Essays on the Endogeneity of Preferences and Personality Traits

Inaugural-Dissertation
zur Erlangung des akademischen Grades eines
Doktors der Wirtschaftswissenschaft

am Fachbereich Wirtschaftswissenschaft der
Freien Universität Berlin

vorgelegt von
Malte Preuß, M.Sc.
aus Gifhorn

Wiesbaden 2019

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Tag der Disputation	28. Juni 2019

Erklärung zu Koautoren

Diese Dissertation umfasst neben einer Einleitung (Kapitel 1) drei Forschungspapiere (Kapitel 2, 3 und 4). Die Kapitel 1 und 2 sind allein verfasst worden. Kapitel 3 ist in Ko-Autorenschaft mit Dr. Clemens Hetschko entstanden. Koautorin von Kapitel 4 ist Juliane Hennecke (M.Sc.). Für die Dissertation sind Kapitel 3 und 4 gegenüber den gemeinsam verfassten Manuskripten leicht, redaktionell angepasst worden. Diese Veränderung verantwortet allein der Autor der vorliegenden Dissertation. Eine Liste mit Vorveröffentlichungen der Kapitel befindet sich auf Seite 140.

Acknowledgements

I would like to express special thanks to Prof. Dr. Ronnie Schöb for supervising this thesis. His encouragement, ideas and advice have been tremendously important to me while writing this dissertation. I would also like to thank Prof. Dr. Silke Anger for acting as a second supervisor for my thesis. Chapter 2 especially has benefited substantially from her suggestions and ideas.

During my PhD, I have collaborated with several co-authors, although some of these outcomes are not a part of this thesis. I have benefited a lot from the joint work with the University of Potsdam and the DIW Berlin. In particular, I would like to thank Alexandra Fedorets, Cosima Obst and Linda Wittbrodt for working closely with me. I would also like to thank Juliane Hennecke who is the co-author of one of the following studies. A very deep thanks goes to Dr. Clemens Hetschko, co-author of Chapter 3. He has been helpful and supportive throughout my time at Freie Universität Berlin. All papers in this thesis benefited substantially from his advice, and I always enjoyed his company. Furthermore, I would like to thank my colleagues at the Freie Universität Berlin, in particular, Cordula Arlinghaus, Prof. Dr. Katja Görlitz, Tom Günther, Prof. Dr. Katharina Jenderny, Dr. Daniel Nachtigall, Prof. Dr. Carsten Schröder, and Tobias Wolf. Prof. Dr. Liam Delaney welcomed me for a short research stay at the UCD. Thank you to him and his chair for very fruitful discussions. I would also like to thank the German Council of Economic Experts, its staff, and its liaison office who welcomed me during the final period of this thesis.

Outside the office, I would like to thank my friends from Berlin and Gifhorn. In addition, I thank Heiko's running group for sharing countless hours in the woods, the streets, and the 'Tunnel'. A special thanks goes to my family: my parents Katrin and Reinhard as well as my siblings Tillmann, Rike and Thorben.

At last, I am indebted to my fiancée Jana who accompanied me throughout my PhD. Her company and support have been – and will be – outstandingly important for me. Thank you.

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Chapter 1

Introduction

1.1 Economic preferences, personality traits and decision making

Understanding how individuals manage limited resources and make economically relevant decisions under budget or time constraints has been a main objective of economic research. Herein, economists' essential tools still trace back to the 19th century where the key paradigm of the *homo oeconomicus* was introduced, in other words, the assumption that individuals maximize utility in accordance with their economic preferences. Since then, individuals' tastes have been in the spotlight of interest.

Introduced by Marshall (1920) to illustrate individuals' demand for goods, preferences have been formalized with respect to numerous aspects of everyday decision making. Valuation of leisure (Robbins, 1930), future rewards (Samuelson, 1937), or security (Arrow, 1971; Pratt, 1964) are nowadays fundamentals of any economic textbook in explaining individuals' actions. This success has its origin in two reasons. First, the elegant and intuitive formalization of preferences allows a straightforward deduction of testable hypotheses. Second, these hypotheses turned out to be true in many different cases. People differ with respect to their tastes and act in accordance with them. Valuing leisure decreases labor supply (Killingsworth and Heckman, 1986; Pencavel, 1986), preferring future outcomes comes along with healthier behavior, more savings, and higher education (Chabris *et al.*, 2008; Finke and Huston, 2013; Golsteyn *et al.*, 2014), and the willingness to take risks affects occupational sorting, migration, or investment decisions (Bonin *et al.*, 2007; Jaeger *et al.*, 2010; Dohmen *et al.*, 2011).

However, an increasing number of anomalies within the framework of preferences has lead to various extensions. On the one hand, additional functional arguments have been identified. Utility from social norms (Akerlof and Kranton, 2000) or preferences for fairness (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000) can explain, for instance, why individuals deviate in their actions from predictions of the baseline framework and value not only consumption opportunities. On the other hand, modifications of the functional form of preferences have been proposed. Kahneman and Tversky (1979), for example, formalize in their prospect theory that individuals evaluate uncertainty within gains differently than uncertainty within losses. Laibson (1997) proposes hyperbolic discounting, which implies different time preferences for different time horizons. Despite the criticism and the corresponding modifications, preferences kept their essential role within economic theory.

Preferences, however, were not able to explain heterogeneity in behavior to its full extent. Herein, personality traits have been identified as a missing link only recently (Almlund *et al.*, 2011a; Heckman, 2011). In contrast to economic preferences, personality traits are not restricted to one particular set of decisions. Defined as 'enduring patterns of thoughts, feelings, and behaviors' (Roberts, 2009, p. 140), they can affect decision making through multiple channels. They may shape preferences (Becker *et al.*, 2012) or define the set of constraints in which individuals make their economic decisions (Borghans *et al.*, 2008). Being optimistic or open for new experiences, for example, widens the set of potential choices, while a lack of motivation obstructs the decision maker to recognize possible choices, although she would prefer the corresponding outcome. Most prominent is the Big Five taxonomy (McCrae and Costa Jr, 2008) which aims to describe the personality of individuals by five factors only, openness, conscientiousness, extraversion, agreeableness and neuroticism. In addition, other, more specific traits have been introduced. Within labor economics, for instance, locus of control has been identified as a tremendous explanatory factor in understanding motivation and expectation (see Cobb-Clark, 2015).

Parallel to the increasing evidence on *how* decision making is affected by individuals' inherent characteristics, interest has risen on the question of *where* these characteristics originate from. Traditionally, economists take preferences as given (e.g. Friedman, 1962). Motivated to understand the heterogeneity in human behavior better and to improve models on decision making, more and more effort has been put on the identification of the determinants of preferences and personality traits. Here, genetic disposition and childhood environment have been identified to play a key role (e.g. Cunha and Heckman, 2007; Dohmen *et al.*, 2012; Anger and Schnitzlein, 2016). Although the importance of genes and childhood for the formation of preferences and personality is broadly accepted, there is no consensus yet whether this process stops at a certain age or whether preferences and personality characteristics evolve throughout the life span (Golsteyn and Schildberg-Hörisch, 2017).

Typically, economic preferences and personality traits are considered exogenous variables and thereby assumed to be set like plaster after the formation process is finished, in other words, after adolescence (James, 1890; Costa Jr. and McCrae, 1994). If, however, the 'set like plaster' assumption does not hold, and preferences and personality traits are time-variant during working age, several important issues arise. Foremost, the causal interpretation of decision making and tastes would be at stake. Preferences would not only be the reason for socio-economic outcomes; they could also be their result. In return, unpreferable outcomes – from an individual or social planner's point of view – could manifest themselves due to a feedback effect between actions, environment, and preferences.

Here, this thesis makes its contribution. It presents three studies on the endogeneity of economic preferences and personality traits: time preferences, risk attitudes, and locus of control. It thereby contributes to the ongoing discussion on whether individuals' tastes and personality changes during adulthood and whether the endogeneity concerns turn out to be true. Although three different measures are analyzed, all studies point out that all three variables are relatively stable across time. Even though individuals change their attitudes after specific events, they soon fall back to their old habits.

The remainder of this introduction is structured as follows. Section 1.2 will give an overview of the consequences of instability within preferences and personality traits from a theoretical as well as an empirical point of view. Section 1.3 introduces the previous literature. Section 1.4 summarizes the contribution of the present thesis in more detail and makes some concluding remarks.

1.2 Consequences of instability

1.2.1 Endogenous preferences in economic theory

To illustrate the consequences of unstable preferences, a short introduction to the baseline framework is helpful. Often, a von-Neumann-Morgenstern utility function $u(\bullet)$ represents preferences. u is concave ($u' >$ and $u'' < 0$), implying positive but decreasing gains from its arguments, which are chosen in correspondence with the focus of interest; for instance, consumption c in the realm of risk preferences.

Under the premise of '*de gustibus non est disputandum*' (Stigler and Becker, 1977), a specific utility function allows us to compare individuals with each other, for instance, with respect to their willingness to take risks. Following the definition of Arrow (1971) and Pratt (1964), individual i is more risk averse than individual j if her absolute risk aversion (ARA) is higher:

$$ARA_i(c) = -\frac{u_i''(c)}{u_i'(c)} > -\frac{u_j''(c)}{u_j'(c)} = ARA_j(c) \quad \forall c.$$

In an uncertain world where consumption is not fixed to one level but uncertain, both individuals would experience a loss in utility even if they can expect from both scenarios an equal level of consumption. This loss in utility due to uncertainty is, however, smaller for individual j. She is less risk averse. Accordingly, her willingness to pay for insurance against the uncertainty will be smaller from a theoretical point of view (Mossin, 1968). Although there is substantial criticism concerning this expected utility framework (see Rabin and Thaler, 2001), the heterogeneity between individuals with respect to risk aversion has been shown to manifest itself in different kinds of observable behavior. Risk-prone individuals act less healthily, invest more riskily (Dohmen *et al.*, 2011), choose a job with higher wage variance (Bonin *et al.*, 2007), are more likely to migrate (Jaeger *et al.*, 2010), and are more often self-employed (Caliendo *et al.*, 2014).

Within this framework, the stability assumption concerns utility $u(c)$.[1] While the level of consumption may change – for example due to variations in income – $u(\bullet)$ is assumed

[1] A common applied utility function is the so-called 'constant relative risk aversion' (CRRA), i.e. $u(c) = (c^{1-\sigma} - 1)/(1-\sigma)$ with $\sigma \in [0, 1)$ as level of risk aversion. Here, a change in preferences would imply that σ changes.

to be exogenous and thus fixed. This restriction is as convenient as it is sufficient in a static, one-period setting. However, problems arise when the model is extended to more than one period: defining utility in period $t-1$ but the decision of interest in t could be inappropriate. Theoretical expectations would be rejected in empirical tests if preferences change within the considered time frame. Under the assumption of exogenous and fixed preferences, the empirical rejections could lead to the conjecture that preferences are not transitory, such as, if $c_1 > c_2$, then $u(c_1) > u(c_2)$ – a necessary assumption within the neoclassical framework. In light of these issues, Palacios-Huerta and Santos (2004) propose a model which formalizes individuals' risk aversion as a function of the exposure to market volatility. The better markets function, the more individuals are willing to risk.

Another example of inconsistency within preferences is related to time preferences. Typically, these are defined within a lifetime utility model. Here, periodical utility $u(\bullet)$ is maximized over a given number of periods T, such that

$$U\left(c_t, c_{t+1}, ..., c_T\right) = u(c_t) + \sum_{d=1}^{T-t} F(d)u(c_{t+d}).$$

Utility in each period $u(c_{t+d})$ is weighted by time preference $F(d)$ and aggregated to lifetime utility U. Following Samuelson (1937), time preferences are defined as $F(d) = \delta^d$, where discount factor δ defines individuals' perceived value of future rewards. Accordingly, if $\delta_i > \delta_j$, individual i is more patient and values future outcomes more than j. Empirically, this relation affects various dimensions in everyday decisions. More patient individuals invest, for instance, more into education and sacrifice current income today in return for a higher income in the future (Becker, 1964). Inpatient individuals are not willing to accept this trade and spend less time in education. In consequence, δ comes along with increasing lifetime earnings (Golsteyn *et al.*, 2014). More patient individuals are also more concerned with the future and tend to save more for their retirement (Finke and Huston, 2013). On an aggregated level, Dohmen *et al.* (2018) and Falk *et al.* (2018) show that the preference for future rewards is a key determinant of comparative development.

These findings must be reconsidered if time preferences are not fixed but a result of decision making. Becker and Mulligan (1997), for example, model discount rates in dependence of individuals' efforts. Here, the valuation of future outcomes is a costly, iterative task where opportunity costs shrink if lifetime wealth or expected life span increase. Time discounting may thus change throughout the life span. Following this proposition, education, for instance, may shape individuals' patience as it increases lifetime wealth. The same may apply within economic development: prosperity increases individuals' wealth and thereby their discount factors.

One additional application of unstable time preferences concerns dynamic inconsistency. The observation that individuals act more forward-looking when it comes to more distant payoffs has lead to the introduction of hyperbolic-discounting (see Laibson, 1997). It proposes that a future-looking attitude is not only shaped by the discount factor δ but also depends on a present bias.[2] Due to this additional parameter, behavior or decisions change as time goes by. Many smokers, for instance, agree to stop their bad habit *tomorrow* in order to improve their future health. Typically, this decision is reconsidered the next day. Here, the immediate benefit of smoking outweighs the benefit of future health, again. Inconsistencies like this could also become possible when preferences are not stable, and individuals truly change their discount-factors. Changing time preferences could, for example, be an alternative explanation of why individuals drop out of college even right before their graduation (see Cadena and Keys, 2015).

Instability within personality traits implies similar endogeneity concerns. In their case, the impact of instability, however, is less straightforward since personality traits shape, on the one hand, preferences themselves and, on the other hand, define the set of constraints in which individuals make their decisions. Changing non-cognitive skills may thus simply be a reason why risk, time or any other economic preference may vary over time. It is, however, also relevant when constraints change. Caliendo *et al.* (2015), for instance, argue that locus of control improves job search strategies by positively affecting expectations on the job arrival rate. Individuals believing that they can influence the outcomes of their life thus scoring high on the locus of control scale have a higher reservation wage and search more intensively for new employment. If locus of control were affected by labor market experiences, the modeled relation would be endogenous: the success in the labor market would shape expectations and vice versa.

1.2.2 Endogeneity and error-in-variables bias

Concerns regarding the instability of preferences are not a theoretical issue only. It affects any empirical investigation that aims to identify the causal effect of preferences on economic outcomes. To illustrate these issues, it is helpful to follow Verbeek (2017) and to look at potential identification issues due to instability. For this purpose, an OLS model is considered which aims to test the effect of a preference or personality factor F on the outcome Y:

$$Y_{it} = \beta_0 + \beta_1 F_{it} + \varepsilon_{it} \tag{1.1}$$

[2] Following Laibson (1997), (quasi-)hyperbolic discounting can be described by $F(d) = \varphi\delta^d$ for all $d > 1$ with $\varphi \in (0,1)$ as the present bias and $\delta \in (0,1)$ as the discount rate.

with i as the individual and t as a time indicator. Herein, β_1 identifies the marginal effect of interest. By assumption, the residuum ε is i.i.d with mean zero and variance σ^2. β_0 denotes the average level of the dependent variable.

Endogeneity bias Issues within the identification of β_1 arise when F is endogenous, for instance, when it is determined by Y and some other, exogenous variable X. Say F can be described by the function,

$$F_{it} = \gamma_1 Y_{it} + \gamma_2 X_{it}. \tag{1.2}$$

To achieve unbiased estimates of β_1, the assumption that F is independent of ε must hold. However, since Equation (1.2) implies that Y affects F, this assumption is violated, which leads to an endogeneity bias:

$$plim(\hat{\beta}_1) = \beta_1 + \frac{Cov(F_{it}, \varepsilon_{it})}{Var(F_{it})}. \tag{1.3}$$

A priori, the direction and extent of the bias $\hat{\beta}_1 - \beta_1$ is not clear since the sign and size of $Cov(F_{it}, \varepsilon_{it})$ are not predetermined.

Consider the effect of time preferences on educational choices as an example. Following Becker (1964), more patient individuals (F) invest more into education (Y). It may, however, be the case that schooling also affects time preferences: increasing cognitive abilities (X) may shape forward-looking behavior while being in school (Dohmen *et al.*, 2010). Estimates on the marginal effects of time preferences on schooling could thus be biased in accordance with Equation (1.3). Similar concerns are discussed by Cobb-Clark and Schurer (2013) on the effects of personality traits on wages. If personality (F) is affected by past labor market outcomes (X, Boyce *et al.*, 2015; Anger *et al.*, 2017), estimates on productivity or wages (Y) could also be biased.

Error-in-variables bias Estimates will not only be affected if the preference of interest changes endogenously. Estimation of β_1 could already be biased if F changes for an unrelated reason. In many empirical studies, preferences or personality traits are not measured simultaneously with the economic decision of interest. Although Equation (1.1) proposes to measure F at the same point in time as Y (e.g. t), surveys or experiments on F may have happened before the decision of interest took place. Accordingly, F_{it} is often approximated by F_{it-1}. If, however, F changes between $t-1$ and t, the so-called error-in-variables bias occurs. Following Verbeek (2017) and defining $u_{it} = F_{it} - F_{it-1}$, estimations of β_1 in Model (1.1) will yield,

$$plim(\hat{\beta}_1) = \beta_1 \left(1 - \frac{Var(u_{it})}{Var(F_{it}) + Var(u_{it})}\right). \tag{1.4}$$

Only if there is no change in F (e.g. $u_{it} = 0$) will $Var(u_{it})$ equal zero and assure the unbiased identification of β_1. If, however, F varies across time, $\hat{\beta}_1$ will suffer from an attenuation bias.

The error-in-variables bias has two important insights. First, the direction of the bias is predetermined: $\hat{\beta}_1$ is biased towards zero in any case. Accordingly, if the reason for the instability is not associated with changes in F, estimates will identify the lower bound. The importance of preferences or personality traits may thus have been underestimated in many cases. The effect of personality on wages, for example, may thus be higher. Second, instability does not imply the rejection of exogenous preferences per se; one must distinguish between the causes for instability.

1.3 Previous findings on instability

In light of these technical implications, empirical studies relying on traits or preferences as explanatory variables have been especially subjected to scrutiny (Borghans *et al.*, 2008). This motivated many researchers to investigate these sources of error more intensively. The following section will summarize their results. Herein, this section is limited to findings on stability during adulthood since the stability assumption concerns individuals who are in their working age only. That preferences and personality traits are intensively shaped during childhood and adolescence has been broadly acknowledged.[3]

Intra-individual consistency The psychological literature started to discuss the stability of inherent characteristics very early and potentially motivated economists to encounter these issues, too. The stability discussion has accompanied psychologists since the 19th century, where the image that personality is 'set like plaster' was introduced (James, 1890). The introduction of the Big 5 taxonomy as a universal description of personality verified this view. Costa Jr. and McCrae (1994, p. 33) note, however, that '*plaster is not granite*', in other words, personality does not need to be perfectly consistent in order to consider it as stable. An intra-individual correlation of 0.50 is sufficient to verify personality traits as

[3] See the technology of skill formation for an elegant formalization (see Cunha and Heckman, 2007, 2008; Cunha *et al.*, 2010) and the HighScope Perry Preschool Program as a prominent example for an empirical investigation (see Heckman *et al.*, 2010).

persistent.[4] Empirically, this holds: in a meta-analysis, Roberts and DelVecchio (2000) find intra-individual correlations of various personality measurements between 0.60 to 0.75 in the long-run.

The observation that personality varies within individuals to some degree led to the proposition of separating personality traits into two components, the time-invariant 'trait' and the situational, time-variant 'state'. While 'trait' is a highly enduring or even unchanging entity, 'state' is a temporary deviation from it caused by the interaction between the survey participant and her current surroundings (e.g. Hamaker *et al.*, 2007). One might even withdraw from the perception that personality is a single parameter but is best represented by a probability function (Fleeson, 2001).

Recent findings on preferences' stability are very similar (Golsteyn and Schildberg-Hörisch, 2017). Within different time horizons – ranging from days to years – various preferences show a sufficient degree of intra-individual correlations; time (Krupka and Stephens, 2013; Meier and Sprenger, 2015), risk (Dürsch *et al.*, 2017; Schildberg-Hörisch, 2018), or social preferences (Carlsson *et al.*, 2014; Chuang and Schechter, 2015) are some examples.

The lack of perfect stability does, however, not allow us to reject endogeneity concerns per se. Even small variations within preferences or personality may cause estimation biases. Although traits or preferences are stable within the majority of individuals, it does not rule out that they are affected by specific events. It must thus be tested to which degree plaster crumbles naturally and whether there are events that can crack it.

Crumbling traits in the short run In line with the theoretical framework of the trait-state model from above, Borghans *et al.* (2008) hypothesize that the measurement of personality or preferences is dependent on situational circumstances. In varying situations, the measurement of traits changes. The behavioral nature of these small changes has, however, not been characterized yet. In principle, two options are possible. If individuals always behave in correspondence with their revealed characteristics, even small, situational changes will cause adaptations in behavior. Depending on the amplitude of the variation (intensity and duration), endogeneity concerns can thus sill arise. Contrarily, small changes could be a measurement issue only. Social desirability, for instance, could motivate survey participants to misreport their answers (un-)intentionally (Paulhus, 1984). The observed volatility in preferences would have no behavioral impact. Individuals would

[4] Psychological literature differentiates between different kinds of stability (see Roberts and Mroczek, 2008). Two definitions are most common. Rank-order consistency, on the one hand, describes whether the ranking within a population changes across time. Mean-level consistency, on the other hand, tests whether changes occur within an individual. Since mean-level changes can verify endogeneity concerns only, this thesis refers in all cases to the latter definition, if not stated otherwise.

always behave in correspondence with their underlying, time-invariant preference, which is, unfortunately, unobservable.

Borghans *et al.* (2008) as well as Cunha and Heckman (2008) emphasize the second option and state that paper-and-pencil questionnaires on personality traits should always be considered as imperfect proxies only. The same applies to laboratory methods for economic preferences (Frederick *et al.*, 2002). Error-in-variable biases will thus not only occur if personality or preference is measured before the decision of interest takes place (as illustrated in Section 1.2), but estimations are prone to measurement errors at any time.

Reasons behind situational changes can be minor like emotions (Kusev *et al.*, 2017; Meier, 2018) or media coverage of economic news (Tausch and Zumbuehl, 2018). Similarly, the salience of a potential threat shapes economic attitudes. The nuclear catastrophe of Fukushima, for example, rendered Germans more risk averse although the objective risk of nuclear power plants in Germany was not affected by the disaster (Goebel *et al.*, 2015). From one day to the other, preferences or personality may thus change.

Crumbling traits in the long run In addition to the situational aspects, crumbling traits also imply that preferences or personality follow a long-term trend caused by biological maturation (McCrae and Costa Jr, 2008). During the working-age (and beyond), individuals' traits can change slowly but steadily. However, the shape of these long-term trends differs substantially between measures and sometimes even between studies.

While risk aversion has been found to increase with age (Schurer, 2015; Dohmen *et al.*, 2017), results on time preferences have not reached a consensus. Some find the lowest preference for future rewards within middle-aged individuals (Harrison *et al.*, 2002; Read and Read, 2004; Richter and Mata, 2018), some find a negative trend across the life-course (Green *et al.*, 1999; Löckenhoff *et al.*, 2011), and others identify no trend at all (Green *et al.*, 1994; Chao *et al.*, 2009). Findings on long-term trends in personality are inconclusive, too (e.g. McCrae and Costa Jr., 1994; Roberts and Mroczek, 2008; Specht *et al.*, 2011), but sample sizes are often very small, results depend on cross-sectional data, and different methods to elicit personality or preferences are used. A comparison is thus hard to achieve.

Reasons for long-term trends are manifold. Bonsang and Dohmen (2015) point out that cognitive-aging is the major reason why risk aversion increases with age. Also, political institutions have been shown to shape individuals redistribution and social preferences (Alesina and Fuchs-Schündeln, 2007; Brosig-Koch *et al.*, 2011). The same is likely to apply for other economic attitudes as well (Fehr and Hoff, 2011; Falk *et al.*, 2018).

From a theoretical and empirical point of view, these long-term changes are often not crucial for economics as their size is relatively small. Moreover, since age is available to the researcher in most cases, its inclusion in the estimation model is straightforward. Endogeneity concerns due to age are thus easily accountable.

Cracking plaster In addition to short-term volatility and long-run changes, preferences or personality may be affected more substantially by dramatic positive or negative life events. An exogenous shock may crack the plaster and cause the trait of interest to change persistently.

Within risk and time attitudes, several of these events have been identified. Becoming a parent (Görlitz and Tamm, 2015), losing a child (Bucciol and Zarri, 2015), experiencing a natural catastrophe (Cameron and Shah, 2015), being exposed to poverty (Haushofer and Fehr, 2014), being a victim of violence (Voors *et al.*, 2012; Callen *et al.*, 2014; Kim and Lee, 2014), experiencing financial crises (Malmendier and Nagel, 2011; Guiso *et al.*, 2018), or just being in a recession (Cohn *et al.*, 2015; Bucciol and Miniaci, 2015, 2018; Dohmen *et al.*, 2016) affect individuals' risk aversion. Time preferences are shaped by the general and individual economic situation (Krupka and Stephens, 2013; Dean and Sautmann, 2014), violence (Voors *et al.*, 2012), consumption constraints (Carvalho *et al.*, 2016), and natural disasters (Callen, 2015). Personality traits follow a similar pattern and have been found to change for various reasons, such as labor market or family related events (Kandler *et al.*, 2011; Specht *et al.*, 2011). All these results imply endogeneity: if preferences or personality change with past experiences, issues as described in Section 1.2 will apply.

