrast, slow, but not fast, life history strategies predict stronger moral intuitions, including the five moral foundations (i.e., harm/care, fairness, loyalty, authority, purity; Gladden & Cleator, 2017), moral disgust sensitivity, and moral dumbfounding (Gladden, Welch, Figueredo, & Jacobs, 2009), or the insistence that an intuitively bothersome act is morally reprehensible even when no reasons can be given (see Haidt & Hersh, 2001). Hence, we expected that fast life history strategies would be negatively related to both deontological and utilitarian response tendencies.

**Method.**

***Materials****.* Participants completed the mini-K, which aims to measure psychosocial attributes of life history strategy (Figueredo et al., 2006). Participants responded to 20 items, which tap insight and planning, relationships with and support from family and friends, and religiosity on scales from -3 (*disagree strongly*) to +3 (*agree strongly*): for example, *I avoid taking risks, I often get emotional support and practical help from my blood relatives,* and *I am closely connected to and involved in my religion*. We converted scores to range from 1 to 7, reverse coded items, and averaged across items such that higher scores represent relatively faster life history strategies (*M* = 2.78, *SD* = .91, α = .90).

**Results and discussion.**

In contrast to predictions, fast life history on the mini-K correlated positively with both parameters, such that people who reported a relatively fast (vs. slow) life history strategy appeared more concerned with both avoiding harm andmaximizing outcomes (See Table 1S). The association between life history strategy and the parameters held when controlling for adult income, age, gender, and the other parameter: D parameter, *b* = .23, *SE* = .07, *t*(246) = 3.57, *p* < .001, CI95[.104, .362], and U parameter, *b* = .14, *SE* = .07, *t*(246) = 2.05, *p* = .041, CI95[.006, .279]. Conventional acceptance of sacrificial judgments correlated negatively with fast life history strategy (*r* = -.17, *p* = .007), but the PD analysis suggested this reflects partial suppression of two positive effects of different sizes. However, prior work has challenged the assumption that the mini-k life history scale serves as an appropriate measure of life history on both conceptual and psychometric grounds (e.g., Copping et al., 2014). Confidence in these findings requires replication using an improved life history measure– one goal of Study 2.

Table 1S

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | **1** | **2** | **3** | **4** | **5** | **6** | **7** |
| **1. Utilitarian PD Parameter** | — |  |  |  |  |  |  |
| **2. Deontology PD Parameter** | **.30\*\*\*** | — |  |  |  |  |  |
| **3. Childhood Unpredictability** | **-.39\*\*\*** | **-.46\*\*\*** | — |  |  |  |  |
| **4. Childhood Harshness** | -.03 | .03 | **.12\*** | — |  |  |  |
| **5. Fast Life History Strategy** | **.17\*\*** | **.24\*\*\*** | -.09 | .12† | — |  |  |
| **6. Adult Income** | .08 | -.01 | **-.17\*\*** | **-.43\*\*\*** | -.12† | — |  |
| **7. Gender**  **(*f*=1, *m*=2)** | -.11† | **-.15\*** | .04 | .08 | -.02 | .02 | — |
| **8. Age** | **.14\*** | **.28\*\*\*** | **-.23\*\*\*** | **.21\*\*** | -.04 | .01 | **-.18\*\*** |

*Correlations Among Variables, Study 1.*

*Note*: †*p* < .10, \**p <* .05, \*\**p <* .01, \*\*\**p* < .001

**Study 2**

**Conventional Sacrificial Moral Dilemma Analyses**

Conventional judgments did not correlate with childhood unpredictability, *r* = -.01, *p* = .870, or harshness, *r* = -.07, *p* = .271, but again process dissociation revealed a more nuanced pattern (see main manuscript).

**Life History Strategy Analyses**

Study 2 entailed a replication of Study 1 with the addition of longer versions of the life history strategy scale, which tends to demonstrate stronger psychometric properties (e.g., factor structure; see Figueredo et al., 2004; Figueredo et al., 2017). We also included some reverse-coded items in the life history measure to reduce the influence of random responding.

**Method.**

***Materials****.*

*Extended life history scale.* To more thoroughly capture life history strategy, we administered the 42-item short-form of the Arizona Life History Battery (K-SF-42; Figueredo et al., 2017). To reduce the effects of random responding, we included five reverse-coded items, such as *When I encounter problems, I tend to give up before I solve them* and *I do not contribute a great deal to the welfare and well-being of my blood relatives in the present.* Again, we coded items such that higher scores represent faster life history strategies. We standardized and averaged across all items from all subscales (i.e., insight, altruism, religiosity, attachment, parental relationship, family relationships, friendships) to produce the life history composite (*SD* = .61; reliability across items: α = .891).