However, objections have been raised. Cobb-Clark and Schurer (2012, 2013) analyze the aggregated effect of several negative and positive events on individuals' personality and conclude that the overall effect is not substantial enough to cause meaningful interferences in econometric estimations. In addition, many of the effects appear to be temporary only. After a while, individuals fall back to their initial level of preferences (Schildberg-Hörisch, 2018). Evidence on cracking plaster is thus ambiguous. Plaster cracks, but these cracks are often small and not irreversible.

1.4 Contribution of the present thesis

1.4.1 Common features

The present thesis makes its contribution within the stability discussion with respect to three different traits, time preferences, risk attitude, and locus of control. It analyzes to which extent events affect the trait of interest and whether the change is persistent or not. Herein, this thesis does not rely on the traditional approach to measure preferences. Typically, preferences are elicited by theoretically motivated laboratory tests. Holt and Laury (2002), for example, propose to elicit risk preferences by asking individuals to choose between a save-smaller and an uncertain-larger payment. To elicit time preferences, individuals choose between sooner-smaller or later-larger rewards (Frederick *et al.*, 2002). Social preferences are measured in 'dictator games' in which one individual is allowed to share her endowment with others (Charness and Rabin, 2002). In all cases, the critical identification assumption is that individuals reveal their preferences by their actions in the lab.

Alternatively to this 'revealed preference approach', the following thesis relies on the 'stated preferences approach' which asks individuals directly about their preferences in a paper-and-pencil questionnaire. In contrast to laboratory tests, these questions do not need to rely on monetary incentives to assure that individuals reveal their economic attitudes for time or risk. Moreover, financial literacy is not needed to understand the question correctly. Answers to these surveys preserve, nevertheless, a high degree of behavioral validity: answers match everyday decision making as well as standard laboratory measures (Dohmen *et al.*, 2011; Burks *et al.*, 2012; Falk *et al.*, 2016).

Assuring behavioral validity while being easily implementable in representative and longitudinal surveys has led to widespread implementation of the stated preferences approach in various panel data sets, allowing the testing of several hypotheses that would have been hardly testable with the standard laboratory approach. Two of these surveys are the foundation of the present thesis. The German Socio-Economic Panel (SOEP, Goebel *et al.*, 2019) and the Dutch Household Survey (DHS, de Bruijne *et al.*, 2014) include various measures on preferences and personality traits in their annual questionnaire thereby allowing researchers to analyze not only the effect of certain traits and preferences on decision making but also provide the opportunity to test the intra-individual stability of several measures with respect to time and certain events.

While all chapters share their data basis and the quest for stability, they differ with respect to their scope. Chapter 2 takes a general perspective on the stability of time preferences, analyzing several different events and their impact on this key economic attitude since the literature in this field is scarce in general. Contrarily, evidence on risk attitude and locus of control is more advanced. Chapter 3 and Chapter 4, therefore, look at the effect of one specific event only, unemployment. Losing employment is of specific interest as it affects individuals in many different dimensions. It reduces individuals' well-being long after the event took place (Lucas *et al.*, 2004; Clark *et al.*, 2008), diminishes future wages persistently (Arulampalam *et al.*, 2001), and causes behavioral changes related to health (Browning and Heinesen, 2012), fertility (Huttunen and Kellokumpu, 2016), or social life (Kunze and Suppa, 2017). Foremost, it is subjected to affect the Big Five personality traits (Anger *et al.*, 2017). Unemployment is thus as detrimental to individuals as it is common in labor markets.

Testing the impact of unemployment on individuals' underlying traits is not only relevant from a technical point of view. Effects of unemployment on individuals and their preferences or personality traits could explain why unemployment has such detrimental and persistent effects on the affected, opening up new opportunities for policy intervention (Heckman, 2011; Almlund *et al.*, 2011b). If unemployment does not only affect cognitive abilities but also preferences or personality traits, active labor market policy could also consider psychological measures in their tool kit. Therefore, this thesis forges a link between behavioral and labor economics.

1.4.2 Chapter 2: Intra-individual stability of time preferences: a survey approach for the long run

Although substantial effort has been spent to evaluate the stability of personality traits and risk preferences, the current literature has not yet established a full picture of the stability of time preferences. The majority of studies in this field of research is either focused on very short time frames, relies on very small samples sizes, or is restricted by both constraints (see Chuang and Schechter, 2015). Herein, Krupka and Stephens (2013) and Meier and Sprenger (2015) make a substantial contribution by testing the stability of time preferences within two large samples. While the latter study finds a sufficient degree of intra-individual stability, Krupka and Stephens (2013) conclude that the economic situation of individuals shapes current preferences. Nevertheless, both analyses have their own limitations. Foremost, their data is not representative, so general conclusions cannot be made. Moreover, they cannot make deductions on the timing of the effects. Questions concerning the anticipation or persistence of changes in time preferences are, therefore, out of bound. Lastly, other, non-income related events have not been analyzed at all.

Chapter 2 can make various contributions to all of these aforementioned limitations by relying on the stated preferences approach. Using the DHS allows Chapter 2 to test the stability of the 'consideration of future consequences'-scale (Strathman *et al.*, 1994), a questionnaire of twelve items to elicit individuals' forward-looking behavior and self-control abilities (Joireman and King, 2016). The structure of the data set allows for a timing of events analysis in the form of a fixed effect model (see Clark *et al.*, 2008). Anticipation, as well as persistence, can thus be analyzed within a representative sample. Thereby, Chapter 2 is the first study to give a full picture of time preferences' stability in the long run. As a robustness test, an alternative stated preference approach on time preferences, which was included in the SOEP (Vischer *et al.*, 2013), is applied to verify the results from the DHS.

In summary, time preferences are very similar to personality traits or risk attitudes. They imply, on the one hand, a sufficient degree of stability in order to consider them as 'stable traits' in the sense of Costa Jr. and McCrae (1994). On the other hand, forward-looking attitude is – like any other trait – far from being perfectly stable. The instability is, however, only partially caused by specific events. Although unemployment, income shocks, or health appear to affect forward-looking attitudes, the effects are temporarily restricted and relatively small; not substantial enough to explain the instability in time preferences in general. Age implies a weak effect, at most.

Chapter 2, therefore, comes to a similar conclusion as Meier and Sprenger (2015). Since time preferences vary so substantially without an obvious reason, they appear to include a substantial situational component as discussed in Section 1.3. Changes seem to be random to a large extent and only partly caused by specific events. Empirical studies relying on the stated preferences approach are, therefore, prone to error-in-variables biases.

1.4.3 Chapter 3: Income in jeopardy: how losing employment affects the willingness to take risks

Chapter 3 focuses on one specific event and its impact on individuals' willingness to take risks, unemployment, allowing several contributions to be made. The previous literature focuses either on very general (e.g. the business cycle, media coverage of economic news) or very uncommon (e.g. natural disasters, losing a child) events. Unemployment is, on the contrary, an event that is as detrimental as it is common. Furthermore, relying on plant closures as a reason for unemployment allows the approximation of the causal effect of unemployment on individuals as close as possible. While other events like fertility decisions or poverty are subjected to endogeneity concerns, being laid off due to the closure of a firm is an event that is independent of individuals' history, ability, or preferences. It confronts every affected person with the threat of being involuntarily unemployed and the task of finding a new job. Unemployment can thereby act as a natural experiment to evaluate the impact of income shocks and uncertainty on risk attitudes.

Due to its large sample size, its detailed information on individuals' occupation, and its repeated information on participants' willingness to take risks (see Dohmen *et al.*, 2011), the SOEP is ideally suited for the purpose of Chapter 3. It allows testing of the impact of a job loss with a difference-in-difference approach, which compares those who experience a plant closure and those who do not. Therefore, not only can time-invariant characteristics of the dismissed can be controlled for but also general time trends can be identified.

The analysis reveals that a job loss renders individuals more risk averse. The effect is observable before the job loss takes place and occurs independently from the time spent in unemployment. Even those individuals who are immediately back to employment change their risk aversion. After some time, individuals turn back to their initial level of risk attitude.

Chapter 3 tests several channels to explain these findings. Here, neither the immediate income loss nor parallel life events nor negative emotions correspond with the results. Yet, those individuals with the highest income at stake turn out to react the strongest. Therefore, Chapter 3 concludes that uncertainty about the future mediates changes in people's risk aversion. Accordingly, as soon as the uncertainty vanishes again, individuals turn back to their usual level of risk attitude. This could also explain why risk aversion changes with the business cycle, as uncertainty about employment increases during recessions.

The study, however, does not conclude that risk preferences change. Instead, it proposes to interpret the applied stated preference approach as a local measure of risk preferences, in other words, as a level of absolute risk aversion. While standard neoclassical theory assumes preferences to be fixed, it likewise predicts that local risk aversion changes. If consumption opportunities decrease (Pratt, 1964; Arrow, 1971) or immutable, non-insurable events change their probability (Eeckhoudt *et al.*, 1996), people behave less risk loving without necessarily changing their preferences. This thesis can thus not only present evidence on the causal effect of unemployment on risk attitude and its underlying channels, but it also proposes a theoretical interpretation of the question on individuals' general willingness to take risks, a survey item which is increasingly used in the literature.

Although it cannot be claimed that the general willingness to take risks does reflect risk preferences only, results presented in Chapter 3 have technical implications nevertheless. The question on risk aversion implies endogeneity biases if the corresponding regression analysis does not sufficiently control for the level of uncertainty or the measurement of risk aversion does not take place at the same point in time as the decision of interest.

1.4.4 Chapter 4: Biased by success and failure: how unemployment shapes locus of control

Within labor economics, locus of control has been identified as an important factor for labor market success (see Cobb-Clark, 2015). Aiming to identify individuals' beliefs regarding the causal relationship between one's own efforts and life's outcomes, locus of control is able to mirror motivation as well as expectations of labor market participants (Rotter, 1966). Therefore, locus of control clearly separates itself from personality traits such as the Big Five.

Similar to economic preferences, locus of control must be time-invariant in order to consider its effect on behavior as causal. Otherwise, endogeneity biases will arise. Reassuringly, Cobb-Clark and Schurer (2013) do not find substantial effects of past life events on locus of control. They conclude that changes are not substantial enough to cause economically meaningful estimation errors. The events tested here have already included unemployment, but neither causality nor heterogeneity was discussed. Findings from Chapter 3 and evidence on the Big Five (see Anger *et al.*, 2017), however, point out that these dimensions play a key role in the identification of unemployment's effects. This motivates the examination of unemployment and its impact on locus of control more closely in Chapter 4. Consequently, Chapter 4 focuses on plant closures as a trigger for a job loss.

Using the SOEP and entropy balancing as a matching procedure to account for selection on observables (see Hainmueller, 2012), Chapter 4 identifies, in general – and in line with Cobb-Clark and Schurer (2013) – no effect of a job loss on locus of control. Yet, this zero-effect does not hold for everyone: those dismissed individuals who are still unemployed during the locus of control interview report a highly significant reduction in their control perception: they believe more strongly in the power of fate, bad luck, and the power of others. Contrarily, those individuals who are already employed again do not respond to the event at all. Even those people who experienced a job loss or unemployment for the first time do not change their answers in the ten-item questionnaire.

Those dismissed who have not been successful in their job search react homogeneously – neither the previous unemployment experience nor socio-economic characteristics nor duration of the unemployment spell moderates the identified effect. Looking at the unemployed one interview later – where most of them are finally in a new job – does indicate that the effect is not persistent: all individuals fall back to their pre-event level of locus of control. The effect of a job loss is thus restricted to unemployment.

The results presented in Chapter 4 are, therefore, in line with the previously summarized proposals on trait measurement (see Section 1.3). In specific situations, individuals change their answers, and one of these situations is unemployment. Accordingly, as soon as the situation does not apply anymore, individuals turn back to their baseline level of locus of control. From this point of view, past life events have no lasting impact on personality. Aside from its implications on labor economics, Chapter 4 makes an important contribution to the psychological literature by providing evidence on the situational component within the measurement of personality traits.

The unemployment specific change in locus of control raises; however, the question of whether individuals behave differently while being unemployed or whether this change is a survey issue only. In Chapter 3, the analysis reveals that changes in risk aversion correspond with actual behavior. The stronger the increase in risk aversion, the faster individuals are back in employment. The effects thus have a behavioral impact. Since effects on locus of control are state-dependent and homogenous, changes might be survey issues only. Social desirability could motivate individuals to report other beliefs than before. This must not be intentional. In order to cope with unemployment, individuals may shift the responsibility of their current labor force status to external factors. Then, locus of control does not explain coping ability only (Buddelmeyer and Powdthavee, 2016); it may function as a coping channel itself. Unfortunately, testing the behavioral consequences is not feasible with the SOEP. The technical solution induced by the findings of Chapter 4 is thus twofold. If the change in locus of control causes corresponding behavior, future studies must measure locus of control during the correct labor force status. If the change is spurious and survey noise only, locus of control – and maybe other personality traits – must be measured at one reference labor force status within the whole sample. Otherwise, endogeneity issues might arise.

1.4.5 Concluding remarks

The findings of the present thesis lend support to the idea that preferences and personality are stable during the working age, as the different empirical studies do not reject the respective stability assumption. Although the measures analyzed change with time, the following chapters propose alternative, less controversial interpretations of instability. All chapters come to the conclusion that preferences and personality traits have a time-invariant component individuals turn back to. Researchers, however, should be aware that measures on preferences and personality traits are imperfect proxies only. Yet, the resulting issues are – as the present study discusses – addressable.

Chapter 2

Intra-individual stability of time preferences: a survey approach for the long run

2.1 Introduction

Time preferences are a key determinant of decision-making. Introduced to explain individuals' valuation of future utility, forward-looking behavior has been shown to affect educational decisions, labor market success, health outcomes and, in the end, lifetime income (Golsteyn *et al.*, 2014). Astonishingly, all these findings still rely on the conceptual framework introduced by Samuelson (1937), which describes individuals' future orientation by exogenous parameters. However, even though this model has evolved to some extent (e.g. by allowing for hyperbolic discounting), a key assumption has not changed: time preference parameters are exogenous and stable over time.

A violation of this assumption would have fundamental implications. If time preferences change, not only is the level of time discounting crucial, the timeline must also be taken into account. Cost-benefit analysis, for instance, would be much more prone to misspecifications. In return, endogenous preferences may help to understand anomalies within the standard framework of time discounting. Dynamic inconsistency, for example, may not only be caused by hyperbolic discounting. It could likewise be a consequence of changing preference parameters. Moreover, empirical studies may suffer from an error-in-variable or endogeneity bias if they measure time preferences not at the same point in time when the decision of interest is made. Despite its importance for theory and empirics, evidence on the stability of time preferences is scarce and mostly limited to very short time frames and small sample sizes. This study closes this gap, analyzing time preferences' stability with respect to the long run and clarifying whether the exogeneity assumption is truly outdated.

The following study tests the stability of the '*consideration of future consequence*'-scale (Strathman *et al.*, 1994), a behaviorally validated set of questions to measure individuals' time preferences. Using the Dutch Household Survey, the analysis tests whether individuals' attitudes towards the future change in the long run and whether these changes can be traced back to common life events. In extension to the previous literature, the applied fixed effect model not only identifies the immediate effect of certain events on time preferences, but it also considers anticipatory changes and the effect's persistence.

In addition, the present study analyses an alternative survey approach with respect to its long-term stability, i.e. two ultra-short survey items on patience and impulsiveness (Vischer *et al.*, 2013). The German Socio Economic Panel allows cross-checking of all results and validating whether the identified (in-)stability is an issue restricted to one specific survey item or whether other measures of time preferences change in a similar fashion.

The results of both measures point out that individuals' time preferences change considerably. However, several common life events can hardly explain these changes. Although unemployment, income improvements, or health issues appear to change stated future orientation, effects are very small and, in all cases, temporarily restricted. Age trends are very flat at most while becoming a (grand-)parent or a retiree has no effect at all.

Contributing to the previous literature, the results point out that although life events affect time preferences, they do so for a limited time only. Individuals change their preferences during specific situations (e.g. Krupka and Stephens, 2013); yet, they leap back to a reference level soon after. In line with the stability assumption, time preferences appear to include a time-invariant component, which does not change over time. This study does thus not find evidence for endogenous time preference formation during working age. Although the assumption of fixed preferences was introduced for simplicity only (Samuelson, 1937, p. 156), it appears legitimate. Nevertheless, time preferences are very noisy: compared to other economic preferences or personality traits, preferences for future rewards vary within individuals substantially without an observable reason. This empirical investigation, therefore, comes to the conclusion that survey measures on time preferences are as prone to measurement issues as the standard laboratory approach (see Meier and Sprenger, 2015). Empirical studies testing the effect of time preferences could thus be affected by a substantial attenuation bias.

Section 2.2 gives an overview of the previous literature. Section 2.3 introduces the consideration of future consequences' scale and discusses its behavioral validity. The empirical strategy is outlined in Section 2.4, and results and robustness tests are presented in Section 2.5. Then, Section 2.6 replicates the analysis with two ultra-short survey items, and finally, Section 2.7 concludes.

2.2 Endogenous preferences, previous literature, and contributions

2.2.1 Endogenous preferences and their implications

Time preferences are essential for the analysis of individuals' intertemporal maximization problems for which present-value lifetime utility U in period 0 is typically defined as

$$U(c_0, c_1, ..., c_T) = u(c_0) + \sum_{d=1}^{T} F(d)u(c_d),$$

with u as the utility of consumption c in period t. Depending on the budget constraints and individuals' time preferences $F(d)$, consumption patterns are chosen and saving plans are made. Here, $F(d)$ is typically assumed to be quasi-hyperbolic in the sense of Laibson (1997) and thus shaped by two parameters, namely a discount factor δ and a present bias β. Accordingly, $F(d)$ equals $\beta\delta^d$ and individual i will act – independently from the context – more forward-looking (present oriented) than individual j if $\delta_i > \delta_j$ ($\beta_i < \beta_j$).

Based on this framework, the importance of time preferences has been identified within various dimensions. People are very heterogeneous with respect to their tastes and act accordingly. A forward-looking attitude can affect, for example, occupational sorting (Fouarge *et al.*, 2014), job searching (DellaVigna and Paserman, 2005; Halima and Halima, 2009), criminal activity (Åkerlund *et al.*, 2016), and health behavior (Borghans and Golsteyn, 2006; Chesson *et al.*, 2006; Chabris *et al.*, 2008).

The heterogeneity in preferences originates to a great extent from genetic disposition (Kosse and Pfeiffer, 2012; Brown and van der Pol, 2015; Cronqvist and Siegel, 2015; Brenøea and Eppera, 2018; Chowdhury *et al.*, 2018; Hübler, 2018); yet, preferences still evolve during childhood and adolescence (Delaney and Doyle, 2012; Deckers *et al.*, 2017). In the framework from above, however, this evolution stops, at a certain point in time, usually the beginning of the working age. Afterwards, both preference parameters β and δ are exogenous and fixed until T by assumption.

In contrast to this exogeneity assumption, Becker and Mulligan (1997) propose to model time discounting endogenously. In their model, individuals not only choose their consumption path, but they also decide on the optimal level of effort that they put into the imagination of future needs and utility. δ is not a fixed parameter but a concave function of effort S (e.g. $\delta(S)' > 0$ and $\delta(S)'' < 0$). Effort comes along with costs, which directly affect individuals' budget constraints. Thereby, a trade-off arises: S reduces the budget for consumption but shifts present consumption to the future, which will increase lifetime utility U if u is concave. As long as the benefit from more future consumption outweighs the loss from diminishing consumption opportunities, individuals will increase their effort until the optimum S^* is reached.

This optimum shifts, however, if the opportunity costs of S change. According to Becker and Mulligan (1997), losses in lifetime wealth or increasing mortality can have this impact. Consequently, S^* and, thereby, $\delta(S^*)$ are persistently changing units.

If this proposition holds true, many empirical results have to be reconsidered. If health decisions reduce lifetime expectations and thereby individuals' discount factors, the relationship between discounting and health as identified in the literature could not be one-, but two-sided. Both measures affect each other, implying a simultaneity bias. Similar concerns arise for studies identifying a positive relationship between discounting and labor market success.

Changing preferences are, however, not necessarily correlated with economic outcomes. Measurement biases can also result in time-varying tastes for future rewards. However, in contrast to the endogeneity bias from above, the resulting attenuation bias causes an underestimation of marginal effects only (see Cobb-Clark and Schurer, 2013). The direction of the error is thus predetermined. Moreover, random changes do not challenge the exogeneity assumption per se. The reasons of instability, therefore, have contrasting implications. Differentiating between structural and random changes within time preferences is thus obligatory.

2.2.2 Previous literature on intra-individual instability during adulthood

Whether time preferences still evolve during adulthood – the time in life typically considered in economic modeling – is addressed in only a few studies. Kirby *et al.* (2002) test the stability of the time discounting parameter δ within 154 members of a horticultural society in the Bolivian rain forest within one year and identify a relatively high degree of stability. Harrison *et al.* (2005) present similar results using repeated time preference experiments with 97 Danes. Testing approximately 200 participants of an income tax assistance center in Boston (US), Meier and Sprenger (2015) find that time preferences imply a sufficient degree of stability within one year, considering them as 'stable'. However, the authors conclude that instability within time preferences is mainly caused by measurement issues as preferences do not vary considerably with life circumstances.[1]

There are, however, several, specific events affecting the level of δ. Krupka and Stephens (2013) and Dean and Sautmann (2014) find economic circumstances on a general (e.g. inflation) and an individual (e.g. employment) level to affect preferences. In addition, violence (Voors *et al.*, 2012) and natural disasters (Callen, 2015) shape time discounting.

Although findings on risk attitudes indicate that cognitive aging can have a substantial impact on economic attitudes (see Bonsang and Dohmen, 2015), evidence on time preferences' age dependency are inconclusive so far, probably due to small observation numbers. Relying on cross-sectional data, Harrison *et al.* (2002), Read and Read (2004), and Richter and Mata (2018) find middle aged individuals to report the lowest discount-factors, which points to a U-shaped relationship. In contrast, Green *et al.* (1999) and Löckenhoff *et al.* (2011) identify a linear relationship, while Green *et al.* (1994) and Chao *et al.* (2009) do not find any age trend.

Studies on the stability of present bias are even scarcer. However, existing evidence suggests that β and δ are similar with respect to their stability; β is considerably stable within one year (Meier and Sprenger, 2015) but varies with financial constraints (Carvalho *et al.*, 2016).

All these findings elicit time preferences with the 'sooner-smaller or later-larger reward' approach; yet, this approach is time-consuming and costly, which is a likely reason why these studies consider relatively short time frames only. Stability in the short-run must,

[1] Additionally, Kirby (2009), Wölbert and Riedl (2013) and Halevy (2015) test the stability of time preferences within student samples. While the first two find stable time preferences within five to ten weeks, the latter identifies considerable instability within seven days. However, since personality traits are usually considered to settle down in the late 20s and thus after higher education (Costa Jr. and McCrae, 1994), instability within students should not be considered as representative for adulthood.

however, not imply stability in the long run. Additionally, sample sizes within the previous literature are very small, and data is often very selective and thus not representative. Moreover, the experimental approach itself is subjected to scrutiny since elicited preference parameters depend considerably on the laboratory setup and often do not correlate with decision-making outside the lab (Frederick *et al.*, 2002). Reasons could be insufficient financial literacy or incorrect assumptions on individuals' utility functions (Borghans *et al.*, 2008; Andreoni and Sprenger, 2012).

In light of these limitations, eliciting preferences by a survey approach has evolved as a promising alternative. While preserving behavioral validity, survey approaches need neither monetary intensification nor much time. They are thus easily implementable in longitudinal and representative data sets, allowing one to tackle questions of stability within much larger time spans and samples. Approaching time preferences by such a psychological point of view is, however, not new to economic research. Before Samuelson (1937) introduced δ as a convenient and elegant formalization of time preferences, discussions on the reasoning of wealth accumulation were mainly driven by individuals' motives and abilities to anticipate future needs (see Frederick *et al.*, 2002). The stated preference approach falls back on this tradition, as it does not focus on one parameter but on individuals' abilities and motivation to postpone consumption.

Survey approaches have already contributed to the stability discussion. Toepoel (2010) uses the same data set as this study and concludes that the measure proposed by Strathman *et al.* (1994) has a sufficient intra-individual correlation to consider it as a stable trait. She notes, however, that the stability is diminishing the longer the considered time frame is, but reasons behind this decreasing stability are not discussed. Also using the DHS, Hardardottir (2017) finds macroeconomic conditions to affect three items of the CFC measure in one year's perspective. Whether these results hold in the full, behaviorally valid CFC scale is not verified. Drichoutis and Vassilopoulos (2016) test the stability of patience and impulsiveness. Within their sample of 80 survey participants, results imply that these indicators are relatively stable within two years. The sample size, again, does not allow for a more detailed analysis. Relying on the same measures, Meier (2018) finds emotions like happiness, anger, and fear to affect reported patience.

2.2.3 Contribution

This empirical investigation fills several research gaps. First, it extends the analysis of Toepoel (2010) and evaluates the effect of several common life events on the consideration of future consequences scale. The present study can thus evaluate whether this survey approach has similar properties on stability as the experimental approach applied in Krupka and Stephens (2013) or Meier and Sprenger (2015). The first contribution is thus replication of previous results and verification of the stated preference approach.

Second, previous evidence on changing time preferences is mainly concerned with income, age, or employment. The following analysis extends this list by the following events: retirement, becoming a (grand-)parent, and health issues. Retirement is analyzed as an effect of it on time discounting could explain the retirement-consumption puzzle – the observation that individuals tend to save more after retirement although the permanent income hypothesis predicts otherwise (e.g. Haider and Stephens, 2007; Battistin *et al.*, 2009). Discussions on dynasty discounting in the sense of Becker and Barro (1988) motivate this chapter to test family-related events, as descendants (e.g. children or grandchildren) may function as a lifetime extension. Since health issues may have an opposing effect on life expectancy, they are considered, too. This study thus not only tests the exogeneity assumption of time preferences, but it also examines other, so far unanswered theoretical propositions.

At last, the upcoming analysis provides evidence on the timing of changes. Previous studies tested the immediate effect of events on time preferences only. It is not clear yet whether these changes are persistent or only temporary (as it is the case in risk attitude or personality traits, see Schildberg-Hörisch (2018) or Chapter 3 and 4). In advance to the existing literature, the longitudinal information in the Dutch Household Survey allows testing for anticipation and persistency presented in this chapter.

2.3 Data

2.3.1 Measures, data set and samples

The following analysis builds on the Dutch Households Survey (DHS, see CentERdata, 2018), a representative panel data set in the Netherlands. Every year since 1993, approximately 2,000 households complete up to six sets of questionnaires about various aspects of their life, ranging from work and income to investment decisions and wealth (de Bruijne *et al.*, 2014). The DHS stands out from other panel data sets due to its extensive number of items on psychological concepts and economic attitudes. One of these concepts is the scale on '*consideration of future consequences*' (CFC). The following section describes the data set and its measure on time preferences in more detail.

Consideration of future consequences The DHS includes the CFC almost annually.[2] It reflects how much people consider the future consequences of their actions. It thus depicts, on the one hand, whether individuals look into the future and, on the other hand, how

[2] The DHS wave 2008 did not include the CFC at all. Between DHS waves 2010 and 2015, participants answered the CFC only if they did not do so in the previous interview.

Table 2.1: CFC questionnaire in the DHS

Question:	*Now we present you some statements about the future. Please indicate for each statement to what extent you agree or disagree.* *To what extent do you agree with the following statements?* *Please indicate on a scale from 1 to 7 to what extent you agree with the following statements. 1 means 'extremely uncharacteristic'. 7 means 'extremely characteristic'.*
Item No.	
I1.	I think about how things can change in the future, and try to influence those things in my everyday life.
I2.	I often work on things that will only pay off in a couple of years.
I3.	I am only concerned about the present, because I trust that things will work themselves out in the future.
I4.	With everything I do, I am only concerned about the immediate consequences (say a period of a couple of days or weeks).
I5.	Whether something is convenient for me or not, to a large extent determines the decisions that I take or the actions that I undertake.
I6.	I am willing to sacrifice my well-being in the present to achieve certain goals in the future.
I7.	I think it is important to take warnings about negative consequences of my acts seriously, even if these negative consequences would only occur in the distant future.
I8.	I think it is more important to work on things that have important consequences in the future, than to work on things that have immediate but less important consequences.
I9.	In general, I ignore warnings about future problems because I think these problems will be solved before they get critical.
I10.	I think there is no need to sacrifice things now for problems that lie in the future, because it will always be possible to solve these future problems later.
I11.	I only respond to urgent problems, trusting that problems that come up later can be solved in a later stage.
I12.	I find it more important to do work that gives short-term results, than work where the consequences are not apparent until later.

Source: DHS 1996-2017 based on Strathman et al. (1994).

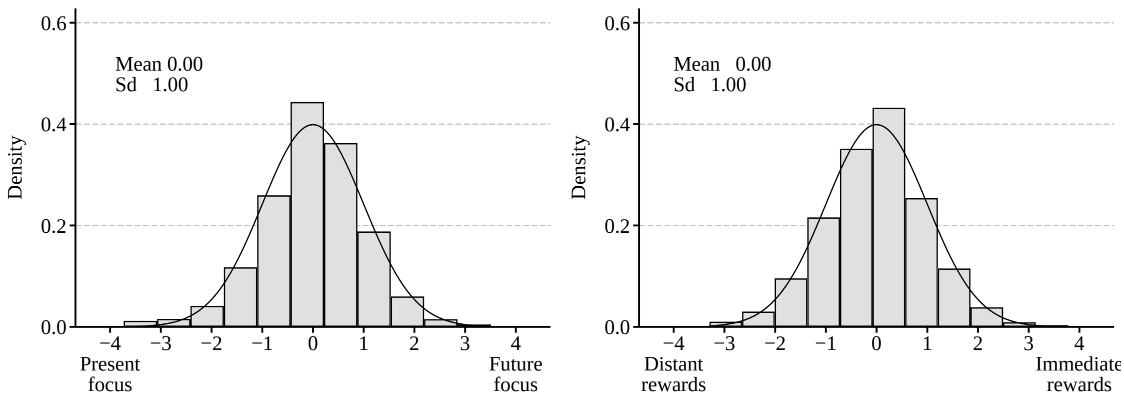
much the corresponding outcomes are valued. To elicit the CFC, participants rate twelve statements on a scale from 1 'extremely uncharacteristic' to 7 'extremely characteristic' (see Table 2.1).

Until 2004, item 12 was not included in the DHS. In order to maximize the sample size and make use of the full time span of the panel, this study relies on eleven items only. In fact, the internal validity of the scale increases by this restriction. Cronbach's alpha rises slightly from 0.75 in the 12-item-scenario to 0.77 when eleven items are used. Based on the remaining items, factor analysis is executed using the principle-component factor and varimax for rotation. Beforehand, the analysis reverses items 3, 4, 5, 9, 10 and 11 such that increasing agreement implies a more future oriented attitude.

Table 2.A1 (see Appendix 2.A) presents summary statistics as well as factor loadings of the CFC questionnaire. Following Toepoel (2010) and Joireman *et al.* (2008), this study looks at *two* latent factors within the CFC. Although Strathman *et al.* (1994) introduced the CFC as an univariate factor, Joireman *et al.* (2008) argue in line with Petrocelli (2003)

Figure 2.1: Distribution of the consideration of future consequences scale

(a) CFC-future *(b) CFC-immediate*



Source: DHS 1996-2017, own calculations.
Note: Both graphs are histograms; y-axis denotes densities of categories (measured in sd). Black line denotes standard normal distribution. Predicted factors by factor loadings presented in Table 2.A1 (see Appendix 2.A). 26,476 observations used.

that the CFC should be divided into CFC-future and CFC-immediate, as both factors reflect distinct dimensions of forward-looking behavior. On the one hand, CFC-immediate correlates very strongly with short-run decision-making and self-control abilities (see Joireman *et al.*, 2008). Its extrema could thus be best described by individuals who prefer immediate rewards. On the other hand, CFC-future reflects individuals' ability to anticipate and plan future outcomes.

Factor loadings are similar to Toepoel (2010). With the exception of items 4 and 5, all items load sufficiently on CFC-future (e.g. loadings are greater than 0.40). CFC-immediate corresponds with all items except 1, 2, 3 and 6. This analysis reverses CFC-immediate such that an increasing score implies more impulsive behavior. The resulting two factors are standard normally distributed (see Figure 2.1). By definition, CFC-immediate and CFC-future are orthogonal.

Variables Aside the CFC, the empirical investigation relies on several socio-demographic information (education, income, height, main occupation), health conditions (smoking, weight, height, alcohol consumption), and information on consumption-saving preferences (planning horizon, saving plans). In addition, several life events are identified. Table 2.2 lists the positive and negative events and how they are computed. Overall, the analysis looks at four different dimensions of situations; that is, experiences associated with labor market occupation, income, family, and health. Herein, the study explicitly distinguishes between positive and negative income and health events, as they may affect individuals differently.

Table 2.2: List of considered events

	Abbreviation	Definition
Labor market		
(1)	Unemployment	Unemployed and looking for work in $\tau = 0$
(2)	Retirement	Any kind of retirement
Income		
(3)	Income improvement*	Compared to last year, income is unusually high
(4)	Income worsening*	Compared to last year, income is unusually low
Family		
(5)	Marriage	Change from unmarried to married status
(6)	Child birth	Number of children in household increases
(7)	Grandchild birth	Number of grandchildren increases
Health		
(8)	Health improvement*	Compared to last year, health is better
(9)	Health worsening*	Compared to last year, health is worse

*Note: * events identified directly with survey items in the DHS.*

Within the DHS, income is measured with a six-category variable, so changes in this variable do not have enough variation to identify exact income changes. Alternatively, the analysis relies on perceived changes in income, i.e. individuals report whether their income is unusually high or low in the respective year. A similar question is used with respect to health. The DHS asks participants whether their health has improved or worsened compared to the previous year. As a complementary test on health issues, the analysis tests the effect of more objective indicators, too, such as reporting a change in the main occupation ‘disabled’ and reporting a long-term illness, disorder, handicap, or consequences of an accident (in the following ‘severe illness’).

A limitation of the CFC – and the stated preference approach in general – are survey biases, in other words, differences between the actual and reported preferences. Social desirability or individuals’ current mood could, in principle, motivate survey participants to misreport their answers (un)intentionally (Paulhus, 1984), causing a survey bias. Instability within CFC would then not reflect changes in preferences but measurement issues only. Two tests are conducted to test whether CFC is prone to these issues. First, the emotional status is used as a proxy for individuals’ mood. Within the DHS, survey participants report the frequency of four different emotions on a six-item scale ranging from ‘never’ to ‘continuously’. From 2014 onwards, the frequency of feeling anxious, sad, depressed, and happy is available. As a second test, the timing of the interview (weekdays or weekends) is also used. Similar to emotions, the timing could correspond with individuals’ mood and thereby explain variation in time preferences. The exact date (day, month and year) of the CFC interview is available between 1996 and 1999.

Lastly, the CFC and its stability will be compared with the stability of other economic preferences and personality traits, i.e. the Big Five taxonomy (e.g. openness, conscientiousness, extraversion, agreeableness, neuroticism), locus of control, and risk attitude. To estimate individuals' Big Five, a factor analysis of 15 items is conducted (see Dehne and Schupp, 2007). Locus of control and risk attitude rely on six trait-specific survey items (see Dohmen *et al.* (2017) and Chapter 4).

Sample The following analysis restricts the sample to men and women between 20 and 70 years of age. Observation numbers out of this interval are too small for analysis. This leaves 29,714 observations with answers to all eleven CFC items at least once between 1996 and 2017. Thereof several observations get lost due to sample attrition and missing variables. To maximize the sample size, this study relies on several sub-samples. Summary statistics of the cross-section sample and the one, thee and five years longitudinal sample are presented in Table 2.A2 (see Appendix 2.A).

2.3.2 Behavioral validity

The CFC is a well-established measure in the psychological literature. Joireman and King (2016) give an extensive overview of its correlations with revealed behavior in every-day situations. Individuals scoring high in the CFC report healthier behavior as well as stronger forward-looking financial and environmental decision-making. Additionally, the CFC predicts job search behavior (van Huizen and Plantenga, 2014; van Huizen and Alessie, 2015) and standard laboratory time-discounting parameters (Joireman *et al.*, 2005, 2008; Daly *et al.*, 2009).

Within the DHS, replication of these results is straightforward when assuming exogeneity of CFC and neglecting potential reverse causality issues discussed above. Table 2.A3 in Appendix 2.A summarizes the corresponding results. Estimating the probability to smoke, having a BMI above the sample's 75th percentile, or drinking alcohol regularly indicates that individuals who focus strongly on the present or prefer immediate rewards behave less healthily. In line with human capital theory, both CFC dimensions predict the probability of holding a college degree. Furthermore, individuals scoring high in CFC-future or low in CFC-immediate report more often that they plan five or more years ahead in their household decisions. Consequently, they also tend to save more often. These results are very robust concerning additional controls on other personality traits (e.g. the Big Five taxonomy or risk attitude).[3]

[3] Additionally, to its predictive properties on behavior, the CFC correlates with alternative psychological measures related to time-dependent decision-making (e.g. delay of gratification, procrastination, self-efficacy, optimism, and locus of control, see Joireman and King, 2016). As it has also considerable intra-individual stability (Toepoel, 2010), the scale fulfills all conditions of a personality trait (see Frederick *et al.*, 2002).

Although CFC-immediate and CFC-future's wording and marginal effects correspond, respectively, with predictions on present bias β and discount factor δ, they do not necessarily represent only one parameter. Differentiating between β and δ outside the laboratory is, in principle, not feasible and speculation only since the CFC questionnaire does not explicitly aim to distinguish between the parameters. Accordingly, the following study assumes that a change in one of the CFC factors resembles changes in time preference as such and not in one specific parameter. The analysis is thus not bounded to one specific functional form.

2.4 Empirical strategy

To evaluate whether time preferences change structurally, the following analysis tests the CFC for its dependency on age and common life events. To make use of the full potential of the panel data set, different econometric models are used, which are introduced in the following section.

Identifying stability with respect to age To identify the effect of age on the time preference measures, the study follows Cobb-Clark and Schurer (2013) and relies on the following model:

$$\Delta Y_{i\tau} = \mathbf{Sex}_i \times \mathbf{Age}_i \theta_{sex,age} + \mathbf{Cohort}_i' \gamma_1 + \gamma_2 Growth_i + \varepsilon_i. \tag{2.1}$$

On the left hand side, Model (2.1) considers the intra-individual change in one of the time preference measures, i.e. $\Delta Y = Y_t - Y_{t-\tau}$ with $Y \in \{\text{CFC-future, CFC-immediate}\}$, t as time indicator, and τ as a lag of interest. To look at short-, mid- and long-term changes, the model considers $\tau \in \{1,3,5\}$. To avoid any pre-determined functional form of age's effect, age is included as a categorical variable in the model (**Age**). As gender may have diverging age trends, these categories are interacted with a gender dummy (**Sex**). $\theta_{sex,age}$ thus reports the average change in Y for a specific age-sex-combination. As this fragmentation comes along with low observation numbers in some cells, since the analysis groups individuals within intervals of two years of age. In cases where observations within age-sex cells falls below 25, the analysis does not report the corresponding coefficient. As an alternative to the categorical specification, the analysis includes tests on age as a continuous variable and estimates the linear effect of age and age squared on ΔY.

Although the first difference perspective applied in Model (2.1) already accounts for time-invariant characteristics, θ_{age} might not capture age effects only. Since cohort or calendar time varies uniformly with age, the resulting coefficients may capture their effect on ΔY, too (see Dohmen *et al.*, 2017). Model (2.1), therefore, accounts for these parallel channels. **Cohort** denotes a vector of birth cohorts in ten-year intervals, starting in 1930. Since age and cohort are multicollinear related to calendar fixed effects, year fixed effects cannot be included directly. Instead, economic growth is used as a proxy for time fixed effects (Heckman and Robb, 1985). Growth is measured as a percentage change in GDP per capita. Note that Model (2.1) does not include the usual constant. It thus refrains from normalization to one particular age group to ease graphical illustration. Finally, ε denotes the residuum.

Identifying stability with respect to events Section 2.5.3 aims to test the stability of both CFC factors with respect to various life events. The estimation procedure follows Clark *et al.* (2008) and estimates the impact of various lags and leads of an event D on Y within a fixed-effect estimation:

$$Y_{it} = \alpha_i + \sum_{\tau=-2}^{1} \theta_\tau D_{\tau,it} + \mathbf{X}_{it}'\gamma + \varepsilon_{it}. \tag{2.2}$$

Relying on the annual routine of the DHS, $D_{\tau=0}$ equals 1 if the event of interest occurred within the last 12 month, 0 otherwise (see Table 2.2 for the full list of considered events). Accordingly, the first lead of this binary variable ($D_{\tau=-1}$) equals 1 if the event is observed within the next 12 months. The first lag and the second lead imply that the event happened or is going to happen in 12 to 24 months, respectively. By including the individual fixed effect α_i, θ_τ identifies anticipation effects ($\tau \in \{-2,-1\}$), immediate effects ($\tau = 0$), and the effect's persistence ($\tau = +1$) within individuals. Note that these coefficients are not necessarily causal. Changing time preferences might affect decision-making, which would reverse the causal relation between D and Y. Nevertheless, sign and significance of θ_τ reveal whether changing preferences are structurally associated with certain life events.

X denotes a standard set of time-variant socio-demographic control variables (marital status, income level, number of children in household, employment status) as well as year and region fixed effects. Together with individuals in the sample who do not report D at any point in time, **X** allows Model (2.2) to control for general trends in the CFC which would otherwise be falsely accounted by θ_τ.

Because the DHS sends participants six different questionnaires separately within several weeks, D and Y are not necessarily measured at the same point in time. This affects the labor, family and health-related events. For example, interviews on CFC and current labor market occupation are separated on average by 6.5 weeks.[4] In consequence, estimates on θ_τ could be underestimated. However, the risk is limited as the analysis looks

[4] Information about questionnaires' timing is available from 2011 onwards only.

at events that are either anticipated (e.g. unemployment, retirement, childbirth) or expected to affect individuals for longer time frames (e.g. health issues).

Sample attrition imposes another potential restriction to the analysis. Life events – especially the negative ones – may motivate survey participants to leave the panel. If these 'leavers' differ from the 'stayers' with respect to their preferences' stability, θ_τ does not necessarily hold for everyone, and the results would not be representative anymore. Since the stability of time preferences within the leavers are unobservable, the analysis needs to make an assumption at this point, that is, stayers and leavers do not differ with respect to their preferences' stability.

Robustness analysis As a sensitivity test, the analysis aims to test the generalizability of the results from Model (2.2) by testing subgroups of interest separately. Because the fixed effect model from above implies high demands on data, observation numbers are, for some events, not sufficiently large to allow a meaningful sub-sample analysis. Therefore, Section 2.5.4 introduces a first difference model which relaxes the minimum number of observations per individual. The corresponding model reads

$$\Delta Y_{i,2} = \alpha + \theta_{FD} D_i + \Delta \mathbf{X}_i' \gamma_1 + \mathbf{Year}_i' \gamma_2 + \mathbf{Region}_i' \gamma_3 + \varepsilon_i. \tag{2.3}$$

To account for anticipation effects, Model (2.3) focuses on the two-year difference, i.e. $\Delta Y_2 = Y_t - Y_{t-2}$. As before, D equals 1 if the event of interest happens in the last twelve months (between t and $t-1$). Therefore, θ_{FD} includes the anticipatory and the immediate effect of D on Y. $\Delta \mathbf{X}$ denotes the first difference of all previously considered socio-demographic control variables. Therefore, γ_1 has the same interpretation as γ from Model (2.2). **Year** and **Region** denote time and region fixed effects. α and ε denote the average change in Y and the residuum, respectively.[5]

2.5 Results

2.5.1 Intra-individual stability

As a first step, the analysis tests if time preferences change within individuals across time and how strong these changes are. This is done, on the one hand, with respect to various time horizons. Changes in time discounting may have long-term trends that are not observable within one year. On the other hand, the analysis compares the stability of the time-preference measures with risk attitude and several personality traits, which puts

[5] Although Model (2.3) relaxes the demands on observations, it fixes the reference level of Y to one specific point in time, $t-2$. If event D already affects Y in this period, θ_{FD} underestimates the actual effect of D. This does not affect Model (2.2), which is why it is the more robust and, therefore, the preferred specification.

Table 2.3: Intra-individual correlations of time, risk and personality measures

	Pearson coefficient			Spearman coefficient		
	(1) Δ1 year	(2) Δ 3 years	(3) Δ 5 years	(4) Δ1 year	(5) Δ 3 years	(6) Δ 5 years
CFC-future	0.517	0.458	0.460	0.526	0.460	0.458
CFC-immediate	0.521	0.466	0.455	0.524	0.471	0.468
Risk attitude	0.681	0.638	0.610	0.669	0.625	0.597
Locus of control	0.535	0.541	0.496	0.527	0.525	0.510
Big Five						
Openness	0.701	0.707	0.640	0.690	0.693	0.619
Conscientiousness	0.594	0.558	0.544	0.599	0.568	0.582
Extraversion	0.703	0.700	0.683	0.681	0.685	0.659
Agreeableness	0.640	0.573	0.555	0.633	0.573	0.573
Neuroticism	0.744	0.713	0.650	0.739	0.694	0.654

Source: DHS 1996-2017, own calculation.
Note: Sample includes men and women between 20 and 70 years of age. Observation numbers of one, three and five year case are: 14,028, 7,034 and 4,392 within CFC; 25,046, 15,668 and 10,850 within risk attitude; 2,798, 1,087 and 873 within locus of control; 2,713, 1,005 and 809 within the big five.

the stability into perspective. Table 2.3 presents intra-individual Pearson and Spearman correlations. The analysis looks at individuals with longitudinal information on their CFC only. The baseline sample of 29,714, thereby, reduces to 14,028 (7,034 / 4,392) observations when the annual (three- / five-years) difference is tested.

In line with the literature, the Big Five, locus of control, and risk attitude inhabit a considerable intra-individual stability (Cobb-Clark and Schurer, 2012, 2013; Schildberg-Hörisch, 2018). Looking at the one-year difference in the DHS (see Column (1) of Table 2.3), intra-individual Pearson correlations among these measures range from 0.535 within locus of control to 0.744 within neuroticism. Correlations of the CFC measures are the lowest in the list with 0.521, implying a considerable time-invariant component. The correlation is, however, in line with experimental results of Meier and Sprenger (2015).

All stability indicators diminish as time goes by (see Column (2) and (3)). However, the loss in correlation moves parallel in all measures such that the ordering of the traits with respect to their intra-individual stability is almost not affected. Alternatively to Pearson's correlation coefficients, Table 2.3 presents evidence on rank-stability, a common criteria for a trait's stability within the psychological literature (see Roberts and Mroczek, 2008). Computing Spearman's rank correlation coefficient reaches equivalent results (see Column (4) to (6)).

Figure 2.2 allows one to look deeper into the intra-individual stability and presents the distribution of changes in the CFC again for three different time horizons. Independently from the considered time frame, a graphical analysis reveals almost symmetric distributions around the mean within CFC-future and CFC-immediate, indicating no systematic changes in general. In the one year analysis of CFC-future, 98 % of the sample are in the range from -2.58 to 2.40 sd, 50 % between -0.56 sd and 0.52 sd (see Figure 2.2a). Roughly 10 % of the sample in Figure 2.2a report ΔCFC-future between ±0.1 sd. The analysis finds similar intervals for CFC-immediate and longer time horizons. The results are thus ambivalent. On the one hand, there is substantial variation; on the other, many individuals report almost no change at all.

Additional tests do not reveal differences between genders. Two-sample Kolmogorov-Smirnov tests find a weak significant difference between ΔCFC-future distribution of men and women in the one and five-year cases only ($p = 0.054$ and $p = 0.095$, respectively). Here, changes within women tend to be more negative.

In summary, time preferences inhabit a substantial degree of instability. The next question is, therefore, where does this instability stem from. Is it structural and thereby implying endogeneity, or is it only white noise due to measurement issues? In the following, the extent to which instability in time preferences is structural is tested. This is done with respect to two categories: Section 2.5.2 tests for age effects, Section 2.5.3 analyzes life events.

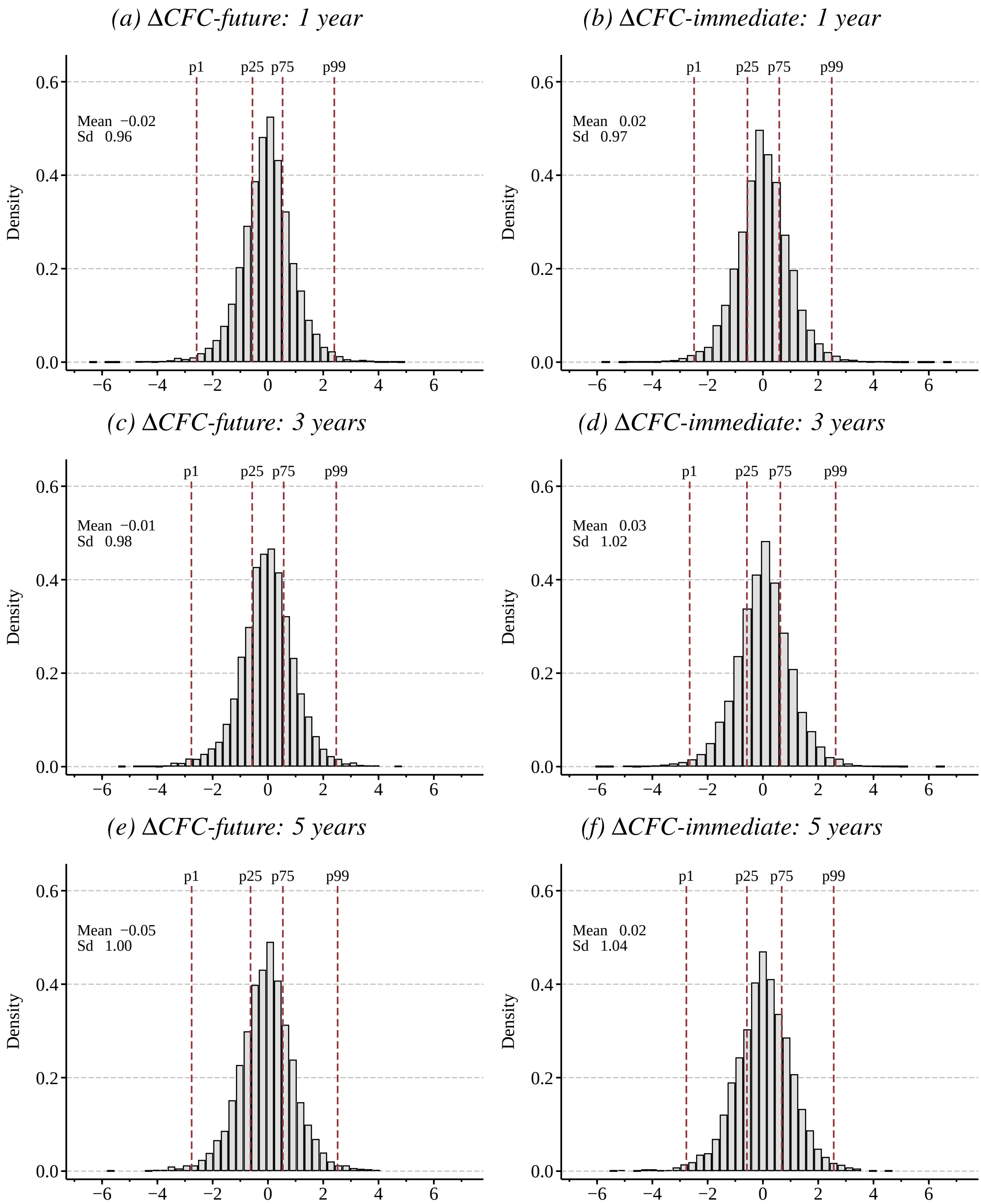
2.5.2 Stability with respect to age

As the first test on structural changes in time preferences, the analysis aims for age trends. For this purpose, the following section discusses the estimation results of Model (2.1). Figure 2.3 presents the corresponding results. Recall that Model (2.1) includes age categories to avoid any restrictions by pre-determined functional forms. All estimations include cohort fixed effects and economic growth as additional control variables and interact the age categories with a gender dummy. The analysis uses the same samples as Section 2.5.1.