*Developmental indicators of life history strategy.* A faster life history strategy is characterized by younger reproductive development and activity (Chisholm, Quinlivan, Petersen, & Coall, 2005; Xu, Norton, & Rahman, 2018). Hence, participants reported the age of first menarche (if they did not select male as gender; *M* = 13.01, *SD* = 1.70), sexual debut (*M* = 18.12, *SD* = 2.74), and first child (*M* = 26.28, *SD* = 4.87).

**Results and discussion.**

***Life history.*** In contrast to predictions and Study 1, life history strategy was not significantly related to either parameter (see Table 2S). This pattern held when controlling for adult income, age, gender, and the other parameter: LH did not predict the D parameter, *b* = .07, *SE* = .14, *t*(249) = 0.46, *p* = .649, CI95[-.218, .350], or the U parameter, *b* = .07, *SE* = .15, *t*(249) = 0.49, *p* = .623, CI95[-.215, .359]. It may be the case that components of the life history measure related to the parameters in opposing ways, such that they may cancel each other out (see exploratory analyses below). This time life history strategy also failed to significantly correlate with acceptance of sacrificial harm in conventional analyses (*r* = -.05, *p* = .391).

In exploratory analyses, we assessed the correlations among the parameters, the subscales of the K-SF-42, and the composite life history measure (see Table 3S). Recall that items were reverse scored, such that higher values of subscales represent the psychosocial markers of faster life history strategies – lower insight and planning, altruism, religiosity, secure attachment, and quality parent relationships with parents, family, and friends. These analyses reveal potential suppression: although low religiosity was associated with stronger utilitarian tendencies (i.e., the U parameter), lower insight and secure attachment were related with weaker utilitarian tendencies (likewise, parental relationship quality was marginally negatively related to the U parameter). No other subscales were significantly associated with utilitarian responding. Combining these effects produces a null effect of life history on utilitarian response tendencies.

The exploratory analysis also revealed suppression on the D parameter: social (friendship) relationship quality was positively associated with the D parameter, whereas secure attachment was negatively associated with the D parameter. No other relationships between the life history subscales and the deontology parameter were significant. Again, combining these effects results in suppression, as the omnibus association between the composite and the D parameter becomes nonsignificant. A few of the findings with the subscales of the life history measure fit with prior work. For example, religiosity often predicts lower U tendencies and higher D (e.g., Reynolds et al., 2019) and secure attachment predicts higher U and D (e.g., Maranges, Chen, & Conway, 2020). However, these relationships cancel each other out when included in the theoretically-derived life history scale.

***Developmental indicators of life history****.* Contrary to predictions, sexual debut was negatively related to the U parameter (*r* = -.21, *p* = .002), such that people who had sexual intercourse for the first time at an older age (i.e., slow strategy) less often maximized outcomes. This association held when controlling for the other parameter, adult income, age, and gender, *b* = -.07, *SE* = .02, *t*(216) = -2.57, *p* = .011, CI95[-.109, -.014]. Sexual debut was unrelated to the D parameter (*r* = -.09, *p* = .184). Age of first menarche was unrelated to the U (*r* = -.10, *p* = .219) and D (*r* = -.14, *p* = .106) parameters. Likewise, age of first childbirth was unrelated to the U (*r* = .14, *p* = .229) and D (*r* = -.02, *p* = .862) parameters. There were no other associations between developmental indicators and dilemma responding beyond age of sexual debut and utilitarian response tendencies. Other work examining life history strategies and morality (e.g., the moral foundations, Gladden & Cleator, 2017) also found no association with developmental markers of life history.

Table 2S

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | **1** | **2** | **3** | **4** | **5** | **6** | **7** |
| **1. Utilitarian PD Parameter** | — |  |  |  |  |  |  |
| **2. Deontology PD Parameter** | **.23\*\*\*** | — |  |  |  |  |  |
| **3. Childhood Unpredictability** | **-.39\*\*\*** | **-.25\*\*\*** | — |  |  |  |  |
| **4. Childhood Harshness** | -.09 | -.03 | **.34\*\*\*** | — |  |  |  |
| **5. Fast Life History Strategy** | -.06 | .00 | **.39\*\*\*** | **.40\*\*\*** | — |  |  |
| **6. Adult Income** | **.20\*\*** | .12† | **-.28\*\*\*** | **-.46\*\*\*** | **-.37\*\*\*** | — |  |
| **7. Gender**  **(*f*=1, *m*=2)** | -.10† | **-.16\*\*** | .10 | **.22\*\*\*** | **.20\*\*** | **-.18\*\*** | — |
| **8. Age** | **-.13\*** | **.13\*** | **.21\*\*\*** | **.28\*\*\*** | **.21\*\*\*** | **-.21\*\*** | .10 |