Within the one year difference, both CFC factors are considerably stable (see Figure 2.3a and 2.3b). From 30 years of age onwards, almost no changes can be observed within any specific age group, neither for women nor men. Confidence intervals are relatively equal among all age groups, indicating that no particular group is either very stable or unstable in its preferences. An exemption are those younger than 30. Here, CFC-future seems to change iteratively. This points towards the common perception of instability within personality traits during early adulthood. Wide confidence intervals within CFC-immediate for those before the age of 30 indicate a similar pattern.

Looking at three and five-year changes in the CFC reveals a weak negative trend in CFC-immediate. While CFC-future is relatively stable and does not change significantly with respect to age (see Figure 2.3c and 2.3e), CFC-immediate falls constantly from 50 years of age onwards (see Figure 2.3d and 2.3f). Accordingly, aging individuals tend

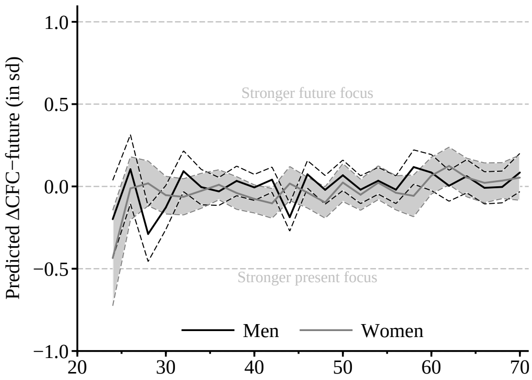
Figure 2.2: One, three and five years differences in CFC factors (in sd)



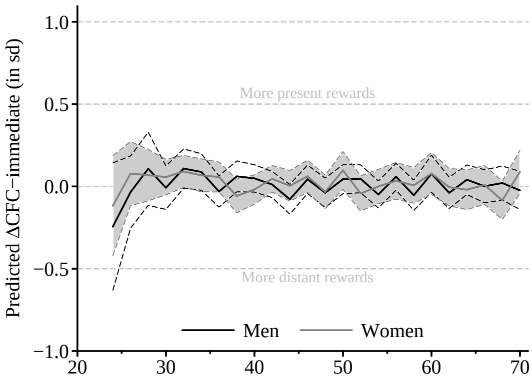
Source: DHS 1996-2017, own calculations.
Note: All graphs are histograms; y-axis denotes densities of categories of changes in CFC factors (measured in sd). Sample in (a) and (b) is 14,028, in (c) and (d) 7,034 and in (e) and (f) 4,392. p1, p25, p75 and p99 denote the percentiles of the corresponding distribution.

Figure 2.3: One, three and five years change in CFC factors by age

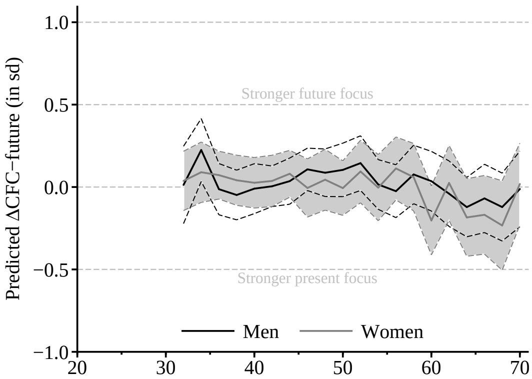
(a) ΔCFC-future: 1 year



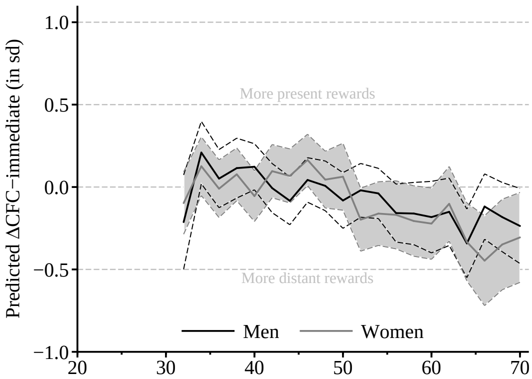
(b) ΔCFC-immediate: 1 year



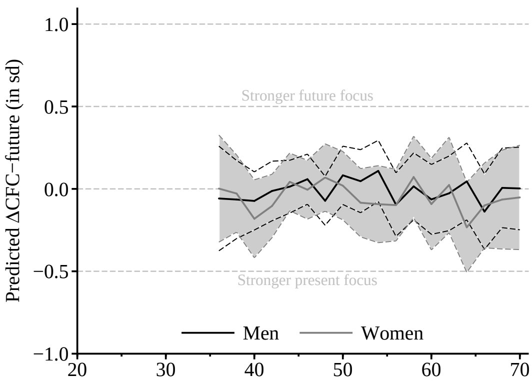
(c) ΔCFC-future: 3 years



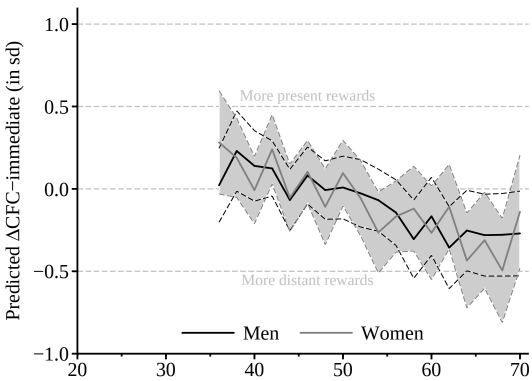
(d) ΔCFC-immediate: 3 years



(e) ΔCFC-future: 5 years



(f) ΔCFC-immediate: 5 years



Source: DHS 1996-2017, own calculations.
Note: X-axis denotes age. Dashed lines represent 95% confidence intervals based on clustered robust standard errors. Sample in (a) and (b) is 14,028, in (c) and (d) 7,034 and in (e) and (f) 4,392. Additional control variables are economic growth and cohort fixed effects.

to prefer distant rewards more than immediate ones. To some respect, this finding is counterintuitive since older age groups have no objective reason to become more future-focused. However, the five-year changes within each age cell are relatively small (approx. -0.25 sd), implying a change in CFC-immediate by 1 sd in 20 years only. With respect to Figure 2.2, a change of this amount does not explain much of CFC's variation. In addition, the negative trend in CFC-immediate depends on the cohort fixed effect to a large extent (see Figure 2.A1 in Appendix 2.A). The negative age effect is thus not robust.[6]

Table 2.A4 (see Appendix 2.A) repeats the estimation with an explicit assumption on the functional form of the age effect, a second-degree polynomial. Estimations point out a positive but decreasing effect of age on CFC-future for all considered time perspectives; yet, the marginal effect is relatively small. In addition, Table 2.A4 also emphasizes the importance of current macroeconomic conditions. Current economic growth has a highly significant effect on ΔCFC-future as discussed by Hardardottir (2017). However, the effect is relatively small – a 1 %-point growth increases CFC-future on average by 0.03 sd. The negative trend on CFC-immediate indicated by Figure 2.3d and 2.3f does not manifest itself in Table 2.A4. Here, age has no effect on ΔCFC-immediate. F-tests on different coefficients between genders do not indicate diverging trends.

In summary, age affects time preferences only weakly. The instability identified in Section 2.5.1 does not stem from this dimension.

2.5.3 Stability with respect to events

The analysis now turns to the identification of instability with respect to life events. The following section uses Model (2.2), an individual fixed effect model which identifies within-changes in the CFC before and after the event of interest. In contrast to Section 2.5.1 and 2.5.2, the event analysis needs – aside longitudinal CFC information – various socio-demographic characteristics to identify the life events of interest. The analysis uses topic specific samples in the following estimations to minimize data losses. Observation numbers are summarized in Table 2.A5 (see Appendix 2.A).

Figure 2.4 presents the results of the event analysis and the corresponding coefficients on CFC-future (black lines) and CFC-immediate (gray lines). All presented estimations use a standard set of control variables as described in Section 2.4. Whiskers denote the 95 % confidence interval based on robust standard errors. At first, a focus is put on

[6] Cohorts are defined in intervals of ten years. Due to the long interval of the DHS (20 waves), the analysis observes individuals within one cohort at different ages. For example, cohort '1950-1960' is observed within 16 age groups. The overlap between cohorts is thus sufficient to differentiate between cohort and age effects. However, the overlap of the first (1930-1940) and the last cohort (1980-1990) with other cohorts is – by definition – smaller, restricting the reliability of the corresponding coefficients to some extent. Testing the five-year change in CFC-immediate is the only estimation indicating significant cohort effects. CFC-future and other time horizons are not significantly affected.

unemployment for an illustrative reason. All of the following results can be interpreted analogously.

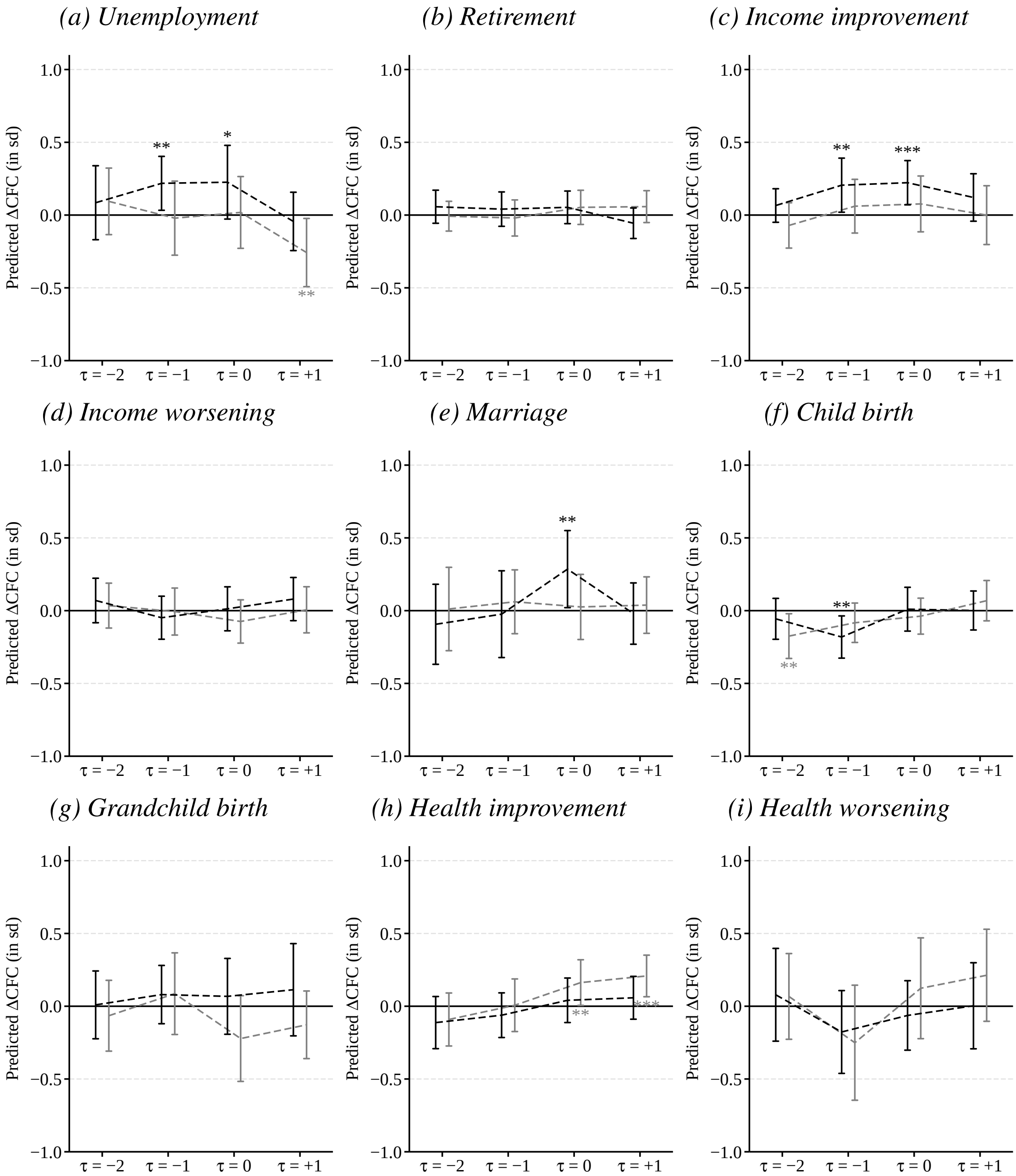
Figure 2.4a implies that individuals change their forward-looking behavior due to a job loss. Losing employment and being unemployed in $\tau = 0$ increases the focus on the future by 0.23 sd (see black line). Even before the job loss takes place, individuals change their CFC-future. In $\tau = -1$, estimations identify a significant increase in the corresponding trait by 0.22 sd. It is thus not unemployment itself affecting CFC-future. The perspective of losing employment is sufficient. In $\tau = +1$ individuals do not deviate from their long-run average CFC-future anymore, implying that losing one's job has only a transitory effect. CFC-immediate is affected at a later point in time only (see gray line). At least twelve months after the loss of employment took place ($\tau = +1$), individuals report a significantly stronger focus on distant rewards. This effect is exclusively driven by individuals still unemployed in $\tau = +1$. Accordingly, long-term unemployment appears to have its own, additional effect on individuals' time preferences.

As a robustness test, the group of unemployed is extended by those who report benefits from the short-term unemployment insurance scheme in the last twelve months thereby including those with very short-term unemployment spells in the analysis, too. Although the sample diminishes to some extent as information on unemployment benefits is not available for every individual, Figure 2.A2a (see Appendix 2.A) reveals the same line of effects within CFC-future. As the majority within this group is already re-employed in $\tau = 0$, the effect of long-term unemployment on CFC-immediate is not observable within this test.

Inconsistency within time preferences may explain the lack of consumption smoothing after retirement, but, according to Figure 2.4b, retirement does not come along with a change in the CFC. The retirement-consumption puzzle thus does not stem from changing time preferences. With respect to income, only CFC-future is affected. If individuals report that their income is unusually high in $\tau = 0$, they increase CFC-future by 0.22 sd on average (see Figure 2.4c). Like unemployment, this effect is anticipated one period before. CFC-immediate does not change. A negative income shock affects neither CFC-future nor CFC-immediate (see Figure 2.4d). With reference to the results from above, changes due to unemployment are thus not caused by losses in income. The results, however, give a reasonable explanation for the dependency of time preferences and economic growth: income gains through economic growth may allow individuals to be more forward-orientated.

Considerations of dynasty discounting motivate this study to look at family-related events. Marrying or becoming parents may extend the perception of one's own life span and thereby the motives to value the future. Changes in the family setting have, however,

Figure 2.4: Predicted lagged and lead effects of life events on CFC



Source: DHS 1996-2017, own calculations.
*Note: Black (gray) line shows predicted changes in CFC-future (CFC-immediate). X-axis denotes lead and lags of the event. Whiskers represent 95 % confidence intervals based on clustered robust standard errors, *p<0.1, **p<0.05 and ***p<0.01. Control variables are employment and marital status, economic growth, income level, number of children, year and region fixed effects. See Table 2.2 for the definition of the events and Table 2.A5 (see Appendix 2.A) for number of observations.*

a minor impact only. A recent marriage increases CFC-future in $\tau = 0$ by 0.29 sd (see Figure 2.4e). A childbirth decreases it some time before the child is actually born (see Figure 2.4f). However, both effects are restricted to one point in time. Looking at the effect of the first child does not indicate any persistent effect on the CFC (see Figure 2.A2b in Appendix 2.A). The birth of a grandchild does not come along with a significant effect (see Figure 2.4g).

Contrarily to dynasty discounting, health problems may reduce subjective life span and increase individuals' weight on the present and immediate rewards. With respect to this conjecture, Figure 2.4h implies that a health improvement in $\tau = 0$ increases the preference towards immediate rewards significantly although the event is not limited to severe illnesses. Apparently, being in good shape or recovering from a long-term illness increases individuals' spontaneity. In contrast, reporting a worsening of health does not come with a significant, opposing effect although a negative tendency in $\tau = -1$ is observed (Figure 2.4g). As a robustness test, Figure 2.A2c and 2.A2d (see Appendix 2.A) look at the events of becoming disabled or severely ill. Here, only becoming disabled shifts CFC-future downwards, although it is only weakly significantly. Becoming severely ill – the closest (yet rare) measure to identify life-threatening health issues – does not affect the CFC significantly.

In summary, the analysis reveals that both CFC factors change with respect to various common life events, but these effects are temporarily restricted and considerably small. With the exception of long-term unemployment, effects are within an interval of ± 0.3 sd. With reference to the behavioral effects of the CFC (see again Table 2.A3), a gain in CFC-future by this amount would increase the probability of having a high BMI by -0.3 %-points, smoking by -0.5 %-points, planning at least 5 years ahead by 1.6 %-points, or saving by 0.8 %-points. The effect size is thus relatively small. The range of changes in the CFC is also in line with findings of Cobb-Clark and Schurer (2013) who identify locus of control to change by the same amount but conclude that these changes are economically negligible.

In addition, CFC-future and CFC-immediate are never affected simultaneously. Only one of the factors change for a time, while the other is constant. On the one hand, this reflects that CFC-immediate and CFC-future are truly distinct features of forward-looking attitudes, shaped by different events. On the other hand, it indicates that future-orientation is not changing entirely. Only distinct aspects of time preferences are affected at a time.

With this and the small, temporary effects in mind, concerns arise that the results reflect survey biases only. By assumption, the revealed CFC corresponds with the underlying preference for future rewards at any time. The difference between both, the so-called survey bias, is considered random and, therefore, irrelevant for the analysis. However, this assumption could be violated: individuals could perceive or describe themselves as more patient due to a specific event, although their future-oriented behavior is not affected. Consequently, the effects from above would be spurious only.

When individuals expect an unusually high income some time in the future and plan how to spend it, they might feel more forward-oriented, although their actual preference did not change. Similar concerns arise with respect to unemployment and health issues: expecting a job loss and looking for a new job or waiting for a health recovery are future-focused tasks which may shape individuals' perception, yet not their valuation of future consumption. Accordingly, as soon as these future-oriented tasks are not necessary anymore (e.g. due to a normalization of income or a successful job search), the effect vanishes again – just the like the effects from above. In consequence, this study cannot claim at this point that events truly affect time preferences. Before that, further tests on survey issues are needed.

2.5.4 Robustness

Survey biases Unfortunately, it is not feasible to differentiate between changes in actual and revealed preferences directly. One can, however, test whether the CFC is prone to survey biases in general. Minor events or day-to-day factors should not affect individuals' underlying preferences, as they come as fast as they go. If they change the CFC, nevertheless, it very likely implies a survey bias. To test for the impact of minor events, this study proposes two tests.

First, the analysis uses individuals' current emotional state as a proxy for individuals' mood. For this purpose, the analysis uses the reported frequency of feeling anxious, sad, depressed, or happy and extends Model (2.3) accordingly. The results are summarized in Panel A of Table 2.A6 (see Appendix 2.A).

The estimations imply that ΔCFC-future and ΔCFC-immediate correlate with changing emotions, although only to a weak significance level ($p < 0.1$). Reporting an increase in sadness by one point (on a six-item scale) corresponds with increasing CFC-future by 0.05 sd. The immediate effect of a job loss identified in Section 2.5.3 (0.22 sd) appears, therefore, as relatively small given that losing employment is such a detrimental life event. CFC-immediate changes with anxiety to a similar degree.

The second test employs the interview day as a proxy for mood. Doing the interview on weekdays may come along with a higher level of stress. To test whether the interview day affects the CFC, the fixed effect model from above is augmented by a dummy which equals 1 if an individual answered the CFC in the corresponding wave on a weekday (i.e. Monday till Thursday), 0 otherwise. Panel B of Table 2.A6 (see Appendix 2.A) presents the results.

Switching from weekends to weekdays between two consecutive survey years diminishes individuals' CFC-future by 0.1 sd. Since this effect should not come along with behavioral changes, it gives an impression on how sizable survey effects can be within the CFC; they amount to almost half of the effect of unemployment.

Effect heterogeneity The analysis presented in Section 2.5.3 implies considerable data demands, restraining the analysis from additional sub-sample tests. Reducing the sample to specific groups is, nevertheless, a necessary step to shed light on the generalizability of the results from above, that is, specific groups may react stronger to the events. To relax the data demands and to increase the sample size, the following analysis relies on Model (2.3), a first difference estimation on the change in the CFC from $t = -2$ to $t = 0$. Results are summarized in Figure 2.A3 (see Appendix 2.A).

The effect of unemployment is observed for women, older workers, and low-income households only. When the sample is split at the CFC's median, effects are significantly different from zero for individuals with a strong present focus. However, applying a joint estimation and testing the difference within subgroups' coefficients does not indicate statistically significant differences in any of the listed subgroups. CFC-immediate does not change significantly within any group due to unemployment. With respect to retirement, individuals with an income below the mean report a significant reduction in CFC-future; potentially due to additional financial restrictions. Additionally, men tend to increase their preference for immediate rewards, but, the effects are not substantial.

In contrast to the previous section, an income gain does not change CFC-future significantly in this alternative model. Since the first difference model fixes the reference level to one specific point in time ($t = -2$), the loss in significance is most likely a result of this restriction. The analysis reveals no changes due to income improvements within any subgroup. The same applies to income worsening. With respect to marriage or childbirth, the analysis reveals, again, no heterogeneity within groups. Health improvement causes CFC-immediate to increase within young individuals and high-income households only.

In summary, the analysis identifies no group that changes the CFC in particular. If the CFC changes significantly, effect sizes are not substantial. Accordingly, the results from the previous section are generalizable for a wide range of socio-demographic groups.

Selection issues Given that the results hold for various subgroups, sample attrition on observables does not threaten the representativeness of the analysis. However, a special form of sample attrition concerns the dependent variable. Being patient and forward-looking is likely to correspond with individuals' willingness to participate in surveys repeatedly. In contrast, impatient individuals may self-select out of the sample. Statements about them would then not be possible. To check this potential caveat, a probit estimation is applied to test the connection between the probability of leaving the panel and CFC.

The corresponding estimation identifies a significant effect of CFC-future (see Column (1) and (2) of Table 2.A7 in Appendix 2.A). Although this result joins the list of evidence on CFC's behavioral validity, it questions the representativeness of the analysis. However, this result has two important limitations. First, the marginal effect is relatively small: 1 sd in CFC-future decreases the average probability of 38.5 % by 1 %-point only. Second, the result is exclusively driven by individuals with extreme low forward-looking behavior. Excluding those individuals from the estimation sample who report a CFC-future smaller or equal to the sample's first percentile, results in smaller and insignificant coefficients (see Column (3) and (4) of Table 2.A7 in Appendix 2.A). Accordingly, the selection issues are restricted to outliers and do not threaten the results in general.

Separated items Lastly, the items of the CFC scale are considered separately to test whether the observed change in the CFC is caused by a general shift in survey responses or by single items only. For this purpose, the dependent variable of Model (2.3) is replaced by the change in the single items listed in Table 2.1. Figure 2.A4 (see Appendix 2.A) summarizes results for the effects of unemployment.

The estimations imply that only item numbers 1, 5 and 10 change significantly due to unemployment. Looking at the full CFC scale as proposed by Strathman *et al.* (1994) does not indicate a change within the CFC either (see again Figure 2.A4 in Appendix 2.A). Apparently, a forward-looking attitude does not change systematically. Only some statements are evaluated differently in response to a job loss. In line with the robustness tests from above, this points towards a survey issue as the primary reason for the observed effects: the salience of the latest events may cause a different rating of specific statements only. Being asked, for example, whether individuals '[...] think about how things can change in the future, and try to influence those things in [their] everyday life' (CFC item 1) may change only due to the latest experiences while being unemployed. If unemployment would truly change time preferences, all or at least most items within the CFC should change accordingly. Since this is obviously not the case, it seems questionable whether the results from Section 2.5.3 truly represent changes in the underlying preferences.

Discussion Instability within the CFC seems to originate to a great extent from survey noise and not from structural reasons. Although the analysis looks only at a short list of events and is limited by unobserved events which could affect the CFC more substantially, the empirical investigation points out that various events associated with a wide range of life domains come along with neither large nor persistent effects on a forward-looking attitude. Even unemployment, an event that affects individuals in numerous dimensions (see Chapter 3 and 4, or, e.g., Arulampalam *et al.*, 2001; Lucas *et al.*, 2004; Anger *et al.*, 2017), only changes the CFC to a small degree. Accordingly, this study would not reject the exogeneity assumption. Instead, framing and interview effects are potential reasons for the time-varying CFC.

2.5.5 Quantifying the error-in-variables bias

Although the results do not imply endogeneity, the instability within the CFC comes along with an error-in-variable bias which will affect any empirical investigation. The following section will test how strong this bias from instability can be. Herein, the analysis follows Cobb-Clark and Schurer (2013) and considers the 'classic' measurement bias, which refers to the following regression model:

$$Y_{it} = \alpha + \eta CFC_{it} + \varepsilon_{it},$$

with Y_{it} as any outcome variable, CFC_{it} as time preference proxy, and η as a marginal effect of interest. Typically, researchers cannot measure individuals' preferences at the same point in time the outcome of interest takes place. Usually, CFC_{it-k} approximates CFC_{it}. However, if the survey effects change the CFC across time, a measurement bias arises: $\mu_{it} = CFC_{it} - CFC_{it-k}$ with $Cov(\mu_{it}, Y_{it}) = 0$. Following Verbeek (2017), estimates of η are then biased by

$$plim(\hat{\eta}) = \eta\left(1 - \frac{Var(\mu_{it})}{Var(CFC_{it}) + Var(\mu_{it})}\right) = \eta\lambda.$$

λ is the attenuation factor; it equals 1 if $Var(\mu_{it}) = 0$ and approaches 0 with increasing $Var(\mu_{it})$. In addition to its effect on $\hat{\eta}$, λ also transfers itself to the corresponding standard error as well as to other coefficients in a multiple regression model.

Assuming that any variation in the CFC is a measurement error, this study can simulate λ. For this purpose, the study assumes that $k = 1$, implying a twelve-month pass between the measurement of time preferences and the considered outcome. This implies that CFC_{it} equals the true time preferences and is not affected by measurement issues at all: a strong, but necessary assumption. Table 2.4 presents the simulation results for CFC-future, CFC-immediate, and the full CFC scale for several socio-demographic subgroups. $\hat{\lambda}$ and its standard error result from bootstrapping with 200 repetitions.

Table 2.4 indicates that the attenuation bias of the CFC is substantial. Marginal effects of CFC-future or CFC-immediate could be underestimated by approximately 50 %. Applying the full CFC scale comes along with a smaller, yet significant error. Here, $\hat{\eta}$ will be 58 % of the true η. Between subgroups, the attenuation bias appears to be the strongest with low-income households and lower-educated individuals. Nevertheless, all groups

Table 2.4: *Simulated attenuation bias for CFC by different subgroups*

	Obs.	CFC-future $\hat{\lambda}$	CFC-future se	CFC-immediate $\hat{\lambda}$	CFC-immediate se	CFC $\hat{\lambda}$	CFC se
Full sample	10,997	0.508	0.005	0.508	0.005	0.581	0.004
By gender							
Women	5,132	0.511	0.007	0.498	0.007	0.576	0.006
Men	5,865	0.501	0.007	0.516	0.007	0.582	0.006
By income in $t-1$							
Income $\leq$ 14k	850	0.492	0.016	0.481	0.018	0.539	0.017
14k $<$ Income $\leq$ 40k	4,700	0.510	0.007	0.497	0.007	0.582	0.007
Income $> 40k$	5,447	0.508	0.007	0.522	0.007	0.586	0.007
By education							
Low	2,674	0.497	0.010	0.478	0.011	0.548	0.011
Mid	6,979	0.504	0.006	0.501	0.006	0.575	0.006
High	1,344	0.530	0.014	0.560	0.012	0.614	0.014
By age in $t-1$							
Age $\leq$ 35	2,255	0.492	0.007	0.498	0.009	0.565	0.004
35 $<$ Age $\leq$ 50	4,936	0.505	0.011	0.511	0.004	0.587	0.010
Age $>$ 50	4,427	0.515	0.003	0.514	0.006	0.582	0.006
By occupation in $t-1$							
Working	7,750	0.509	0.006	0.512	0.006	0.591	0.005
Unemployed	285	0.506	0.024	0.473	0.027	0.565	0.030
Non-working or other occupation	2,962	0.501	0.010	0.499	0.009	0.554	0.010

Source: DHS 1996-2017, own calculation.
Note: $\hat{\lambda} = 1 - [Var(j_t - j_{t-1})/(Var(j_t) + Var(j_t - j_{t-1}))]$. *First row denotes j.* $\hat{\lambda}$ *and standard errors (se) based on bootstrapping with 200 repetitions.*

are affected.[7] In comparison with other economic preferences or personality traits (see Table 2.A8 in Appendix 2.A) and in line with Table 2.3, the attenuation factor is slightly smaller within CFC-future and CFC-immediate.

2.6 Cross-validation with another survey approach

2.6.1 Measure, data, and empirical strategy

The findings on the CFC may be restricted to this specific measure and not representative for survey approaches on time preferences in general. CFC's wording, for example, may be specifically prone to survey issues, while other approaches for time preferences are not. Cross-checking the results with an alternative survey approach helps to allay these doubts.

[7] By assumption, the correlation between μ_{it} and CFC_t must equal zero within 'classical' measurement error. If this assumption is violated, the definition of λ changes to $1 - [(Var(\mu_{it}) + Cov(\mu_{it}, CFC_{it}))/(Var(CFC_{it}) + Var(\mu_{it}) + 2Cov(\mu_{it}, CFC_{it}))]$, which is also known as a 'non-classical' measurement error (Bound and Krueger, 1991). Applying this definition does not affect the simulations to a large extent.

Therefore, the following section introduces a second survey approach on time preferences to replicate the analysis from above and to test whether instability is an observation within the CFC only.

Two ultra-short survey items For this purpose, this study relies on two ultra-short survey items on patience and impulsiveness (USS, see Vischer *et al.*, 2013) which aim – similar to the CFC – to measure individuals' tastes for future rewards within surveys yet, with only two items:

> '*Would you describe yourself as an impatient or a patient person in general? Please answer on a scale from 0 to 10, where 0 means very impatient and 10 means very patient.*'
>
> '*How would you describe yourself: Do you generally think things over for a long time before acting – in other words, are you not impulsive at all? Or do you generally act without thinking things over for long, in other words, are you very impulsive? Please answer on a scale from 0 to 10, where 0 means not at all impulsive and 10 means very impulsive.*'

The German Socio-Economic Panel (SOEP, 2017) includes these survey items in 2008 and 2013. It thus allows the testing of their stability within a five-year interval.[8]

Although the DHS and SOEP originate from two different countries, the stability of the CFC and USS should be, nevertheless, very similar. Both measures are behaviorally valid.[9] Both countries are equally advanced with respect to GDP per capita, life expectancy, and the welfare state. Labor market or health-related events should thus affect individuals similarly. Moreover, the Netherlands and Germany are relatively similar with respect to their patience level (Falk *et al.*, 2018) and show equal age patterns in other economic preferences (Dohmen *et al.*, 2017).

Variables and empirical strategy In order to replicate the DHS analysis, the upcoming section follows the definitions and proceedings of Section 2.3 to a large extent. However,

[8] Analyzing USS's stability does not only function as validation of the results from above. Since the USS is a relatively new approach, evidence on its general validity is limited so far. By discussing the stability of USS measures and their relationship with the extensively validated CFC scale, the following analysis contributes to their validation process.

[9] Using a real-stake experiment, Vischer *et al.* (2013) show that answers to USS-patience correspond with individuals' time discounting parameter. Replicating the tests on CFC's behavioral validity (see Section 2.3.2) with the SOEP and its USS items identifies the theoretically expected correlations: reporting a high USS-impulsiveness corresponds with unhealthier behavior, lower education, and a higher preference towards immediate consumption. USS-patience correlates with less alcohol consumption and more savings. See Table 2.A9 in Appendix 2.A for the corresponding probit estimations.

some adjustments are inevitable due to differences between the DHS and the SOEP.[10] Due to its large sample size, the SOEP also allows this study to extend the list of events. Observation numbers are sufficient to look into relatively rare events, too, that is, negative family events, such as divorce or death of a family member, involuntary unemployment (job loss due to dismissal by the employer or due to plant closure), and finishing education.

To test for age effects, the following analysis can apply Model (2.1) from above, yet without cohort fixed effects and controls for economic growth due to multicollinearity. Since only one five-year difference in the USS is available, the analysis can rely on the following modification of Model (2.3):

$$\Delta Y_i = \alpha + \theta_{2009} D_{2009,i} + \theta_{2011} D_{2011,i} + \theta_{2013} D_{2013,i} + \theta_{2015} D_{2015,i} + \Delta \mathbf{X}_i' \gamma + \varepsilon_i. \quad (2.4)$$

As before, ΔY is the standardized within-change in USS-patience or USS-impulsiveness from 2008 to 2013; $\Delta\mathbf{X}$ represents, again, the first difference of a standard set of control variables. In contrast to Model (2.3), however, Equation (2.4) includes not one but multiple event dummies D_t with $t \in \{2009, 2011, 2013, 2015\}$ in order to account for temporary effects. D equals 1 if an individual reports the event of interest in t or $t-1$ and equals 0 otherwise. The corresponding θ_t coefficient then identifies whether individuals experiencing the event in one specific period change their time preferences on average.

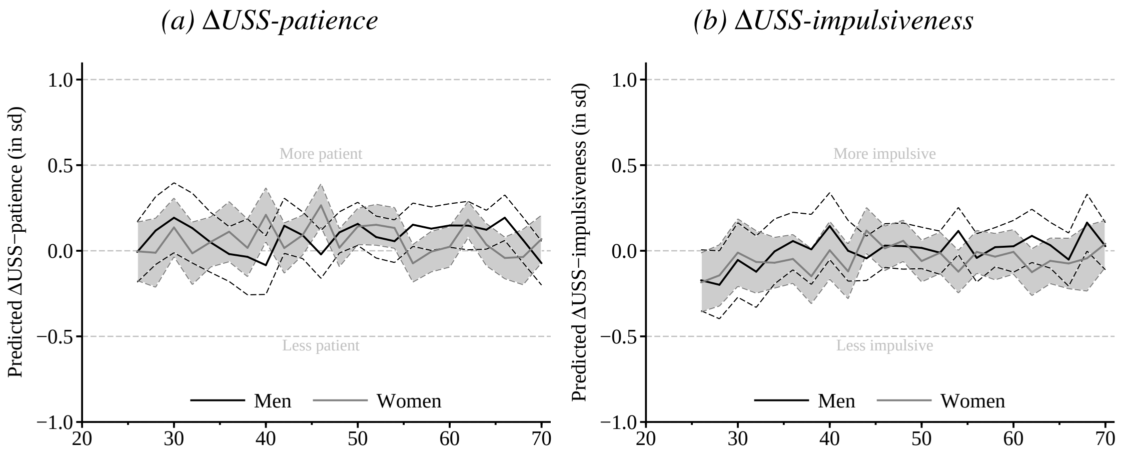
Individuals who changed their USS due to an event right before or immediately after the first interview in 2008 might leap back to their (unobserved) pre-event level until 2013. Accordingly, θ_{2009} will deviate from zero if effects are not persistent. θ_{2015} identifies anticipation effects. Individuals who are going to experience D after the second USS interview (in 2014 or 2015) may already have adjusted their USS in 2013.

2.6.2 Results on patience and impulsiveness

Level of instability With respect to the intra-individual correlations, the SOEP comes to similar conclusions as with the DHS. USS-patience has an intra-individual correlation within five years of 0.492. This is at a similar level of the general willingness to take risks scale (see Dohmen *et al.*, 2011) or locus of control ($corr = 0.477$ and $corr = 0.523$, respectively). USS-impulsiveness reveals a correlation of 0.408 only, implying a considerable level of instability. In accordance, its distribution of changes varies more than within patience (see Figure 2.A5 in Appendix 2.A). 98 % of the sample change their impulsiveness (patience) between ±2.72 sd (±2.60 sd) and 50 % between ±0.45 sd

[10] This concerns the definition of event dummies only. Unemployment is defined as a change in individuals' unemployment histories between two years. An income improvement (worsening) is identified as the change in household income between two consecutive survey interviews by more than 50 %. Health worsens if individuals start to report a severe illness, such as diabetes, cardiac disease, cancer, cardiovascular disease, migraine, or depression. A 'health improvement' is the end of these health conditions.

Figure 2.5: Five years change in USS by age (in sd)



Source: SOEP 2008, 2013, own calculations.
Note: X-axis denotes age. Dashed lines represent 95% confidence intervals based on clustered robust standard errors. No additional controls. 9,390 observations used.

($\pm$0.43 sd). In addition, changes in impulsiveness differ between genders. A Kolmogorov-Smirnov test implies that women reduce impulsiveness significantly more than men ($p =$ 0.038).

Stability of USS with respect to age Figure 2.5 presents the predicted change in the USS items by age categories separately for women and men. In summary, findings are similar to those on the CFC. Although the USS varies considerably, it is not dependent on age. Restricting the functional form of the age effect to a second-degree polynomial trend identifies a significant effect of age on impulsiveness for both women and men (see Table 2.A10 in the Appendix 2.A). However, the identified inverted u-shaped does not indicate strong marginal effects. Following the estimations, women increase impulsiveness, on average, by 0.01 sd within five years at the age of 45. Gender does not differ on a significant level.

Stability of the USS with respect to life events Table 2.5 presents the results concerning the effects of common life events on the USS measures. Panel A displays results on USS-patience and Panel B on USS-impulsiveness. In all cases, the dependent variable is measured in standard deviations.

The estimation does not indicate that unemployment affects the USS extensively (see Column (1) of Table 2.5), but, if the event of unemployment is limited to involuntary causes (e.g. dismissal by employer or due to plant closure), individuals report a significant reduction in patience when the job loss will occur in the future (see Column (2) of Table 2.5). Losing work thus does not affect patience in general; it must come along involuntarily to change time preferences.

Table 2.5: Regression results of events on ΔUSS

Considered event	(1) Un-employment	(2) Involuntary job loss	(3) Retirement	(4) Finish education	(5) Income improv.	(6) Income worsening	(7) Marriage	(8) Child birth	(9) Divorce	(10) Death in family	(11) Health improv.	(12) Health worsening
Panel A: Dependet variable Δ USS-patience												
Event reported												
in 2008 or 2009	0.017	-0.028	0.074	0.119	0.058	-0.001	-0.145	-0.112	0.246	0.090		
	(0.062)	(0.094)	(0.100)	(0.213)	(0.067)	(0.103)	(0.094)	(0.083)	(0.172)	(0.065)		
in 2010 or 2011	0.059	0.105	-0.068	-0.275	-0.065	-0.043	-0.095	-0.029	-0.231	-0.056	0.151*	-0.053
	(0.070)	(0.094)	(0.091)	(0.272)	(0.066)	(0.112)	(0.108)	(0.090)	(0.177)	(0.077)	(0.082)	(0.043)
in 2012 or 2013	-0.043	-0.040	0.022	0.166	0.062	0.096	-0.271***	0.028	0.116	-0.044	0.047	-0.152***
	(0.080)	(0.103)	(0.072)	(0.281)	(0.062)	(0.106)	(0.083)	(0.114)	(0.238)	(0.067)	(0.062)	(0.055)
in 2014 or 2015	-0.082	-0.241**	-0.013	-0.212	-0.077	-0.097	-0.015	-0.035	0.006	-0.094	-0.189***	0.049
	(0.079)	(0.101)	(0.077)	(0.241)	(0.067)	(0.106)	(0.120)	(0.106)	(0.120)	(0.067)	(0.061)	(0.057)
Observations	3,351	3,351	3,351	3,351	3,211	3,211	3,595	3,584	3,595	3,583	3,767	3,767
R^2 adj.	-0.000	0.001	-0.000	0.000	0.000	-0.000	0.002	0.001	0.000	0.001	0.004	0.003
Panel B: Dependet variable Δ USS-impulsiveness												
Event reported												
in 2008 or 2009	0.090	0.046	-0.023	-0.201	0.003	-0.135	0.132	-0.007	-0.014	0.042		
	(0.069)	(0.106)	(0.112)	(0.166)	(0.074)	(0.115)	(0.100)	(0.083)	(0.199)	(0.071)		
in 2010 or 2011	-0.136*	-0.066	0.005	-0.360	-0.050	-0.073	0.007	-0.028	0.186	-0.161**	0.015	-0.086*
	(0.077)	(0.114)	(0.100)	(0.256)	(0.072)	(0.108)	(0.101)	(0.103)	(0.194)	(0.078)	(0.086)	(0.047)
in 2012 or 2013	0.117	0.047	0.122	0.109	-0.042	0.026	0.171	-0.005	-0.098	0.012	0.065	-0.049
	(0.087)	(0.106)	(0.093)	(0.242)	(0.068)	(0.123)	(0.105)	(0.120)	(0.236)	(0.081)	(0.069)	(0.055)
in 2014 or 2015	-0.002	-0.179	0.002	-0.065	-0.003	0.003	-0.135	-0.079	0.038	0.110*	-0.062	0.026
	(0.083)	(0.114)	(0.096)	(0.298)	(0.076)	(0.104)	(0.123)	(0.101)	(0.172)	(0.064)	(0.068)	(0.060)
Observations	3,351	3,351	3,351	3,351	3,211	3,211	3,595	3,584	3,595	3,583	3,767	3,767
R^2 adj.	0.000	-0.001	-0.001	-0.000	-0.000	0.000	0.000	-0.001	-0.001	0.001	-0.000	0.000

Source: SOEP 2008, 2013.

Note: Robust standard errors in parentheses, $^{*}p<0.1$, $^{**}p<0.05$ and $^{***}p<0.01$. Dependent variable denoted by panel title, measured in sd. All estimations include the following controls: Changes in employment state, marital status, number of children and income level. Single controls excluded if they correspond with the event of interest. Health measures available from 2010 onwards only.

In line with the findings on the CFC, individuals anticipate the loss of employment. However, since all other coefficients of interest do not differ from zero, the effects are not persistent. However, the reduction in patience stands in contrast to the findings on CFC-future. Here, forward-looking attitudes increased due to unemployment.

Retirement or finishing education changes neither patience nor impulsiveness (see Column (3) and (4)). Additionally, experiencing an increase or decrease in income by more than 50 % within one year does not change the USS either. The same applies for a childbirth or divorce. Similar to the CFC, marriage affects USS-patience – even though it is only temporarily. In cases where individuals have married right before the second interview, patience reduces, on average, by 0.27 sd. Since other corresponding coefficients are not significantly different from zero, the effect cannot be considered as persistent. Marriage thus comes literally along with a 'honeymoon effect' on time preferences. While marriage affects CFC-future positively, implying a stronger forward-orientation, marriage reduces patience, which implies a loss in time discounting. Similar to involuntary job losses, effects are thus contradictory.

Bad health affects patience negatively. Individuals who experience a severe illness right before the second interview tend to being less patient. In accordance, individuals who are going to experience a health improvement in the future (after the second interview) and thus, most likely experience a health worsening in the period before the second interview also report to be less patient. The results are therefore in line with findings on CFC-future.

Discussion In summary, the USS replicates the findings on CFC to a large extent. The level of stability is very similar, and the corresponding instability is only weakly related to common life events. Neither age nor several positive or negative life events shape USS measures in the long run. If significant changes are observable, they are only temporarily restricted and small.

However, there are also contradictions. Unemployment and marriage reduce USS-patience while they increase CFC-future. Since both measures correspond with individuals' discount factors, implying behavioral validity, a contradiction arises: how can one indicator of time discounting increase while another decreases?

A potential explanation is that CFC-future and USS-patience correspond with different motives of time preferences. While patience and impulsiveness are very distinct factors for forward-looking behavior, the CFC questionnaire subsumes different motives such as laziness, goal-orientation, or problem-awareness (see again Table 2.1). In line with this reasoning, the correlation between CFC-future and USS-patience is relatively small (see Appendix 2.B). The controversial effects of unemployment and marriage on the CFC and USS would thus simply reflect their different scope. Nevertheless, if forward-looking behavior truly changes, its underlying motives should change consistently with it. This is obviously not the case.

Section 2.5.4 already discussed that only specific items of the CFC change due to a job loss. It, therefore, concluded that changes in the CFC reflect survey issues rather than endogenous preferences, i.e. individuals only perceive themselves as more forward-looking. Changes in the USS may originate from similar issues. Individuals may perceive themselves as less patient after they lost employment, although their actual preference does not change.

2.7 Conclusion

The DHS and the consideration of future consequences scale, a behavioral validated survey instrument for time preferences, allow this study to test the assumption of exogenous and time-invariant time preferences. The results are twofold. On the one hand, time preferences vary substantially within individuals. Compared to other economic preferences or personality traits, intra-individual correlations are relatively low. On the other hand, the corresponding instability is only weakly related to aging or life experience. Past life events have only a small and temporarily restricted impact on time preferences. In addition, further tests suggest that changes are likely a consequence of survey issues and not structural changes in forward-looking attitudes. Cross-checking the results with two ultra-short survey items validates these results.

The present study can, therefore, make several, important contributions to the literature. First, it shows that the stated preference approach inhabits a similar intra-individual (in-)stability as the experimental methods to elicit forward-looking behavior (see Meier and Sprenger, 2015). From this perspective, none of the two approaches seem to be inferior. Moreover, the results imply that time preferences are very similar to personality traits with respect to their stability. Following Hamaker *et al.* (2007) and Borghans *et al.* (2008), measured personality includes a time-invariant and a situational component. Accordingly, as soon as the situation changes, revealed personality changes, too. This study provides evidence for a similar pattern within time preferences.

Furthermore, this study complements previous findings concerning events that shape time preferences (see Krupka and Stephens, 2013). It not only replicates findings from the laboratory, but it also introduces evidence on anticipation as well as persistence and is, thereby, the first study to present the full picture of time-varying time preferences. In addition, the empirical investigation tested theoretical proposals on endogenous time preferences formation, dynasty discounting, and the retirement-consumption puzzle; yet, none of the corresponding events affected time discounting persistently.

This study has important implications for theory and empirics. In relation to the high level of time preferences' instability, the effects of age and common life events are negligible. In the absence of any other observable reasons for instability, results indicate that the survey approach on time preferences is – similar to the experimental approach (Meier and Sprenger, 2015) – subject to survey noise. This study does thus not falsify the assumption of exogenous time preferences, but, empirical studies using time preferences as an explanatory variable may suffer from an attenuation bias. Future empirical studies must thus be cautious about their identification strategy and apply corresponding countermeasures such as instrumental variables or averages of time preferences across multiple time periods.

2.A Appendix: supplementary tables and figures

Table 2.A1: Summary statistics and factor loadings of CFC

	Scale 1-7: Share									Loadings	
	1	2	3	4	5	6	7	Mean	Sd	Factor 1	Factor 2
Item 1	0.10	0.18	0.19	0.23	0.18	0.09	0.03	4.16	1.50	0.64	-0.39
Item 2	0.10	0.18	0.19	0.23	0.18	0.09	0.03	3.60	1.56	0.70	-0.32
Item 3*	0.08	0.17	0.20	0.23	0.19	0.10	0.03	3.70	1.52	0.69	0.27
Item 4*	0.09	0.18	0.18	0.25	0.18	0.09	0.03	3.64	1.55	0.22	0.56
Item 5*	0.03	0.06	0.11	0.30	0.29	0.16	0.05	4.42	1.34	0.02	0.61
Item 6	0.10	0.16	0.20	0.29	0.17	0.07	0.02	3.56	1.46	0.45	-0.39
Item 7	0.03	0.03	0.06	0.22	0.31	0.25	0.10	4.88	1.38	0.48	-0.40
Item 8	0.04	0.07	0.13	0.37	0.24	0.12	0.03	4.18	1.33	0.54	-0.43
Item 9*	0.09	0.22	0.24	0.26	0.13	0.04	0.01	3.29	1.36	0.54	0.40
Item 10*	0.06	0.13	0.21	0.30	0.19	0.09	0.02	3.79	1.40	0.61	0.40
Item 11*	0.07	0.15	0.21	0.27	0.18	0.09	0.02	3.71	1.45	0.66	0.43

Source: DHS 1996-2017, own calculations.
*Note: * indicates those items which are reversed prior to the factor analysis. Scale is labeled from 1 'extremely uncharacteristic' to 7 'extremely characteristic'. Item numbers correspond with Table 2.1. Factor loadings based on principal-component factor. 29,714 observations used.*

Table 2.A2: Means and shares by cross section and longitudinal samples

	Cross-	Longitudinal sample with		
	section	Δ 1 year	Δ 3 years	Δ 5 years
CFC-future	0.00	0.02	0.00	0.01
	(0.98)	(0.96)	(0.93)	(0.93)
CFC-immediate	-0.01	0.00	0.01	0.02
	(0.99)	(0.98)	(0.99)	(1.01)
Age (in years)	45.39	46.06	46.06	46.81
	(11.22)	(11.01)	(10.59)	(10.09)
Height (in cm)	175.32	175.38	175.43	175.53
	(9.11)	(9.12)	(9.17)	(9.14)
Annual net income (cat. 1-6)	4.28	4.31	4.44	4.35
	(1.21)	(1.17)	(1.17)	(1.16)
Gender (shares sum to 1)				
Men	0.51	0.53	0.56	0.58
Women	0.49	0.47	0.44	0.42
Education level (shares sum to 1)				
Low level	0.23	0.24	0.27	0.27
Mid level	0.64	0.63	0.61	0.62
High level	0.12	0.12	0.12	0.11
Occupation (shares sum to 1)				
Working	0.72	0.70	0.70	0.70
Unemployed	0.02	0.03	0.02	0.02
Not working	0.18	0.19	0.19	0.19
Other occupation	0.02	0.02	0.02	0.02
Disabled	0.06	0.07	0.07	0.07
Observations	21,787	10,997	5,716	3,629

Source: DHS 1996-2017, own calculations.
Note: Standard deviation in parentheses. Means and shares from the reference level, i.e. in $t-i$ with $i \in \{1,3,5\}$. Income measured in the following six categories: ≤10k, ≤14k, ≤22k, ≤40k, ≤75k and >75k.