*Correlations Among Primary Variables, Study 2.*

*Note*: †*p* < .10, \**p <* .05, \*\**p <* .01, \*\*\**p* < .001

Table 3S

|  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** |
| 1. **Utilitarian PD Parameter** | — |  |  |  |  |  |  |  |  |
| 1. **Deontology PD Parameter** | **.23\*\*\*** | — |  |  |  |  |  |  |  |
| 1. **Insight and Planning** | **-.14\*** | -.05 | — |  |  |  |  |  |  |
| 1. **Altruism** | .10 | .02 | **.18\*\*** | — |  |  |  |  |  |
| 1. **Religiosity** | **.21\*\*** | .04 | **.13\*** | **.37\*\*\*** | — |  |  |  |  |
| 1. **Secure Attachment** | **-.18\*\*** | **-.18\*\*** | **.30\*\*\*** | -.11 | -.11† | — |  |  |  |
| 1. **Parental Relationship Quality** | -.11† | -.01 | **.33\*\*\*** | **.36\*\*\*** | **.22\*\*\*** | -.02 | — |  |  |
| 1. **Familial Relationship Quality** | -.09 | .04 | **.33\*\*\*** | **.48\*\*\*** | **.21\*\*** | .06 | **.59\*\*\*** | — |  |
| 1. **Social Relationship Quality** | -.04 | **.13\*** | **.36\*\*\*** | **.36\*\*\*** | **.23\*\*\*** | .07 | **.38\*\*\*** | **.53\*\*\*** | — |
| 1. **K-SF-42** | -.06 | .00 | **.62\*\*\*** | **.61\*\*\*** | **.49\*\*\*** | **.29\*\*\*** | **.68\*\*\*** | **.77\*\*\*** | **.72\*\*\*** |

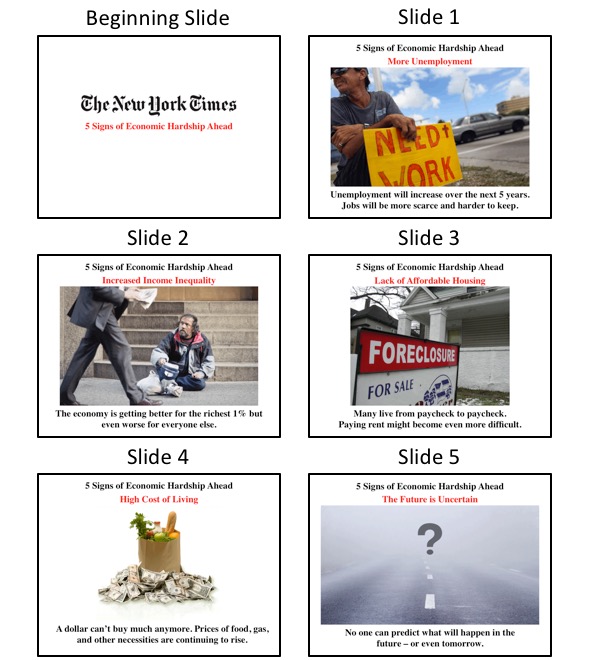
*Correlations among the utilitarian and deontology parameters, subscales of life history measure, and the composite life history measure (K-SF-42), Study 2.*

*Note*: †*p* < .10, \**p <* .05, \*\**p <* .01, \*\*\**p* < .001

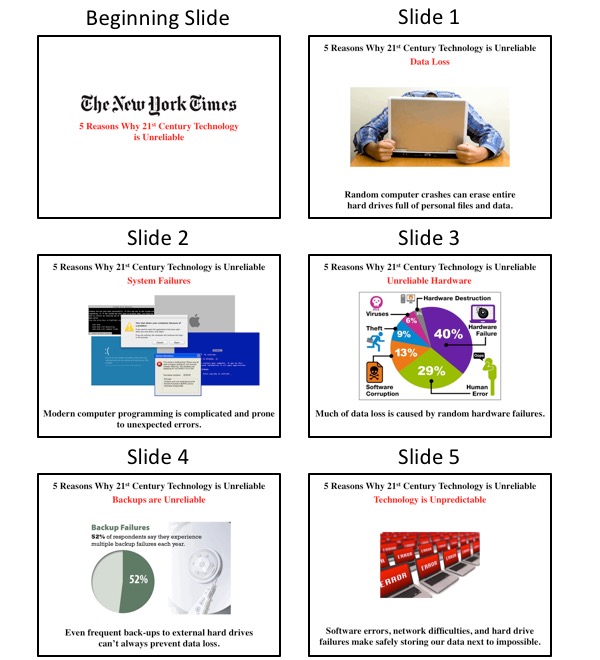
**Study 3a**

**Uncertainty Slide Show from Young et al. (2018)**

Uncertainty Slideshow



##### Control Slideshow



**Conventional Sacrificial Moral Dilemma Analyses**

As in Study 2, conventional sacrificial judgments were not correlated with childhood unpredictability, *r* = .043, *p* = .458, or harshness, *r* = .029, *p* = .621, but process dissociation revealed more nuance (see main manuscript).

**Life History Strategy Analyses**

**Method.**

***Materials****.* For developmental indicators of life history strategy,participants reported and the age at which they had their first menarche (if they did not select male as gender; *n* = 151, *M* = 12.29, *SD* = 1.51), age of sexual debut (*n* = 244, *M* = 17.95, *SD* = 2.79), and age when they had their first child (*n* = 142, *M* = 25.77, *SD* = 5.20).

**Results and discussion.**

There were no significant relationships between the PD parameters and reported the age of first menarche (U: *r* = -.14, *p* = .090; D: *r* = .01, *p* = .916), or sexual debut (U: *r* = -.10, *p* = .122; D: *r* = .09, *p* = .167), or of first child (U: *r* = .07, *p* = .406; D: *r* = .06, *p* = .485).

Conventional sacrificial dilemma judgments did not correlate with age of first menarche (*r* = -.06, *p* = .472), of sexual debut (*r* = -.11, *p* = .097), or of first child (*r* = -.01, *p* = .923). Thus, developmental markers of life history strategy may not be associated with moral dilemma decision making in adulthood. Other work demonstrates no significant relationships between individual differences in such markers individual differences in moral concerns about harm, fairness, loyalty, authority, purity, or liberty (Gladden & Cleator, 2017).

**General Discussion of Life History Findings**

Across Studies 1-3a, we did not find consistent relationships between psychometric or development indicators of overall life history strategy and moral dilemma decision making. One possible explanation is that moral decision making reflects specific aspects of a person’s life history strategy, rather than a global strategy. For example, religiosity was negatively but attachment security was positively related to utilitarian responding. Likewise, relationships with friends was negatively but attachment security was positively related to deontological responding. These relationships may have cancelled each other out. These findings fit with recent work suggesting that human life history strategies exist along more than a single psychometric dimension (Copping et al., 2014; Richardson et al., 2017). A second possible explanation is that psychometric measures, such as those used in these studies, do not always serve as valid assessments of overall life history strategy (Gruijters & Fleuren, 2018). Designing and implementing appropriate measures of life history strategy has been a challenge for the field, and measures may need to be tailored to the specific research questions being investigated.

There have also been several recent criticisms of the application of life history theory to humans (e.g., Baldini, 2015; Nettle, 2018; Zietsch & Sidari, 2019). Some of these criticisms have focused on the idea that applying inter-species differences to individual differences is a troublesome conceptual leap (Baldini, 2015), and that individual differences may not be organized into a coherent fast-versus-slow life history strategy continuum (Zietsch & Sidari, 2019). In support of this criticism, evidence from the animal literature suggests that inter-individual covariation of traits does not neatly adhere to a fast-slow continuum (e.g., Royauté, Berdal, Garrison, & Dochtermann, 2018). Other criticisms have highlighted that predictions derived from a LHT framework can be too broad, and should instead focus on a specific set of assumptions about the eliciting circumstances such as ecological or demographic variables (Nettle, 2018). Taken together, these criticisms suggest that researchers should use caution when interpreting the covariation between observed traits as evidence of a coherent life history strategy. Rather, it is sensible to predict and model processes that occur throughout development under specific assumptions and because of specific inputs.

**Study 3b**

**Conventional Sacrificial Moral Dilemma Analyses**

As in Study 3a, conventional sacrificial judgments did not correlate with either unpredictability (*r* = .043, *p* = .365) or harshness (*r* = -.071, *p* = .137). However, once again the PD analyses revealed additional nuance (see main manuscript).