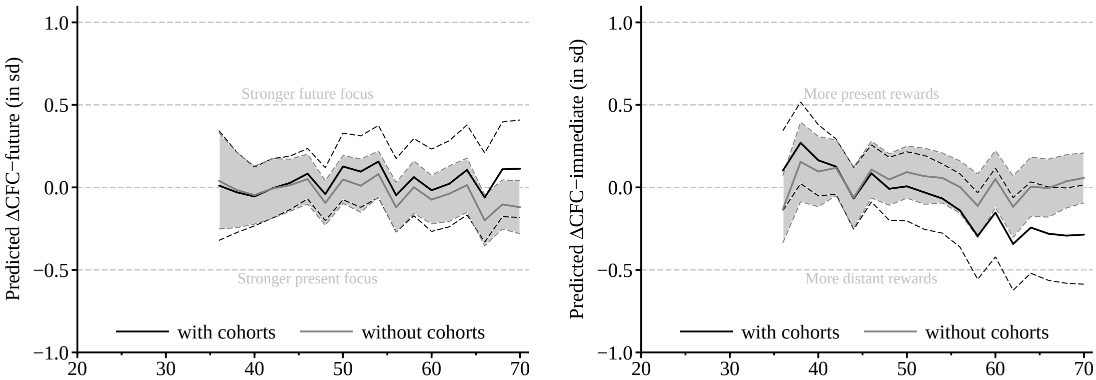
Table 2.A3: Predicted marginal effects of CFC factors on revealed behavior

Binary outcome	(1) High BMI	(2) Smoker	(3) Reg. alcohol	(4) College degree	(5) 5 years plan?	(6) Savings?
CFC-future	-0.010**	-0.015***	0.002	0.026***	0.050***	0.025***
	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.004)
CFC-immediate	0.011**	0.016***	0.007***	-0.042***	-0.052***	-0.031***
	(0.005)	(0.005)	(0.003)	(0.004)	(0.003)	(0.004)
Average Probability	0.251	0.267	0.064	0.125	0.172	0.747
Controls	yes	yes	yes	yes	yes	yes
Observations	19,261	19,261	19,261	19,261	19,261	19,261
Pseudo R^2	0.033	0.034	0.080	0.109	0.059	0.069

Source: DHS 1996-2017, own calculations.
Note: Clustered robust standard errors in parentheses, *p<0.1, **p<0.05 and ***p<0.01. Pseudo R^2 based on corresponding probit estimation. Estimation uses all observations of the baseline sample (n = 29,714) reporting all control variables and all considered outcome variables. Controls are age, age squared, gender, height, main occupation, level of income and education. 'High BMI' equals 1 if individual's BMI is greater than sample's 75th percentile (BMI > 27.5). 'Smoker' equals 1 if individuals smoke at all. 'Alcohol' equals 1 if individuals report more than 4 alcoholic beverages a day. 'Smoker' equals 1 if individual has a college degree. Corresponding estimation does not control for level of education. '5 years plan?' equals 1 if individuals consider 5 or more years as 'most important' time span for their financial decisions. 'Saving?' equals 1 if individual put any money aside in the last twelve month.

Figure 2.A1: Five years change in CFC factors by age with and without cohort fixed effects

(a) ΔCFC-future: 1 year *(b) ΔCFC-immediate: 1 year*



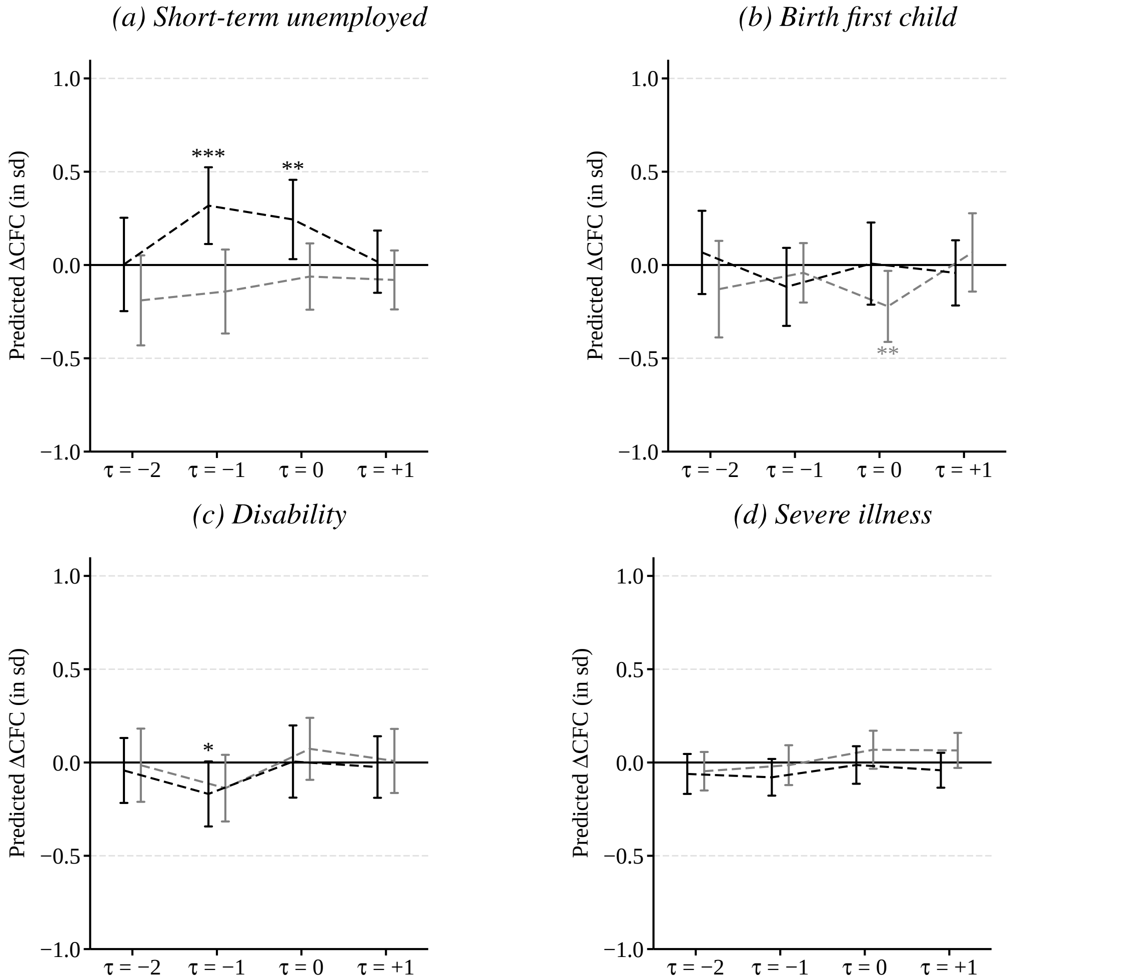
Source: DHS 1996-2017, own calculations.
Note: X-axis denotes age. Dashed lines represent 95% confidence intervals based on clustered robust standard errors. Economic growth included as control variable. Men only. 4,392 observations used.

Table 2.A4: Regression results of age, cohort and economic growth on ΔCFC

Time difference	Δ 1 year		Δ 3 years		Δ 5 years	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ΔCFC-future as dependent						
Men × Age (β_1)	-0.005	-0.003	0.022***	0.024*	0.016	0.039*
	(0.004)	(0.005)	(0.008)	(0.014)	(0.014)	(0.022)
Men × Age × Age (β_2, in 1000)	0.038	0.040	-0.243***	-0.269*	-0.198	-0.366*
	(0.041)	(0.055)	(0.084)	(0.143)	(0.130)	(0.206)
Women × Age (β_3)	-0.007*	-0.005	0.023**	0.025*	0.016	0.039*
	(0.004)	(0.006)	(0.009)	(0.015)	(0.014)	(0.022)
Women × Age × Age (β_4, in 1000)	0.076*	0.078	-0.269***	-0.298**	-0.211	-0.378*
	(0.045)	(0.057)	(0.095)	(0.149)	(0.145)	(0.214)
GDP per capita growth (in percent)		0.001		0.023***		0.030***
		(0.003)		(0.003)		(0.004)
Constant	0.139	0.051	-0.459**	-0.496	-0.293	-1.016*
	(0.095)	(0.136)	(0.209)	(0.354)	(0.346)	(0.563)
Cohort FE		yes		yes		yes
p-value(F-test): $\beta_1 = \beta_3$	0.047	0.044	0.732	0.652	0.947	0.969
p-value(F-test): $\beta_2 = \beta_4$	0.085	0.086	0.544	0.487	0.838	0.842
Observations	14,028	14,028	7,034	7,034	4,392	4,392
Adj. R^2	0.000	0.000	0.002	0.011	0.003	0.020
Panel B: ΔCFC-immediate as dependent						
Men × Age (β_1)	-0.005	-0.008	0.006	0.022	-0.007	-0.027
	(0.004)	(0.006)	(0.008)	(0.014)	(0.014)	(0.023)
Men × Age × Age (β_2, in 1000)	0.039	0.062	-0.073	-0.303**	0.055	0.113
	(0.042)	(0.057)	(0.083)	(0.145)	(0.135)	(0.220)
Women × Age (β_3)	-0.005	-0.007	0.008	0.024	-0.006	-0.025
	(0.004)	(0.006)	(0.009)	(0.015)	(0.015)	(0.024)
Women × Age × Age (β_4, in 1000)	0.032	0.057	-0.125	-0.355**	0.021	0.072
	(0.046)	(0.059)	(0.096)	(0.151)	(0.153)	(0.231)
GDP per capita growth (in percent)		0.003		0.004		-0.009**
		(0.003)		(0.003)		(0.004)
Constant	0.162*	0.225*	-0.042	-0.328	0.262	1.024*
	(0.097)	(0.137)	(0.210)	(0.361)	(0.363)	(0.593)
Cohort FE		yes		yes		yes
p-value(F-test): $\beta_1 = \beta_3$	0.743	0.778	0.399	0.398	0.637	0.545
p-value(F-test): $\beta_2 = \beta_4$	0.755	0.807	0.235	0.239	0.591	0.512
Observations	14,028	14,028	7,034	7,034	4,392	4,392
Adj. R^2	0.000	-0.000	0.001	0.002	-0.000	0.003

Source: DHS 1996-2017, own calculation.
*Note: Clustered robust standard errors in parentheses, *p<0.1, **p<0.05 and ***p<0.01. Effects measured measured in standard deviation (sd). Cohorts in ten year categories starting at 1930. Growth in GDP per capita with respect to previous year, centered to sample mean.*

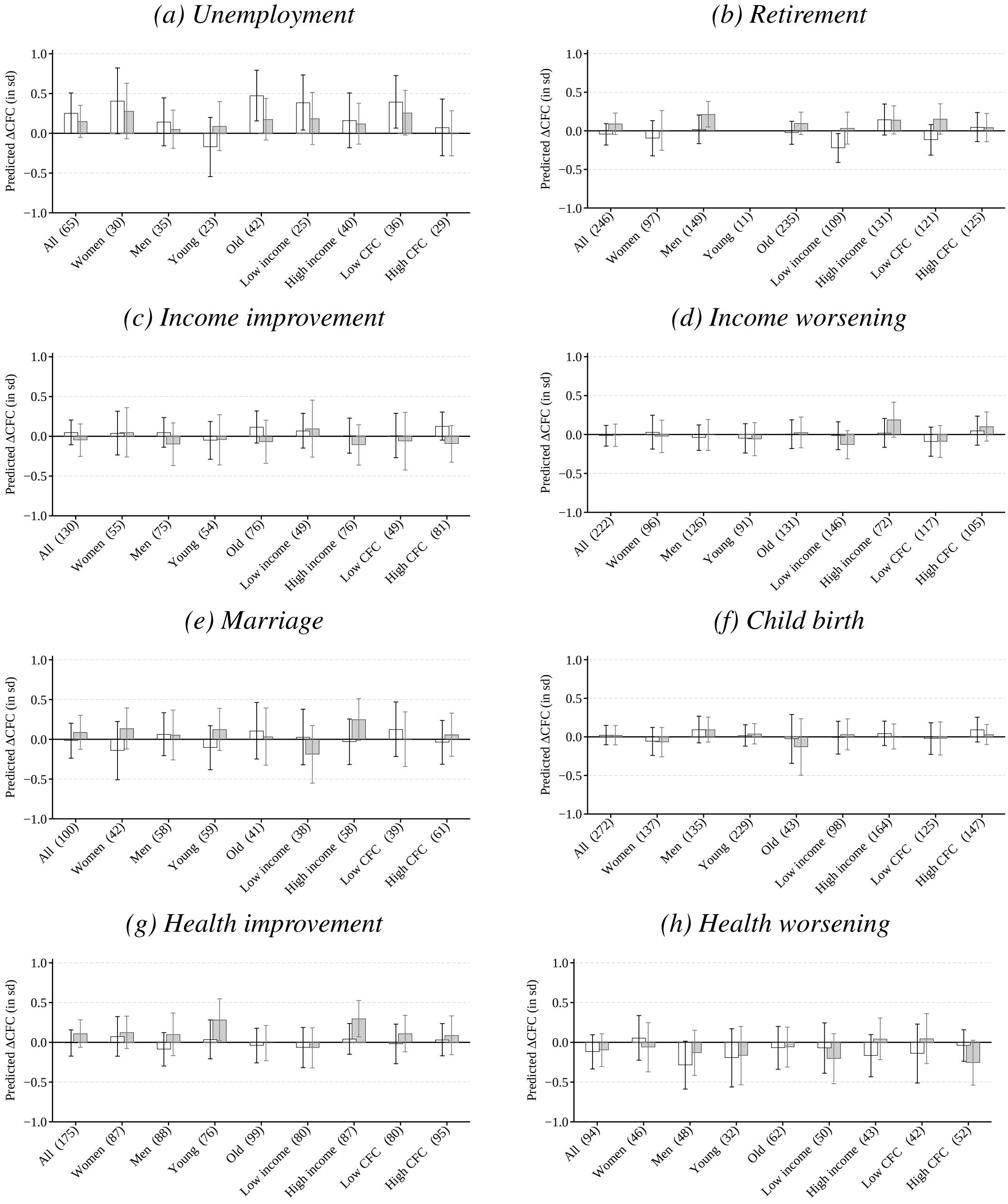
Figure 2.A2: Predicted lagged and lead effects of alternative life events on CFC factors



Source: DHS 1996-2017, own calculation.

*Note: Black (gray) line shows predicted changes in CFC-future (CFC-immediate). X-axis denotes lead and lags of the event. Whiskers represent 95 % confidence intervals based on clustered robust standard errors, *p<0.1, **p<0.05 and ***p<0.01. Control variables are employment and marital status, economic growth, income level, number of children, year and region fixed effects. 'Short-term unemployed' equals 1 if individuals report unemployment or short-term unemployment benefits in the previous year. 'Disability' equals 1 if individuals start to report 'disabled' as main occupation. 'Severe illness' indicates whether individuals suffer from a long-term illness, disorder, handicap or consequences of an accident.*

Figure 2.A3: Predicted effects of life events on CFC within two years by subgroups



Source: DHS 1996-2017, own calculations.

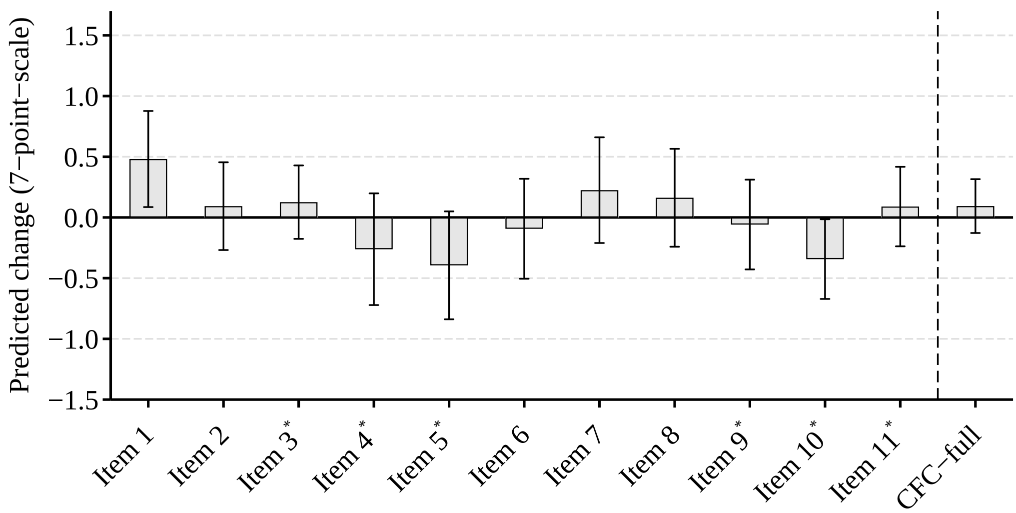
Note: Black (gray) bars represent predicted changes in CFC-future (CFC-immediate). X-axis denotes considered subgroup. Whiskers represent 95 % confidence intervals based on clustered robust standard errors. Number of observations reporting the event in parantheses. Control variables are employment and marital status, economic growth, income level and number of children. See Table 2.2 for the definition of the events. Individuals are considered as young if they are younger than sample mean (45 years). 'Low (high) income' denotes individuals reporting an income smaller (greater) than 40,000 € in $t-2$. 'Low (high) CFC' includes individuals reporting a level of the full CFC scale below the sample mean in $t-2$ (0.03) only.

Table 2.A5: Observation numbers of lag and leads and sample sizes

	Obs. in $\tau =$				Sample		
	-2	-1	0	+1	n	i	$\bar{n}$
Labor market							
Unemployment	45	58	60	61	7,007	1,452	4.8
Retirement	229	246	256	260	7,007	1,452	4.8
Income							
Income improvement	88	84	80	78	4,816	1,040	4.6
Income worsening	143	153	149	144	4,816	1,040	4.6
Family							
Marriage	62	52	67	79	5,282	1,116	4.7
Child birth	159	189	171	185	7,021	1,456	4.8
Grandchildren birth	51	45	49	51	6,352	1,359	4.7
Health							
Health improvement	119	134	128	167	6,382	1,420	4.5
Health worsening	71	65	61	48	6,382	1,420	4.5

Source: DHS 1996-2017, own calculations.
Note: See Table 2.2 for definition of considered events. n, i and $\bar{n}$ denote the number of all observations, individuals and average observations per individual, respectively.

Figure 2.A4: Predicted effects of unemployment on CFC items



Source: DHS 1996-2017, own calculations.
*Note: Bars represent predicted changes in CFC items from $t = -2$ to $t = 0$ on a 7-point-likert-scale when reporting occupational change into unemployment in $t = 0$. Item numbers correspond to numbering in Table 2.1. 'CFC-full' indicates the effect of unemployment on the the CFC score as proposed by Strathman et al. (1994, one factor including all items). Effect measured in standard deviation (sd). Whiskers represent 95 % confidence intervals based on clustered robust standard errors. * denotes items reversed prior to the analysis such that higher agreement indicates stronger forward-looking preference. Control variables are employment and marital status, economic growth, income level, number of children, year and region fixed effects.*

Table 2.A6: Fixed effect estimation of emotions and interview day on CFC factors

Dependent variable	CFC-patience		CFC-immediate	
	(1)	(2)	(3)	(4)
Panel A: Frequency of emotions				
Anxious	0.001	-0.002	0.037*	0.038*
	(0.023)	(0.023)	(0.022)	(0.022)
Sad	0.049*	0.047*	-0.011	-0.011
	(0.025)	(0.025)	(0.026)	(0.026)
Depressed, gloomy	-0.010	-0.009	-0.011	-0.011
	(0.025)	(0.025)	(0.027)	(0.027)
Happy	0.032	0.031	0.021	0.022
	(0.023)	(0.023)	(0.023)	(0.023)
Constant	-0.186	-29.687**	-0.186	21.055
	(0.136)	(14.226)	(0.134)	(13.938)
Controls		yes		yes
Observations	4625	4625	4625	4625
R^2 between	0.008	0.000	0.001	0.001
R^2 within	0.002	0.013	0.001	0.008
R^2 overall	0.006	0.000	0.000	0.001
Panel B: Day of interview				
Interview on weekdays	-0.099**	-0.079**	-0.016	-0.039
	(0.039)	(0.040)	(0.043)	(0.044)
Constant	0.061***	12.699***	-0.026**	-18.287***
	(0.010)	(3.651)	(0.011)	(4.298)
Controls		yes		yes
Observations	3887	3887	3887	3887
R^2 between	0.000	0.000	0.000	0.001
R^2 within	0.003	0.022	0.000	0.023
R^2 overall	0.000	0.001	0.000	0.000

Source: DHS 2014-2017 in Panel A, DHS 1997-1999 in Panel B, own calculations.
*Note: Robust standard errors in parentheses, *p<0.1, **p<0.05 and ***p<0.01. Controls are employment status, marital status, income, number of children in household, year and region fixed effect. Emotions in Panel A are measured on a six-time scale ranging from 'never' to 'continuously'. Interview on weekdays is a dummy equaling 1 if the participant answered the CFC questionnaire between Monday and Thursday, 0 otherwise.*

Table 2.A7: Predicted marginal effects of CFC factors on panel attrition

	Any CFC-future in t		CFC-future in $t > P(1)$	
	(1)	(2)	(3)	(4)
CFC-future	-0.009**	-0.010***	-0.005	-0.006
	(0.004)	(0.004)	(0.004)	(0.004)
CFC-immediate	-0.003	0.001	-0.001	0.004
	(0.004)	(0.004)	(0.004)	(0.004)
Average probability	0.385	0.385	0.384	0.384
Year FE	yes	yes	yes	yes
Controls		yes		yes
Observations	15384	15384	15240	15240
Pseudo R^2	0.093	0.114	0.093	0.114

Source: DHS 1996-2017, own calculations.
*Note: Clustered robust standard errors in parentheses, *p<0.1, **p<0.05 and ***p<0.01. Pseudo R^2 based on corresponding probit estimation. Dependent variable equals 1 if individual leaves the panel in $t+1$, 0 otherwise. Estimation uses all observations of the baseline sample (n = 29,714) reporting all control variables. Controls are age, age squared, gender, height, main occupation, level of income and education.*

Table 2.A8: Simulated attenuation bias for risk attitude, locus of control and Big Five

	Obs.	$\hat{\lambda}$	se
Risk attitude	25,046	0.612	0.003
Locus of control	2,798	0.514	0.010
Big Five			
Openness	2,713	0.632	0.010
Conscientiousness	2,713	0.556	0.010
Extraversion	2,713	0.624	0.011
Agreeableness	2,713	0.591	0.010
Neuroticism	2,713	0.662	0.009

Source: DHS 1996-2017, own calculation.
Note: $\hat{\lambda} = 1 - [Var(j_t - j_{t-1})/(Var(j_t) + Var(j_t - j_{t-1}))]$. First row denotes j. $\hat{\lambda}$ and standard errors (se) based on bootstrapping with 200 repetitions.

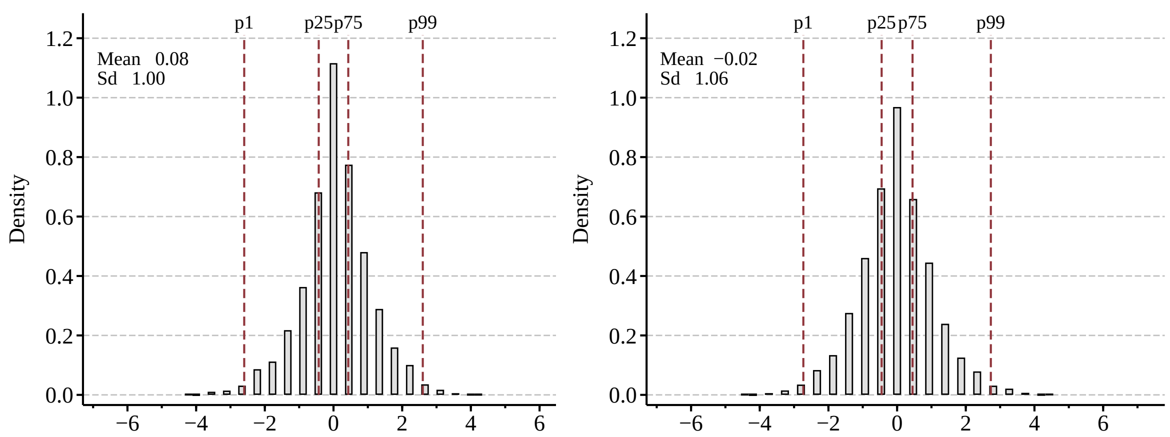
Table 2.A9: *Predicted marginal effects of USS items on revealed behavior*

Binary outcome	(1) High BMI BMI	(2) Smoker	(3) Reg. alcohol	(4) College degree	(5) Renew furniture?	(6) Savings?
USS-patience	-0.001	0.002	-0.006***	-0.005***	0.000	0.004*
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
USS-impulsiveness	0.005***	0.015***	0.004**	-0.007***	0.009***	-0.010***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Average Probability	0.251	0.299	0.192	0.192	0.431	0.649
Controls	yes	yes	yes	yes	yes	yes
Observations	13539	13539	13539	13539	8165	8165
Pseudo R^2	0.040	0.058	0.086	0.046	0.046	0.167

Source: SOEP 2008, 2013, own calculations.
Note: Robust standard errors in parentheses, $^{*}p<0.1$, $^{**}p<0.05$ and $^{***}p<0.01$. Pseudo R^2 based on corresponding probit estimation. Controls are age, age squared, gender, height, main occupation, level of income and education. Column (1) to (4) uses data from SOEP 2008 only. Column (5) and (6) rely on household heads in SOEP wave 2013 only. 'High BMI' equals 1 if individuals BMI is greater than sample's 75th percentile (BMI > 28.1). 'Smoker' equals 1 if individuals smoke. 'Alcohol' equals 1 if individuals drink one type of alcoholic beverage regularly. 'College degree' equals 1 if individuals hold a college degree. Corresponding estimation does not control for level of education. 'Renew furniture?' equals 1 if household head would replace furniture that is worn out but still usable. 'Saving?' equal 1 if households can and do put any money aside (for emergencies) at the end of each month.