**Study 4**

**Conventional Sacrificial Moral Dilemma Analyses**

As in Study 1, conventional sacrificial judgments correlated positively with unpredictability, *r* = .26, *p* < .001, but not harshness, *r* = -.10, *p* = .130. However, PD analyses revealed a more nuanced pattern (see main manuscript).

**Study 5**

**Scale Development: Overall Analytic Strategy**

To develop each of three new measures— childhood social unpredictability, childhood physical unpredictability, and childhood harshness— we reviewed extant literature for conceptualizations and operationalizations, developed a list of items, edited the list for redundancy and clarity, and conducted two pilot studies. The first pilot study allowed us to select the best items and the second to confirm the factor structure within each subscale. Specifically, to examine the factor structure of our new scales and to reduce the number of items to those which loaded most strongly, we conducted a set of principle axis factor analyses using oblimin rotation and a minimum eigenvalue of 2 in SPSS.

In Pilot Study 1, we collected data from 100 participants via MTurk (demographics left out of survey flow), with a final sample of 67 participants after excluding participants who did not complete the whole survey (*n* = 10) or pass the attention check (*I always pay attention to surveys so I will select somewhat disagree; n* = 23). For each construct, we conducted a serial EFA analysis using multiple rounds such that, in each round, we removed items that did not load on the primary factor(s[[1]](#footnote-1)) above .5 (unless they were part of the original Mittal et al., 2015 scale) and reran the factor analysis. This ensured that remaining items loaded highly on their intended factor. In Pilot Study 2, we collected data from 334 participants via MTurk. After excluding participants who failed the attention check (*n* = 63), our final sample included 271 individuals (123 females, 146 males, 2 other; *M*age = 35.10, *SD* = 11.31; 196 White, 46 Black, 19 Hispanic or Latino, 15 Asian, 5 Native American, 1 Middle Eastern, 1 ‘mixed race’). We again conducted a serial EFA analysis using multiple rounds such that, in each round, we removed items which did not load on the primary factor(s) above a more stringent standard of .6 (unless they were part of the original Mittal et al., 2015 scale) and reran the factor analysis. This ensured that remaining items loaded highly on their intended factor.

**Childhood social unpredictability.**To create a new, more comprehensive measure of childhood social unpredictability, we first examined operationalization of unpredictability in the prior literature. Social unpredictability in childhood has been operationalized as frequent changes or ongoing inconsistency in presence of caretakers, relationships between parents and step-parents, and between homes and schools, relationships with and behavior of caretakers and family, and daily routines (e.g., Belsky et al., 2012; Brumbach et al., 2009; Doom et al., 2016; Mittal & Griskevicius, 2014; Simpson et al., 2012). Second, based on that review and on the original 8-item scale (Mittal et al., 2015; Young et al., 2018), we created 45 new items, discussed them, discarded redundant items, and reworded remaining items for clarity, leaving 38 pilot items. Last, we conducted two pilot studies, which left 24 final items (see Table 4S).

In Pilot Study 1, we conducted three rounds of factor analyses and removed a total of 11 items [*I knew I could count on some family, friends, teachers, or neighbors to support me, I went to the same school long enough that the teachers got to know me, I knew how I would get home from school, I had reliable teachers and knew what to expect in the classroom, I was typically on time to my activities and obligations as a child (e.g., school, sports, functions), My parents held each of their jobs for a long time when I was growing up, We lived in the same house or two when I was growing up, I only lived with one parent growing up, My parents had the same career throughout my childhood, I really got to know my neighbors when I was young, I had the same friends year to year*]. Two primary factors emerged (eigenvalue of 12.91, accounting for 39.12% of variance and eigenvalue of 4.64, accounting for 14.07% of variance, mostly reverse-coded items). In Pilot Study 2, we began with 27 items and conducted two rounds of factor analyses, removing 3 items *(I had a certain group of friends I would talk to on a daily basis, I really got to know my neighbors when I was young,* and *I went to the same school long enough that the teachers got to know me*). We confirmed two factors emerged (eigenvalue of 13.56, accounting for 56.65% of the variance and an eigenvalue of 2.85, accounting for 11.86% of the variance; the latter reflected reverse-scored items) in the final version of the scale, comprised of 24 items (see Table 4S).