Figure 2.A5: *Five years difference in USS (in sd)*

(a) ΔUSS-patience: 5 years *(b) ΔUSS-impulsiveness: 5 years*



Source: SOEP 2008, 2013.
Note: X-axis denotes changes in USS items (measured in sd). 9,390 observations used. p1, p25, p75 and p99 denote the percentiles of the corresponding distribution.

Table 2.A10: Regression results of age on ΔUSS

	(1)		(2)	
Dependent variable	ΔUSS-patience		ΔUSS-impulsiveness	
Men × Age (β_1)	0.006	(0.006)	0.019***	(0.006)
Men × Age × Age (β_2, in 1000)	-0.056	(0.066)	-0.165**	(0.069)
Women × Age (β_3)	0.008	(0.006)	0.018***	(0.006)
Women × Age × Age (β_4, in 1000)	-0.102	(0.066)	-0.173**	(0.069)
Constant	-0.065	(0.139)	-0.494***	(0.144)
p-value(F-test): $\beta_1 = \beta_3$	0.344		0.759	
p-value(F-test): $\beta_2 = \beta_4$	0.236		0.841	
Observations	9,390		9,390	
Adj. R^2	0.000		0.002	

Source: SOEP 2008, 2013, own calculations.
*Note: Clustered robust standard errors in parentheses, *p<0.1, **p<0.05 and ***p<0.01. Effects measured in standard deviation (sd).*

2.B Appendix: relation between CFC and USS

As the USS is a relatively new approach to elicit time preferences, there are no studies yet relating the CFC and USS to each other. To fill this gap, this study conducted a survey experiment with 408 students. In October 2018, attendants of the lecture 'Introduction to Economics' at Freie Universität Berlin answered a questionnaire on the USS and CFC as well as several general attitudes and socio-demographic characteristics.

The survey applied the online survey tool 'SoSciSurvey' (see www.soscisurvey.de). Students choose between an online questionnaire, which was optimized for smart-phones but also completable by standard computers or on paper. In the online survey, all questions – except for weight and height – were required. If they were not answered, the survey program did not allow participants to pass through to the next question. 35 of 408 students chose the paper survey. Only 3 % of all participants either made inconclusive answers (paper type only) or did not finish the survey. Before access to the survey was granted, the experimenter told all attendants that the survey aims for the evaluation of different measures on personality traits and will be analyzed anonymously. Right at the start of the survey itself, information on purpose and confidentiality were presented again.

Table 2.B1 summarizes all questions in line with their ordering in the survey. To prevent a focusing illusion, the survey started right away with the USS questions, asked for general individual information and, in the end, for the CFC. 'Introduction to Economics' is compulsory for undergraduate business and economics students and recommended for the first semester. Accordingly, the average age and semester are relatively low. In addition to students' background information, the survey asked for several health and consumptions indicators to evaluate the behavioral validity of both time preference measures. Here, the same items as in Section 2.3.2 have been chosen.

Panel A in Table 2.B2 shows the results of the CFC factor analysis within the survey data. Loadings in Column (1) and (2) correspond with the previous literature and the results from the DHS (see Table 2.A1). The internal validity of CFC thus holds in this survey, too.

Additionally, on this matter, findings on CFC's behavioral validity are replicated. Table 2.B3 presents the corresponding probit estimations. Here, the effect of the CFC (see Panel A) and USS (see Panel B) on students' revealed behavior is estimated. Although CFC factors correspond only weakly with health-related behavior (see Columns (1) to (3) in Panel A), it correlates very strongly with concerns on the future as identified by Strathman *et al.* (1994) (see Columns (4) and (5)). Similarly, Panel B implies behavioral validity of the USS. Both USS items correlate with all three considered health decisions and the scale on consumption preferences. This consensus between stated preferences and revealed behavior points out the behavioral validity of the CFC and USS in the survey.

Despite the internal and behavioral validity of the CFC and USS, the survey finds a weak correlation between both considered measures. Panel B of Table 2.B2 indicates that USS-patience only weakly correlates with CFC-future, although both are associated with forward-looking behavior and time discounting. The highest correlation is observed between CFC-immediate and USS-impulsiveness, which is, nevertheless, also relatively small.

An additional test, a factor analysis, includes all eleven CFC-items and the USS questions. As presented in Columns (3) and (4) of Panel A in Table 2.B2, USS-patience and USS-impulsiveness do not load on one of the CFC factors, implying orthogonality. Estimations on revealed behavior including all four measures for time preferences simultaneously does not change the results from separated estimations (see Panel C in Table 2.B3). This does also point out that CFC and USS are orthogonal.

In conclusion, the USS measures different aspects of time-dependent decision-making than the CFC. Partially, this is in line with previous findings since no unidimensional trait on time preferences has been identified yet (e.g. Daly *et al.*, 2009). Frederick *et al.* (2002) suspect that four traits are needed to explain time preferences to a full extent. CFC-future and USS-patience, which are both related with time discounting choices, may thus resemble two distinct traits within these considerations. Furthermore, self-control and present bias, which correlate with USS-immediate and USS-impulsiveness, respectively, do not typically interact with each other (e.g. Delaney and Lades, 2017). Accordingly, the USS and CFC approach time preferences from different perspectives.

Table 2.B1: Summary statistics of survey experiment (sorted by appearance)

	(1) Scale	(2) Mean / share	(3) Sd
USS-patience	0-10	5.26	2.24
USS-impulsiveness	0-10	4.51	2.18
Female	0/1	0.51	
Age	in years	19.86	2.72
Studies (sum = 1)			
Business	0/1	0.57	
Economics	0/1	0.33	
Other	0/1	0.10	
Semester		1.36	1.32
Consume or save? [1]	1-7	4.53	1.50
Do you smoke?	0/1	0.21	
How often do you drink the following beverages? [2]			
Beer	1-4	2.38	1.06
Wine	1-4	2.15	0.94
Spirits	1-4	1.93	0.86
Long drinks	1-4	2.12	0.87
How concerned are you about the following issues? [3]			
Environmental protection	1-3	2.44	
Climate change	1-3	2.53	
Own economic situation	1-3	2.54	
The economy in general	1-3	2.49	
Height	in cm	175.46	9.87
Weight	in kg	67.81	13.42
CFC items			
Item 1	1-7	4.98	1.34
Item 2	1-7	4.22	1.54
Item 3	1-7	4.80	1.49
Item 4	1-7	4.84	1.46
Item 5	1-7	3.36	1.30
Item 6	1-7	4.98	1.34
Item 7	1-7	5.44	1.27
Item 8	1-7	4.72	1.39
Item 9	1-7	5.32	1.34
Item 10	1-7	5.04	1.39
Item 11	1-7	4.94	1.43

Source: Student survey, own calculations.
Note: 396 observations. CFC and USS correspond to DHS and SOEP questionnaire. CFC items correspond to Table 2.1 (see Section 4.3). [1] *scale has 7 points ranging from 1 'I like to spend all my money immediately' to 7 'I want to save as much as possible'.* [2] *Scale is 1 'never', 2 'rarely', 3 'now and then' and 4 'regularly'.* [3] *scale is 1 'not concerned at all', 2 'somewhat concerned' and 3 'very concerned'.*

Table 2.B2: Factor loadings

	(1) CFC-future	(2) CFC-immediate	(3) CFC-future	(4) CFC-immediate
Panel A: Factor loadings				
CFC items				
Item 1	0.51	-0.49	0.51	-0.48
Item 2	0.56	-0.45	0.56	-0.46
Item 3*	0.66	-0.06	0.66	-0.06
Item 4*	0.27	0.29	0.27	0.28
Item 5*	0.17	0.48	0.17	0.48
Item 6	0.54	-0.30	0.53	-0.31
Item 7	0.51	-0.06	0.51	-0.06
Item 8	0.48	-0.42	0.47	-0.41
Item 9*	0.64	0.40	0.64	0.40
Item 10*	0.62	0.48	0.62	0.48
Item 11*	0.70	0.31	0.70	0.31
USS-patience			0.10	-0.06
USS-impulsiveness			-0.19	-0.07
Panel B: Correlation matrix				
USS-patience	0.06	-0.03		
USS-impulsiveness	-0.07	0.11		

Source: Student survey, own calculations.
*Note: 396 observations. Panel A shows factor loadings of all CFC items (principle-factor component). * items reversed prior to factor analysis. Panel B displays the correlation between CFC-future and CFC-immediate (based on loadings in Panel B and varimax rotation) and averages of CFC and USS factors.*

Table 2.B3: Predicted marginal effects of CFC and USS on revealed behavior

Binary outcome	(1) High BMI	(2) Smoker	(3) Reg. alcohol	(4) Concerns about nature?	(5) Concerns about climate change?	(6) Consume or save?
			Panel A: CFC only			
CFC-future	-0.027	-0.004	-0.007	0.065***	0.050**	0.045
	(0.021)	(0.020)	(0.021)	(0.022)	(0.024)	(0.077)
CFC-immediate	-0.006	0.036*	0.005	-0.136***	-0.128***	-0.089
	(0.021)	(0.019)	(0.021)	(0.022)	(0.023)	(0.078)
			Panel B: USS only			
USS-patience	0.004	-0.016*	-0.019**	-0.003	0.014	0.072**
	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)	(0.034)
USS-impulsiveness	0.023**	0.023***	0.014	-0.025**	-0.027**	-0.062*
	(0.009)	(0.009)	(0.010)	(0.011)	(0.011)	(0.034)
			Panel C: CFC and USS			
CFC-future	-0.024	0.001	-0.005	0.064***	0.046*	0.029
	(0.020)	(0.020)	(0.021)	(0.022)	(0.024)	(0.077)
CFC-immediate	-0.014	0.028	0.000	-0.133***	-0.122***	-0.068
	(0.020)	(0.019)	(0.021)	(0.022)	(0.023)	(0.078)
USS-patience	0.004	-0.015*	-0.020**	-0.007	0.011	0.072**
	(0.009)	(0.009)	(0.009)	(0.010)	(0.011)	(0.035)
USS-impulsiveness	0.023**	0.022**	0.014	-0.018*	-0.020*	-0.059*
	(0.010)	(0.009)	(0.010)	(0.011)	(0.011)	(0.035)

Source: Student survey, own calculations.
*Note: All marginal effects based on probit estimations with 396 observations, except Column (6) which uses an OLS regression. Robust standard errors in parentheses, *p<0.1, **p<0.05 and ***p<0.01. All estimations include controls on age, gender and height. 'High BMI' equals 1 if individuals BMI is greater than sample's 75th percentile (BMI > 23.1). 'Smoker' equals 1 if individual smokes. 'Alcohol' equals 1 if individual drinks one type of alcoholic beverage regularly. If individuals are 'very concerned' about 'environmental protection' or 'the impacts of climate change' the indicator in Column (4) and (5) equals 1. If they state 'somewhat concerned' or 'not concerned at all' indicators equal 0. Column (6) uses a 7-point-scale ranging from 1 'I like to spend all my money immediately' to 7 'I want to save as much as possible'.*

Chapter 3

Income in jeopardy: how losing employment affects the willingness to take risks

3.1 Introduction

The willingness to take risks is a key determinant of choices in life, such as migration, occupational sorting, investment decisions, or tax evasion. Accordingly, there is widespread interest in the origins of risk attitude among social scientists. It appears in previous research that risk attitude does not only stem from stable traits, but also varies around certain life events, with rare and seemingly minor ones among them. Surprisingly, there has not yet been established a link between the particularly far-reaching experience of job loss and risk attitude. We fill this gap by showing how people's risk attitude evolves around an involuntary termination of employment.

Based on previous theoretical and empirical literature, we assume that job loss affects a worker's risk attitude through affecting the monetary and non-monetary resources ('endowment') she employs to fulfill her needs. We hypothesize that job loss decreases both current and future endowment as well as increases uncertainty about the future ('background risk'). In line with standard economic theory and decreasing absolute risk aversion, we therefore expect the event to increase people's risk aversion. The effect should be measurable as soon as people start to anticipate job loss before the event, since growing job insecurity already shatters future expectations.

Our study relies on German Socio-Economic Panel data (SOEP) that provide us with a behaviorally validated measure of risk attitude. The panel structure enables us to follow workers over time and document their risk attitude before and after losing work. Moreover, we are able to approach a causal relationship between unemployment and the willingness to take risks as closely as possible, as the data include plant closure as an exogenous trigger of job loss. Considering a control group of people who do not lose work, we account for age and time trends in risk attitude by means of a difference-in-difference approach. Accordingly, time-stable worker heterogeneity is controlled for. The richness of our data allows us to artificially improve the comparability of people who lose work and the control group, to shed light on the channels through which job loss affects risk attitude, as well as to conduct various sensitivity checks.

We find that risk aversion increases upon job loss. Neither the direct income loss nor a change of emotional state nor parallel life events explain this result. The effect intensifies with the hourly wage earned before job loss, while it is not affected by previous job prestige and job satisfaction. We also observe that the effect already manifests itself over the last year before a loss of work, i.e. at a time when there is no instant impact of the event yet. Taken together, our results point to lowered and more uncertain future incomes as the main reason for the effect of job loss on risk attitude. Moreover, we find that people gradually return to their initial level of risk aversion after the event of a job loss, as they regain employment stability.

As we observe anticipation of job loss, we cannot rule out a selection of workers out of a firm that is about to fail. Reassuringly, we do not observe patterns of selection in the risk aversion of people who approach unemployment in the course of our sensitivity analysis. In addition, we show that our results neither differ dependent on firm size nor between service sector employees and industrial workers, which further mitigates concerns about potential selection. We can also confirm the behavioral validity of our findings by showing that those workers who become particularly risk-averse due to job loss are the ones who take up job offers the most quickly. Finally, our findings allow us to draw conclusions on the stability and exogeneity of risk attitude, as well as on the theoretical interpretation and thus applicability of self-reported risk attitude in empirical studies. Moreover, we relate our findings to risk aversion over the business cycle and to the underwriting of labor market risk by unemployment insurance.

We proceed as follows. Section 3.2 summarizes the previous literature. Section 3.3 discusses potential theoretical channels of how a job loss may affect risk attitude. We then describe the data and our empirical strategy in Section 3.4. The results are presented in Section 3.5. Sensitivity checks and a discussion on selection issues follow in Section 3.6. Section 3.7 concludes.

3.2 Previous literature and contribution

A notable fraction of an individual's risk attitude is inherited and shaped during childhood, i.e. at a time when personality traits are formed (Cesarini *et al.*, 2010; Becker *et al.*, 2012; Dohmen *et al.*, 2012; Mata *et al.*, 2012; Harrati, 2014; Deckers *et al.*, 2015). Afterwards, risk attitude still varies over time, as it, for instance, reduces over the life-cycle (Bucciol and Miniaci, 2011; Schurer, 2015; Dohmen *et al.*, 2017; Schildberg-Hörisch, 2018). Apart from that, the willingness to take risks responds to certain life events, as it decreases when becoming a parent (Görlitz and Tamm, 2015), losing a child (Bucciol and Zarri, 2015), being exposed to poverty (Haushofer and Fehr, 2014), or experiencing violence (Voors *et al.*, 2012; Callen *et al.*, 2014; Kim and Lee, 2014). Similarly, past and current macroeconomic conditions affect risk aversion. People who experienced financial crises in the past display a reduced willingness to take financial risks (Malmendier and Nagel, 2011; Guiso *et al.*, 2018; Dohmen *et al.*, 2016). Also, currently experiencing an economic downturn increases risk aversion, so that risk aversion evolves counter-cyclically (Cohn *et al.*, 2015; Bucciol and Miniaci, 2015, 2018).

Part of the instability of risk attitude is related to changes in the perception of a risk, for instance, when the experience of a natural disaster increases risk aversion (Cameron and Shah, 2015). Even people who have not been exposed to a disaster, but observe it, change risk attitudes, as Germans did after the Fukushima nuclear catastrophe (Goebel *et al.*, 2015). Hence, when risks are perceived to a greater extent, because they have become more salient, risk aversion adjusts. This even applies to seemingly minor changes in a person's life, such as changes in the media coverage of economic news (Tausch and Zumbuehl, 2018).

We contribute to the literature on life events and risk attitude by examining job loss, an event that many workers experience at least once in their lives, and that threatens every private sector employee at least to some extent.[1] Job loss does not only cause a reduction in material welfare, but wounds feelings of sense and purpose in life (e.g. Hetschko *et al.*, 2014; Kunze and Suppa, 2017). Accordingly, people report lowered well-being and end up in poorer mental health after job loss (Clark *et al.*, 2008; Browning and Heinesen, 2012). Even when workers have overcome unemployment, its impact persists. For some time, workers still receive lower wages, are confronted with a higher risk of becoming unemployed again and continue to report reduced well-being relative to the time before job loss (Arulampalam *et al.*, 2001; Eliason and Storrie, 2006; Hetschko *et al.*, 2019). Hence, a higher perception of risk after job loss seems objectively justified. We add to the literature about the effects of unemployment by examining risk attitude, an outcome whose reaction to job loss is unclear so far and that could explain why unemployment affects individual behavior in various respects (e.g. Del Bono *et al.*, 2012; Huttunen and Kellokumpu, 2016).

Evidence concerning the effect of losing work on risk attitude is scarce and relies on highly selective samples only. Sahm (2012) does not find risk tolerance in elderly workers to change after job displacement. Dismissals in her sample may often take place for individual reasons. The group of people who lose work is thus highly selective, since people may take the risk of being fired when behaving in a way that finally leads to their dismissal. Thus, we cannot infer from her finding that being laid off would leave risk attitude generally unchanged. The same argument applies to the results of Cho *et al.* (2018), who find that long-term unemployment renders people less risk-averse relative to the time before unemployment. However, the long-term unemployed are a selective group, too, so that we are unable to draw conclusions about the general effect of job loss on risk aversion from these findings.

Unlike Sahm (2012) and Cho *et al.* (2018), we rely on exogenously triggered job losses due to plant closure. This reason for the termination of employment affects all workers independently of their individual characteristics. It thus enables us to circumvent individual

[1] Previously, labor market research was concerned with the opposite relationship, i.e. the impact of risk attitude on labor market behavior. For instance, risk aversion accelerates job finding, since it reduces the reservation wage (Feinberg, 1977; Pannenberg, 2010). We, in contrast, examine how labor market experience shapes risk attitude. In this spirit, Brachert and Hyll (2014) examine whether self-employment is not only a result of risk attitude, but also a determinant.

selection into unemployment.[2] Moreover, the richness of our data allows us to analyze effect heterogeneity and potential channels, explaining the results in greater detail than previous research.

3.3 Theoretical considerations and hypotheses

Taken together, previous research on the impact of life events on risk attitude suggests that people become more risk-averse after a negative shock (cf. the previous section). In contrast, Sahm (2012) as well as Cho *et al.* (2018) present no evidence for decreasing willingness to take risks in unemployed people, but, as argued above, those findings demand closer scrutiny. Against this background, it is ex ante unclear how losing employment changes risk attitude. We therefore proceed with a theoretical discussion, allowing us to derive concrete hypotheses about the impact of job loss on risk aversion and the factors mediating that effect.

In economic theory, two concepts of risk attitude matter that should be clearly distinguished from each other. There is, first of all, the stable risk preference parameter (or underlying risk preference) that shapes the utility function (cf. Borghans *et al.*, 2008, p. 1002). It is assumed to be stable over time and thus resembles a personality trait.[3] Secondly, the local risk preference, represented by absolute risk aversion, reflects current risk tolerance, which depends not only on the curvature of the utility function, but also on the current circumstances of living ('endowment'). Hence, the risk attitude that we observe in people will reflect the risk preference parameter only if it is arguably unaffected by the current endowment. Job loss, however, strongly changes the circumstances of life (see previous section), which is why it seems most plausible to start from the premise that job loss affects risk attitude through changing endowment.[4]

Following Arrow (1971) and Pratt (1964), an individual's current risk attitude can be described by the level of absolute risk aversion (ARA), i.e. $ARA(c) = -u''(x)/u'(x)$ with x as the level of endowment. Accordingly, ARA varies across individuals, not only due to heterogeneous underlying risk preference, but also within individuals due to different levels of endowment. It is also assumed that absolute risk aversion decreases with the level of endowment (DARA), as suggested by empirical studies (Guiso and Paiella, 2008).

[2] For previous analyses of plant closures using the same data see Kassenboehmer and Haisken-DeNew (2009), Schmitz (2011), or Marcus (2013).

[3] In structural modelling, for instance, risk attitude represents the risk aversion parameter γ in the utility function $v(x) = (x^{1-\gamma} - 1)/(1-\gamma)$ with x as endowment level (e.g. Bombardini and Trebbi, 2012; Fossen and Glocker, 2017).

[4] This premise is also in line with studies on the effect of unemployment on personality traits. If these studies find an effect at all, it is characterized as negligible or as being subject to measurement issues (Cobb-Clark and Schurer, 2012, 2013; Anger *et al.*, 2017). See Chapter 4 for a detailed discussion.

We consider two channels to mediate the effect of losing work on ARA. First, unemployment reduces current endowment x. Although unemployment insurance and severance payments might buffer monetary losses to some extent, non-monetary losses of unemployment (see Section 3.2) are not compensated. Assuming DARA, losses in x should therefore increase ARA. Second, a job loss puts future income in jeopardy, since the duration of unemployment is not fully predictable and the wages paid by future employers are lower and less certain due to deterioration of human capital (Schwerdt *et al.*, 2010) and negative signaling (Kroft *et al.*, 2013). But, in contrast to current endowment, the effect of being laid off does not only affect the level of future endowment, it also increases its uncertainty. To some extent, future endowment is unknown to individuals, as unexpected events constitute a persistent unpredictability which is neither mutable nor insurable. It is thus best described by the cumulative distribution function $F(y)$. Thereby, it introduces to $ARA(\bullet)$ a component similar to a '*background risk*' (Kihlstrom *et al.*, 1981; Nachman, 1982). In the case of a job loss, the expected value of $F(y)$ decreases, while its variance increases due to the scarring effects of unemployment. In both cases, ARA rises, i.e. $\partial ARA(x,\tilde{y})/\partial E(\tilde{y}) < 0$ and $\partial ARA(x,\tilde{y})/\partial Var(\tilde{y}) > 0$ (see Eeckhoudt *et al.*, 1996).[5]

As the effects of job loss on current and future endowment work in the same direction, we expect ARA to increase when people lose work. For this reason, our main hypothesis reads

> *Hypothesis 1: Job loss decreases the willingness to take risks.*

If hypothesis 1 turns out to hold true, we aim at identifying the mediating channel. We hereby, first of all, consider the immediate loss of income.