Table 4S

*New extended measure of Childhood Social Unpredictability.*

|  |
| --- |
| Item |
| 1. My parents went through multiple separations or divorces. |
| 1. I did not know when I would see my parent(s). |
| 1. People often moved in and out of my house fairly frequently.o |
| 1. When I woke up, I often didn't know what could happen in my house that day.o |
| 1. I had a hard time knowing what my parent(s) or other people in my house were going to say.o |
| 1. I could not predict which of many caretakers (e.g., babysitters, nannies, neighbors, family) would be watching me. |
| 1. I never knew whether my parents would be there to pick me up from school. |
| 1. We moved around a lot when I was a kid. |
| 1. I changed schools more frequently than my peers. |
| 1. I had a hard time focusing on school work at home because things were so chaotic. |
| 1. My family life was generally inconsistent and unpredictable from day-to-day.o |
| 1. My family often moved homes and schools when I was a child. |
| 1. My parents' work schedule changed from week to week. |
| 1. Things were often chaotic in my house.o |
| 1. My family environment was often tense and on edge.o |
| 1. I did not know what to expect from my family when I had friends over. |
| 1. My parents had a difficult divorce or separation during this time.o |
| 1. My siblings' behavior was erratic (i.e., hard to predict). |
| 1. My parent(s) frequently had arguments or fights with each other or other people in my childhood.o |
| 1. I often did not know what to expect from other students at school. |
| 1. I always knew where to find help as a kid. |
| 1. My family had a consistent schedule, so I knew what to expect each day.\* |
| 1. I had regularly scheduled activities outside of school.\* |
| 1. I had a stable schedule from day to day.\* |

*Note:* \*reversed ofrom original scale

**Childhood physical unpredictability.**Prior work in the human life history theory literature has focused almost exclusively on social aspects of young children’s environments. Hence, in order to create a new measure of childhood physical unpredictability, we first looked to prior operationalizations of environmental unpredictability in animal biology. That literature has focused on the unpredictability of resources, the unpredictable behavior of predators (Pratt 1993), and the unpredictability of weather and weather events (Dingemanse et al., 2004). Taken together, physical unpredictability has been operationalized as the extent to which environmental harshness varies over time and space (e.g., Ellis et al., 2009; Roff, 2002). Second, based on that review of the previous operationalizations, we generated a list of potential items tapping into aspects of physical unpredictability for humans. We created 20 new items, discussed them, discarded redundant items, and reworded remaining items for clarity, leaving 13 pilot items. Last, we conducted two pilot studies, which left 12 final items (see Table 5S).

In Pilot Study 1, we conducted one round of factor analyses and removed one item for redundancy (*I never knew what the weather was going to be like)*]. One primary factor emerged (eigenvalue of 6.86, accounting for 57.18% of variance). In Pilot Study 2, we began with 12 items and conducted one round of factor analyses, removing no items. We confirmed that a single factor emerged (eigenvalue of 8.78, accounting for 73.16% of the variance) in the final version of the scale, comprised of 12 items (see Table 5S).

Table 5S

*New extended measure of Childhood Physical Unpredictability.*

|  |
| --- |
| Item |
| 1. I was never certain where it was safe to play. |
| 1. The traffic around the house(s) I lived was unpredictable and chaotic. |
| 1. I often got lost as a child. |
| 1. I wasn't always sure if our water was safe to drink. |
| 1. There was a lot of change in the structure (e.g., buildings, signs) of my neighborhood. |
| 1. I experienced extreme, unexpected weather events when I was a kid (e.g., volcano erupting, earthquake, tsunami). |
| 1. It wasn't always easy to get to the store to buy food. |
| 1. I never knew when I would have money for lunch at school. |
| 1. When I left my house I was never quite certain what would happen in my neighborhood. |
| 1. There were often loud noises (e.g., from construction, trains) in my neighborhood. |
| 1. I had to worry about getting hit by a car when I was a kid. |

|  |
| --- |
| 1. The stores where we shopped only sometimes had high quality food. |

**Childhood harshness*.*** To create a new measure of childhood harshness, we first looked to prior psychology literature’s operationalization of harshness. Harshness in childhood has been operationalized as low levels of monetary resources (e.g., Belsky et al., 2012; Brumbach et al., 2009; Doom et al., 2016; Mittal & Griskevicius, 2014; Simpson et al., 2012), which precludes purchasing of essential and nonessential goods and linearly relates to all forms of morbidity and mortality (see Adler et al., 1993; Ellis et al., 2009; Pepper & Nettle, 2017). Second, based on that review and on the original 4-item scale (Mittal et al., 2015; Young et al., 2018), we created 40 new items, discussed them, discarded redundant items, and reworded remaining items for clarity, leaving 31 items for pilot testing. Last, we conducted two pilot studies, which left 28 final items (see Table 6S).

In Pilot Study 1, we conducted two rounds of factor analyses and removed three items: one that did not meet our criteria (*Every school year, I got new school supplies like a new backpack or lunch box if I needed them)* and two redundant items (*Money was tight in my family*  and *When I was young, there was not enough money to go around*). Two primary factors emerged (eigenvalue of 9.13, accounting for 30.42% of variance and eigenvalue of 6.35, accounting for 21.17% of variance, mostly reverse-coded items). In Pilot Study 2, we began with 28 items and conducted one round of factor analyses, removing no items. We confirmed two factors emerged (eigenvalue of 10.42, accounting for 37.20% of the variance and an eigenvalue of 8.10, accounting for 28.92% of the variance, which included reverse-scored items) in the final version of the scale, comprised of 28 items (see Table 6S).

Table 6S

*New extended measure of Childhood Harshness.*

|  |
| --- |
| Item |
| 1. We rarely had guests over to our home due to financial strain. |
| 1. Most of my clothes were hand-me-downs from older siblings, extended family, or friends. |
| 1. Despite how much my parents worked, my family rarely had enough money for luxury items. |
| 1. My family rarely had enough money to go out for a nice dinner. |
| 1. If not for free meals at school, I may not have eaten every day. |
| 1. Growing up, I rarely got spoiled because money was so tight. |
| 1. My family was strained financially. |
| 1. I couldn't do extracurricular school activities if they cost money. |
| 1. There were some nights my parents had to go without a meal in order to keep me (and/or my siblings) well-fed. |
| 1. My house wasn't very nice growing up. |
| 1. I usually was forced to share my personal things with other members of my family. |
| 1. I felt uncomfortable asking my parents for money because money was tight. |
| 1. Sometimes we lost access to heating, water, or electricity because of overdue bills. |
| 1. We had to try to save money when shopping for anything. |
| 1. I never had the newest style of shoes or clothes. |
| 1. My family and I were usually able to purchase expensive presents for holidays, birthdays, etc.\* |
| 1. I felt relatively wealthy compared with other kids in my school.o\* |
| 1. My family rarely had to worry about money when I was a kid.\* |
| 1. I often got the newest toys when I was growing up.\* |
| 1. I wore brand name shoes and clothes.\* |
| 1. I grew up in a relatively wealthy neighborhood.o\* |
| 1. If something of mine broke, my family was usually able to replace it.\* |
| 1. My caregivers typically got me any food I wanted from the grocery store.\* |
| 1. My family usually had enough money for things when I was growing up.o\* |
| 1. I was able to go on all school field trips no matter the cost. \* |
| 1. We were usually able to afford going away on vacations during school breaks and holidays. \* |
| 1. My parents were able to create a college fund or investment account for me. \* |
| 1. What was your family's household income?o\* |

*Note:* \*reversed ofrom original scale

**Results**

**Regression analyses.**

***Conventional Sacrificial Moral Dilemma Analyses.*** Similar to Studies 1 and 4, conventional sacrificial judgments correlated positively with social unpredictability, *r* = .20, *p* = .005, and physical unpredictability, *r* = .19, *p* = .005, but were unrelated to childhood harshness, *r* = .06, *p* = .373, but once again process dissociation revealed more nuance.

***Social vs. Physical Unpredictability.*** We initially expected that social unpredictability would be a stronger predictor of moral dilemma decision making than physical unpredictability. However, these measures are so highly correlated (*r* = .84, *p* < .001) and display moderate multicollinearity (tolerance = .295, VIF = 3.39) that models including both are not easily interpreted. Nevertheless, we provide those analyses here for the curious reader, cautioning interpretation.

We entered both childhood social and physical unpredictability as predictors into two regression models (one for each PD parameter), controlling for childhood harshness, adult income, age, gender, and the other parameter. The effect of childhood social unpredictability became nonsignificant for the D parameter, *b* = -.14, *SE* = .09, *t*(209) = -1.56, *p* = .121, CI95[-.495, -.262], and U parameter *b* = -.03, *SE* = .09, *t*(209) = -.38, *p* = .703, CI95[-.211, .143]. Childhood physical unpredictability continued to significantly predict both the D parameter, *b* = -.25, *SE* = .07, *t*(209) = -3.46, *p* = .001, CI95[-.395, -.108], and the U parameter, *b* = -.36, *SE* = .07, *t*(209) = -4.75, *p* < .001, CI95[-.475, -.196].

***Composite unpredictability.*** We also explored the association between a composite unpredictability measure and the PD parameters, controlling for demographic variables and also the potential confounds. To create this composite, we averaged across all items from the social unpredictability and physical unpredictability measures (*M* = 3.31, *SD* = 5.86, α = .97). Correlation analyses suggest that broad childhood environmental unpredictability is associated with both the U parameter, *r* = -.43, *p* < .001, and the D parameter, *r* = -.46, *p* < .001. These associations held when controlling for childhood harshness, adult income, age, gender, and the other parameter: D parameter, *b* = -.38, *SE* = .065, *t*(210) = -6.41, *p* < .001, CI95[-.495, -.261], and U parameter, *b* = -.36, *SE* = .06, *t*(210) = -5.91, *p* < .001, CI95[-.480, -.240].

When controlling for political conservativism, extroversion, neuroticism, conscientiousness, agreeableness, social desirability and the other parameter, broad childhood social unpredictability continued to negatively predict the D parameter, *b* = -.21, *SE* = .05, *t*(191) = -4.08, *p* < .001, CI95[-.313, -.109]. When controlling for religiosity, political conservativism, and conscientiousness, and the other parameter, broad childhood unpredictability still negatively predicted the U parameter, *b* = -.18, *SE* = .06, *t*(194) = -3.28, *p* = .001, CI95[-.290, -.072].

**Mediation Analyses.**

***Childhood harshness.*** There was no indirect effect of childhood harshness on the D parameter through religiosity, *b* = .01, *SE* = .01, CI95[-.008, .210]; political conservativism, *b* = -.01, *SE* = .03, CI95[-.076, .033]; Big 5 personality traits (openness, *b* = .00, *SE* = .01, CI95[-.018, .018]; conscientiousness,  *b* = -.03, *SE* = .02, CI95[-.098, .003]; extroversion, *b* = .01, *SE* = .02, CI95[-.022, .065]; agreeableness, *b* = -.02, *SE* = .03, CI95[-.091, .044]; neuroticism, *b* = -.02, *SE* = .03, CI95[-.090, .036]); and social desirability *b* = -.03, *SE* = .03, CI95[-.105, .035].

Likewise, there was no indirect effect of childhood harshness on the U parameter through religiosity, *b* = .04, *SE* = .05, CI95[-.049, .131]; political conservativism, *b* = -.01, *SE* = .03, CI95[-.082, .042]; Big 5 personality traits (openness, *b* = .00, *SE* = .01, CI95[-.019, .024]; conscientiousness,  *b* = -.03, *SE* = .03, CI95[-.106, .004]; extroversion, *b* = .01, *SE* = .01, CI95[-.013, .052]; agreeableness, *b* = -.00, *SE* = .01, CI95[-.038, .016]; neuroticism, *b* = .01, *SE* = .03, CI95[-.045, .094]); and social desirability *b* = .00, *SE* = .01, CI95[-.017, .031].

***Composite unpredictability.*** We also tested whether the candidate mediators (religiosity, political conservativism, Big 5 personality traits, and social desirability) accounted for significant indirect variance in the relationships between a composite unpredictability measure and the PD parameters. In a first step, we tested whether each mediator separately carried significant indirect variance between broad unpredictability and each PD parameter in separate 10,000 bootstrapping resample mediation analyses using Model 4 in the PROCESS Macro for SPSS (Preacher & Hayes, 2004). In a second step, we included significant mediators from step one in a simultaneous mediation model to account for their shared variance.

In the first step, only agreeableness accounted for significant indirect variance between childhood unpredictability and the D parameter (when entering the other Big 5 personality traits simultaneously), *b* = -.03, *SE* = .01, CI95[-.067, -.010]. People who experienced more unpredictable environments as children were less agreeable, and this contributed to less concern about rejecting harm.

In the first step, only religiosity accounted for significant indirect variance between childhood unpredictability and the U parameter, *b* = -.06, *SE* = .02, CI95[-.093, -.033], That is, people who experienced more unpredictable environments as children were more religious, and this contributed to less outcome maximization on moral dilemmas.

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1. We took into account that reverse-coded items often separate out from other items even when conceptually related or statistically correlated, or create “artifactors,” as a result of biased responding by participants to differently valanced items, as evidenced by prior work (e.g., Heaven, 1983; Marsh, 1986, 1996; Ray, 1979, 1983; Schmitt & Stults, 1985; Tomás & Oliver, 1999; Weijters, Baumgartner, & Schillewaert, 2013; Woods, 2006). [↑](#footnote-ref-1)