> *Hypothesis 2: The reduction of income explains the increase of risk aversion when people lose work.*

We follow two indirect approaches to identifying future income expectations as a potential driver of changes in risk attitude upon job loss. First of all, the effect of job loss should increase with the loss of future endowment. This deterioration in expectation is, however, unobservable. Alternatively, we aim to approximate the deterioration of future expectation by the level of productivity before the job loss takes place. Highly productive workers can expect to have high returns to employment in the future, unlike lowly productive individuals. The latter are also more likely to become unemployed in general. As a result, they have less endowment to lose in the case of a job loss. One might object that the lowly productive worker should also struggle more with finding a new job, which would inflate

[5] If $F_1(y)$ deteriorates to $F_2(y)$, i.e. $F_1(y)$ stochastically dominates $F_2(y)$ by the first degree, and DARA in the sense of Ross (1981) holds, $ARA(x)|F_1(y) < ARA(x)|F_2(y)$ will apply (see Eeckhoudt *et al.*, 1996).

the loss of future income, but, empirically, job finding rates do not differ across skill levels, unlike separation rates (e.g. Cairó and Cajner, 2018). Altogether, we suspect

> *Hypothesis 3: The effect of losing work on risk attitude intensifies with the level of workers' productivity.*

Secondly, we exploit the panel structure of our data to provide another test for the role of future expectations. In contrast to the losses of current income and other benefits of work, increasing uncertainty and lower expected future income may affect workers already on the eve of job loss when people still receive their wages and benefit from work. When the shadow of death of a firm arises, as employers lack competitiveness or even start insolvency proceedings (Griliches and Regev, 1995), people experience higher job insecurity and thus already start to perceive the risk of future income losses to a growing extent. If future income expectations play a part in the effect of job loss on risk attitude, we should thus also be able to confirm

> *Hypothesis 4: The willingness to take risks already decreases some time before job loss.*

3.4 Empirical methodology

Measures Our analysis is based on eleven waves (2004-2014) of German Socio-economic Panel (SOEP) data (Goebel *et al.*, 2019). Each wave comprises 20,000 individuals living in 11,000 households. The same people are repeatedly interviewed at intervals of roughly one year. They provide information on manifold personal perceptions and attitudes, their employment status, income, health and more. Our measure of risk attitude is available in the SOEP waves of 2004, 2006, and for every wave from 2008 onwards:

> *'Would you describe yourself as someone who tries to avoid risks (risk-averse) or as someone who is willing to take risks (risk-prone)? Please answer on a scale from 0 to 10, where 0 means "risk-averse" and 10 means "risk-prone".'*

We follow Dohmen *et al.* (2011) and name this measure the general risk attitude, GRA, in what follows. It is assumed to elicit absolute risk aversion rather than an underlying risk preference parameter, allowing us to test our hypotheses from above. In fact, the wording of the item closely resembles the definition of Kimball (1990, p. 54) on absolute risk aversion, i.e. '*how much one dislikes uncertainty and would turn away from uncertainty if possible*'.[6]

[6] One might wonder whether the item could also elicit relative risk aversion, but in that case the statement would need to anchor the current endowment as a reference point, for instance along the lines of Eeckhoudt *et al.* (2005, p. 19) who illustrate relative risk aversion by the question "*If your wealth would increase, would*

Dohmen *et al.* (2011) as well as Falk *et al.* (2016) document that people answer to the GRA in line with other measurements of risk attitudes, such as real-stake lotteries, holding stocks or being self-employed. According to Lönnqvist *et al.* (2015), the GRA is, thanks to its behavioral validity and high retest stability, even better suited to elicit risk tolerance than the lottery task invented by Holt and Laury (2002).[7] In addition, the GRA is related to a long list of risk behaviors in the expected way, such as migration (Jaeger *et al.*, 2010), occupational sorting (Bonin *et al.*, 2007; Pfeifer, 2010; Caliendo *et al.*, 2010, 2014; Skriabikova *et al.*, 2014) and educational choice (Fossen and Glocker, 2017).

When workers have terminated an employment relationship since the previous SOEP interview, they are asked about the specific reason: '*How did that job end?*'. Answers saying that the '*office or place of work has closed*' (plant closure in the following) identify exogenously triggered job losses best. At a later stage, we also make use of data about other dismissals ('*I was dismissed by my employer*') to show how including more endogenous reasons for job loss affects our findings.[8]

Our empirical analyses consider various other socio-demographic characteristics, such as age, ISCED level of education, gender, marital status, children living in household, migration background, and unemployment in the past. Net household income is adjusted by OECD equivalent weights (1 for the first adult in the household, 0.5 for all further people being older than 14 years, 0.3 for all children below that age). Job characteristics are also considered (wage, autonomy in occupational actions, tenure in years, company size, sector of industry). We also merge our data with precise 'INKAR' (*indicators of the development of cities and regions*) information about unemployment rates in the 96 German planning regions (*Raumordnungsregionen*, see BBSR, 2015). Data on workers' concerns about the security of their jobs (not concerned at all, somewhat concerned, very concerned) are used to shed light on the anticipation of job loss. We take into account the participant's emotions and use the reported frequency of feeling anxious, happy, sad and angry over the past four weeks. The corresponding five-point scales range from 'very rarely' to 'very often'. Occupational prestige based on the Standardized International Occupational Prestige Scale (see Ganzeboom and Treiman, 1996) as well as job satisfaction on a scale from 0 to 10 are considered as measures for non-monetary benefits of work.

Sampling We apply a difference-in-differences approach to identifying the effect of job loss on GRA. The change in the risk attitude of a treatment group of people who lose their

you want to devote a larger or a smaller share of your wealth to get rid of a given zero-mean proportional risk? For example, what would you pay to avoid the risk of gaining or losing 20 % of your wealth, each with an equal probability?".

[7] For a wider discussion of different measures of risk attitude see Charness *et al.* (2013).

[8] The remaining unconsidered categories are retirement age reached, end of fixed-term contract / apprenticeship, own resignation, mutually agreed termination, suspension / parental leave, ceased self-employment.

work is calculated from the time before the event to the time afterwards and compared to the change in risk attitude in a control group of people who stay employed. Both treatment and control group are limited to working-age people being between 18 and 65 years old. Self-employed workers and public sector employees are not considered, as they do not compare to private sector employees regarding the risk of job loss. We restrict employment to part-time or full-time employment with a number of at least 15 working hours per week, which means that marginal and workfare employment as well as people participating in job-creation schemes are excluded.

For the treatment group, we choose the second-last SOEP interview before job loss as pre-treatment reference point in time. This is because we expect anticipation directly before job loss, as mentioned in Section 3.3, and aim at testing whether people already adjust risk attitude before the event, i.e. from the second-last to the last interview beforehand. As a result, the treated need to be observed as employed for at least two consecutive interviews ('$t = -2$' and '$t = -1$') before they lose their job because of a plant closure ('$t = 0$' is hence the first interview afterwards). They are not restricted to certain labor market states *after* job loss. Besides having taken up a new job, they can be unemployed, have left the workforce, or do occasional jobs. Not being selective at this point avoids any systematic sampling bias. Given these restrictions, we identify 203 treated observations of people who provide us with all the information taken into account in the empirical analyses. 11 treated observations got lost due to missing values.

A comparison group of employees is included in order to control, for instance, for age effects and time trends in the willingness to take risks. Individuals in this group are either part-time or full-time employed for at least three interviews in a row, which we assign to $t = -2$, $t = -1$ and $t = 0$. Members of the control group do not experience a plant closure or a dismissal, but might voluntarily change their job. This allows us to test whether a job change mediates the effect of an involuntary job loss on risk aversion. Given these restrictions, we obtain 24,906 control group observations. Missing values reduce this number by 1,573 observations. Table 3.1 describes treated and control units by several characteristics measured at the pre-treatment reference point in time. While GRA does not differ significantly between the two groups, other characteristics do, particularly attributes of the current job.

Estimation According to our timeline, the treatment effect of interest is the difference in the change of the willingness to take risks (ΔGRA) from $t = -2$ to $t = 0$ between treatment and control group. The key identifying assumption is that the controls experience the counterfactual trend in GRA the treated would have experienced if they had not been treated (parallel trend assumption). To improve the credibility of this assumption, we artificially

Table 3.1: Descriptive statistics at the pre-treatment reference point in time ($t = -2$)

	Treamtent group		Control Group		Difference
Number of observations	203		24,906		
	mean / share	sd	mean / share	sd	
Key variable					
General risk attitude (mean, scale 0-10)	4.87	2.24	4.72	2.15	0.15
Pre-treatment socio-demographics					
Age in years (mean)	43.98	9.52	43.13	9.74	0.85
Monthly net equiv. household income (mean)	1647.99	774.12	1808.35	997.21	-160.36^{***}
Educational level (mean, scale 1-6)	3.61	1.29	3.88	1.35	-0.27^{***}
Years of unemployment (mean)	0.57	1.24	0.50	1.24	0.07
Local unemployment rate (mean, in %)	9.22	4.02	8.68	3.91	0.55^{*}
Men (share)	0.62		0.60		0.02
Children in household: yes (share)	0.33		0.36		-0.03
Married (share)	0.67		0.63		0.03
Migration background (share)	0.13		0.10		0.03
East Germany (share)	0.25		0.24		0.01
Pre-treatment job characteristics					
Net hourly wage (mean)	9.58	5.12	10.21	5.44	-0.62^{*}
Tenure in years (mean)	11.68	9.58	11.64	9.47	0.05
Occupational autonomy (mean, scale 1-5)	2.58	1.01	2.77	1.05	-0.19^{***}
Company size (mean, scale 1-3)	2.08	0.81	2.23	0.80	-0.14^{***}
Weekly working hours (mean)	41.15	9.41	41.42	9.36	-0.27
Part-time contract (share)	0.16		0.16		0.00
Sector of industry (shares, sum = 1.00)					
Extraction, exploitation	0.02		0.04		-0.02
Manufacturing	0.32		0.37		-0.06^{*}
Construction	0.09		0.07		0.02
Trade, transport	0.07		0.07		0.01
Food/domestic services, sales, hotel	0.30		0.16		0.14^{***}
Media, finance, real estate	0.14		0.16		-0.02
Administration, education, health	0.05		0.13		-0.08^{***}

Source: SOEP 2004-2014, INKAR 2004-2012.
Note: $^{*}p<0.1$, $^{**}p<0.05$ and $^{***}p<0.01$.

increase the similarity of the two groups by considering socio-demographic characteristics (vector **SD**) and job characteristics (vector **JC**) displayed in Table 3.1 as controls in a multiple regression analysis. The linear model describes the change in ΔGRA dependent on the treatment indicator *JobLoss*, which equals one if people lose work between $t = -2$ and $t = 0$, and zero otherwise. The year of the interview (**Y**) of $t = 0$ accounts for time trends in GRA. α is the average change in GRA in the reference group and ε_i represents the error term.

$$\Delta GRA_{i,t=-2\to t=0} = \alpha + \beta JobLoss_{i,t=-1\to t=0} + \mathbf{SD}'_{i,t=-2}\gamma + \mathbf{JC}'_{i,t=-2}\delta + \mathbf{Y}'_{i,t=0}\theta + \varepsilon_i \tag{3.1}$$

Consequently, sign and significance of the β-coefficient provide us with evidence regarding the effect of losing work on risk attitude, as measured on the 11-point GRA scale (Hypothesis 1). Model (3.1) will be augmented by the change in income from $t = -2$ to $t = 0$ to test whether the income loss explains the effect of job loss on risk aversion (Hypothesis 2).

We also use the linear model to investigate the role of future income expectations in the effect of job loss on GRA (Hypothesis 3). To approximate income prospects, we interact job loss with previous net hourly wage as a measure of a worker's productivity. Alternatively, a high wage before job loss might reflect high non-monetary benefits. Hence, a positive interaction effect could reflect a larger loss of those benefits, besides particularly high losses of and uncertainty about future income. To shed light on these different channels, we also interact more direct measures of non-pecuniary benefits with our treatment indicator (see Section 3.5.2 for further details). Moreover, we test Hypothesis 4 (anticipation effect) by replacing the outcome variable with the change in risk attitude between the two interviews *before* job loss, i.e. $\Delta GRA_{t=-2\to t=-1}$.

3.5 Results

3.5.1 The effect of job loss on risk attitude

We start by comparing the average two-year change from $t = -2$ to $t = 0$ in the general willingness to take risks between treatment and control group (see Table 3.2, Column (1)). It turns out that risk tolerance evolves 0.454 point more negatively when losing work than when staying employed ($p < 0.01$). When the year of the interview of $t = 0$, socio-demographics and job characteristics at $t = -2$ are controlled for, we still find a highly significant negative effect of experiencing job loss due to a plant closure on the stated risk preference (Column (2)). Thus, the measured treatment effect does not originate from time trends in risk attitude or differences in the characteristics of affected and unaffected workers. We conclude that our analysis supports Hypothesis 1, i.e. job loss increases risk aversion. Standardizing the effect of -0.474 in Column (2) implies a reduction in GRA by 22 % of one standard deviation. According to the results of Dohmen *et al.* (2011), a reduction in the willingness to take risks of this size decreases the average probability of investing in stocks by 4.0 %, of doing sports by 4.0 % and of smoking by 5.7 %. We therefore consider the effect of job loss as substantial.

After testing our first hypothesis, we shed light on the channels leading to a reduction in risk tolerance in the wake of job loss. First, we augment the empirical model with the change of equivalent household income to test whether the immediate income loss associated with job loss is a reason for the decrease of risk tolerance. This extension leaves our results unchanged (Column (3)). A job loss still reduces the risk tolerance by -0.474, so that Hypothesis 2 is rejected. A likely explanation is that the generous German unemployment insurance, together with income provided by other household members, buffers the loss of own labor earnings. In fact, the income loss amounts to 8 % only.

Typically, emotions are associated with changing risk attitude (e.g. Kusev *et al.*, 2017), and they might also be triggered by job loss. We test for the alternative explanation of our results that emotions cause the change in risk aversion by including the changes in the frequency of feeling angry, anxious, happy and sad. The corresponding results are presented in Column (4). Despite a smaller sample size due to missing information on emotions, which were not included in the SOEP before 2007, we still find job loss to affect GRA significantly by -0.447 points.[9] Emotions are significantly related to changing risk attitude in line with the previous literature, but they do not mediate the effect of job loss.

Job loss often leads to other changes in life, such as taking up a new job, or moving to a new place of residence. Such events may have an effect on risk aversion and could also explain the impact of job loss. We therefore extend the set of control variables to parallel life events taking place from $t = -2$ to $t = 0$ (Column (5)). However, this does not change our results either.

To check the generalizability of our main finding, we repeat the specification underlying Column (2) for several subgroups (results summarized in Table 3.A1 in Appendix 3.A). Like the initial sample, all subgroups show a negative sign of the treatment effect. Men and relatively young individuals seem to respond more strongly to job loss. However, interaction effects in an estimation with the whole sample do not imply that these differences are statistically significant.

[9] The effect seems to be slightly smaller compared to Column (3). This is due to the different sample underlying the estimation in Column (4).

Table 3.2: OLS estimation of the effect of job loss on risk tolerance

	(1)	(2)	(3)	(4)	(5)
Job loss between $t=-1$ and $t=0$	-0.454*** (0.156)	-0.474*** (0.154)	-0.474*** (0.154)	-0.447** (0.191)	-0.487*** (0.156)
Pre-treatment socio-demographics					
Age in years		0.004** (0.002)	0.004** (0.002)	0.004* (0.002)	0.004** (0.002)
Monthly HH income (log)		0.001 (0.040)	0.001 (0.043)	0.014 (0.051)	0.002 (0.043)
ISCED level (ref. level 3)					
Level 1		0.444* (0.244)	0.444* (0.244)	0.335 (0.319)	0.441* (0.243)
Level 2		0.067 (0.065)	0.067 (0.065)	0.049 (0.078)	0.068 (0.065)
Level 4		-0.018 (0.049)	-0.018 (0.049)	0.017 (0.057)	-0.022 (0.049)
Level 5		-0.018 (0.050)	-0.018 (0.050)	-0.057 (0.059)	-0.022 (0.050)
Level 6		-0.003 (0.041)	-0.003 (0.041)	0.007 (0.048)	-0.006 (0.041)
Years of unemployment		0.009 (0.013)	0.009 (0.013)	0.020 (0.014)	0.009 (0.013)
Local unemployment rate (%)		0.000 (0.006)	0.000 (0.006)	0.008 (0.007)	0.000 (0.006)
Male		0.039 (0.035)	0.039 (0.035)	0.074* (0.042)	0.042 (0.035)
Children in household: yes		0.069** (0.033)	0.069** (0.034)	0.067* (0.040)	0.070** (0.034)
Married		0.032 (0.032)	0.032 (0.032)	0.038 (0.038)	0.037 (0.034)
Migration background		0.098* (0.055)	0.098* (0.055)	0.037 (0.067)	0.101* (0.055)
East Germany		0.039 (0.048)	0.039 (0.048)	0.028 (0.054)	0.041 (0.048)
Pre-treatment job characteristics					
Net hourly wage (Euros)		-0.000 (0.003)	-0.000 (0.003)	-0.003 (0.004)	-0.000 (0.003)
Tenure in years		0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)
Occupational autonomy (ref. level 3)					
Level 1		-0.021 (0.056)	-0.021 (0.056)	-0.051 (0.067)	-0.020 (0.056)
Level 2		0.025 (0.037)	0.025 (0.037)	-0.005 (0.044)	0.024 (0.037)
Level 4		-0.040 (0.040)	-0.040 (0.040)	-0.018 (0.047)	-0.038 (0.040)
Level 5		-0.176** (0.083)	-0.176** (0.083)	-0.180* (0.096)	-0.174** (0.083)
Company size (ref. 20 to 200 emp.)					
up to 20 employees		-0.012 (0.038)	-0.012 (0.038)	-0.028 (0.044)	-0.013 (0.038)
more than 200 employees		0.005 (0.033)	0.005 (0.033)	-0.021 (0.038)	0.004 (0.033)
Weekly working hours		-0.003 (0.002)	-0.003 (0.002)	-0.001 (0.003)	-0.003 (0.002)
Part-time contract		-0.045 (0.054)	-0.045 (0.054)	-0.018 (0.063)	-0.045 (0.054)
Sector of industry (ref. manufacturing)					
Extraction, exploitation		-0.000 (0.068)	-0.000 (0.068)	-0.047 (0.078)	-0.002 (0.068)
Construction		0.026 (0.058)	0.026 (0.058)	0.045 (0.068)	0.024 (0.058)
Trade, transport		-0.021 (0.056)	-0.021 (0.056)	-0.076 (0.068)	-0.024 (0.056)
Services		0.050 (0.044)	0.050 (0.044)	0.056 (0.052)	0.048 (0.044)
Media, finance, real estate		-0.054 (0.040)	-0.054 (0.040)	-0.108** (0.048)	-0.055 (0.040)
Administration, education health		0.030 (0.046)	0.030 (0.046)	0.047 (0.054)	0.029 (0.046)
Change in monthly HH income (log)			0.001 (0.054)	-0.057 (0.064)	0.004 (0.054)
Change in frequency of emotions					
Anger				0.027 (0.017)	
Anxiety				-0.037* (0.019)	
Happiness				0.078*** (0.021)	
Sadness				-0.020 (0.017)	
Parallel life events					
New job					0.025 (0.046)
Divorce					-0.062 (0.117)
Separation					0.258*** (0.078)
Death of spouse					-0.100 (0.366)
Marriage					0.025 (0.071)
Child birth					-0.016 (0.077)
Relocation					-0.017 (0.080)
Constant	0.065*** (0.013)	0.253*** (0.062)	0.253*** (0.062)	0.277*** (0.070)	0.239*** (0.063)
Year dummies (ref. 2012)	no	yes	yes	yes	yes
Observations	25,109	25,109	25,109	17,006	25,109
Adj. R-squared	0.000	0.025	0.025	0.026	0.025

Source: SOEP 2004-2014, INKAR 2004-2012.

*Note: The table presents OLS estimates of the change in GRA between $t=-2$ and $t=0$. Robust standard errors in parentheses, *p<0.1, **p<0.05 and ***p<0.01. Continuous variables are mean-centered. Changes in frequency of emotions measured from $t=-2$ to $t=0$. Similarly, parallel life events occur between $t=-2$ and $t=0$.*

3.5.2 The role of future income expectations

We conjecture that shattered future income expectations are a reason why job loss alters risk attitude (Hypothesis 3). It is therefore examined whether people respond to job loss according to their individual level of prospects that are put into jeopardy. Building on Model (3.1), we interact proxies for income expectations with job loss. To start with, we assume that the pre-job loss net hourly wage strongly correlates with future earnings. The results are presented in Table 3.3. At the sample average hourly wage of 10.20 euros, losing work decreases ΔGRA significantly by 0.505 points (Column (1)). Each additional euro earned per hour before job loss adds to that effect by -0.050 points. Accordingly, the impact of job loss on risk tolerance becomes more positive at lower wages. At about three euros, it vanishes completely, although we hardly observe such low wages in our data.

As uncertainty about future income should diminish when people get older and approach the usual retirement age, we use the current net labor income times the currently remaining years until retirement age (which is usually 65 years) as another proxy for the amount of future income. The product of net monthly income and the time until retirement also significantly strengthens the impact of job loss on risk aversion (Column (2)).

Finally, we interact our treatment indicator with the number of previous unemployment spells. The more often individuals experienced unemployment before, the smaller their expectations on the future should be. Accordingly, Column (3) indicates that the effect size is smaller within individuals who experienced loss of employment before. Individuals who are more familiar with the risk of unemployment report, on average, smaller effects. Each separate unemployment spell within the previous working life increases the negative effect by +0.073.

To the extent that non-monetary benefits of work increase with productivity, the interaction variables we use so far might also point to losses of non-monetary benefits as alternative explanation for the impact of job loss on risk aversion. To cast light on this conjecture, we additionally examine interaction effects of variables that are more directly associated with those non-monetary benefits: the pre-treatment level of job satisfaction as well as the level of pre-treatment occupational prestige. However, these variables do not significantly interact with job loss (see Columns (4) and (5)). Losses of non-monetary benefits of work thus do not seem to explain our findings.

3.5.3 Timing of the effect

Anticipation As argued in Section 3.3, increasing uncertainty about future income may arise and already make people more risk-averse ahead of the job loss, as they feel increasingly insecure about their current job. In fact, we find that the treated report an increase of their concerns about job security from the second-last interview before job loss ($t = -2$,

Table 3.3: Effect of job loss on risk preference by income expectations

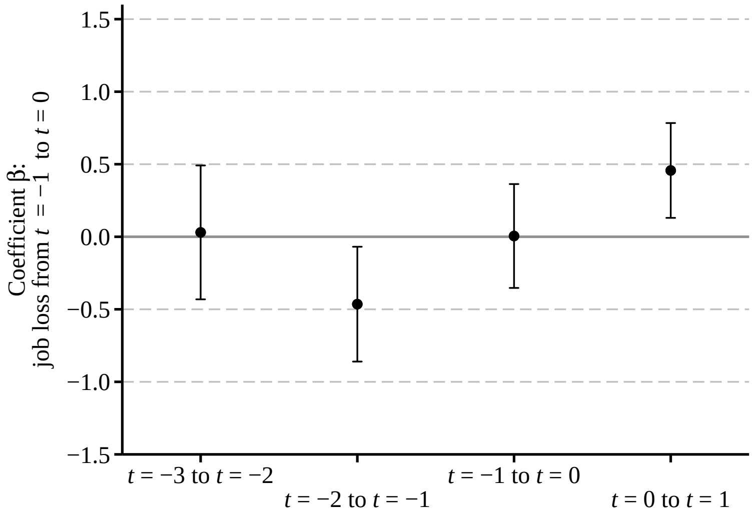
Interaction variable	(1) Wage	(2) income × (65− age)†	(3) unemployment spells	(4) work satisfaction	(5) level of prestige
Job loss	-0.505***	-0.512***	-0.548***	-0.480***	-0.456***
	(0.147)	(0.149)	(0.163)	(0.156)	(0.151)
Interaction variable, level	0.000	-0.105	-0.008	-0.006	-0.001
	(0.003)	(0.070)	(0.007)	(0.007)	(0.002)
Job loss × interaction variable	-0.050**	-1.074*	0.073*	-0.061	0.004
	(0.024)	(0.636)	(0.044)	(0.079)	(0.013)
Constant	0.252***	0.239***	0.253***	0.234***	0.253***
	(0.061)	(0.061)	(0.062)	(0.062)	(0.062)
Controls	yes	yes	yes	yes	yes
Observations	25,109	25,109	25,109	24,500	25,086
Adj. R-squared	0.025	0.025	0.025	0.026	0.025

Source: SOEP 2004-2014, INKAR 2004-2012.
*Note: Robust standard errors in parentheses, *p<0.1, **p<0.05 and ***p<0.01. † in 100K. The table presents OLS estimates of the change in GRA between* $t = -2$ *and* $t = 0$*. Hourly wage calculated by gross monthly labor income and actual weekly working hours (in euros). The control variables are specified as in Table 3.2, Column (2). Pre-treatment characteristics denoted in first row are centered at the sample mean.*

on average 18 months before job loss) to the last interview ($t = -1$, on average 6 month before job loss) of about 0.222 points on a three-point scale ($p < 0.001$). To test whether this insecurity already leads to an increase of risk aversion, we redefine the dependent variable as ΔGRA between $t = -2$ and $t = -1$ and estimate Model (3.1) again. This produces, indeed, a negative impact of an upcoming job loss on the willingness to take risks (Figure 3.1), supporting Hypothesis 4. Again, concerns about the future are suggested as reason for the effect, since in the year before job loss, no real damage on monetary or non-monetary aspects has occurred yet. As these concerns do not fade away directly after job loss, they may likewise explain the impact of job loss itself on risk attitude. In line with this conjecture, it does not seem to matter for the effect on risk attitude whether job loss is expected to happen or has already taken place. Estimating the change in the stated risk preference from $t = -1$ to $t = 0$ reveals that, when job loss actually happens, risk aversion does not increase beyond the anticipation effect.

Adaptation Up to here, we have identified a positive short-term effect of job loss on risk aversion, the strength of which does not only depend on its magnitude (Subsection 3.5.1), but also on how long it lasts. Replicating the estimation of Model (3.1) for the evolution of risk aversion from $t = 0$ to $t = 1$, we find increasing willingness to take risks in people who lost their jobs before, but are now increasingly taking up new jobs (72 % of all observable

Figure 3.1: Anticipation and reversion of the impact of job loss on risk attitude



Source: SOEP 2009-2014, INKAR 2009-2012.
Note: The figure illustrates a timeline of treatment effects obtained from running separate estimations of yearly changes in GRA from $t = -3$ (the third-last interview before job loss) to $t = 1$ (the second interview after job loss). All individuals from the baseline sample (see Table 3.1) with available answers to GRA in $t = -3$, $t = -1$ or $t = 1$ are included. Whiskers denote 95 % confidence intervals. Note that the β-coefficients are predicted values based on the model specification presented in Column (2) of Table 3.2. Complete results of the underlying estimations are presented in Appendix 3.A (Table 3.A2).

treated are reemployed in $t = 1$). We take this finding as further indication of the role of uncertainty about future income in the impact of job loss on risk attitude, since people readjust as soon as uncertainty goes down again. Even people who are directly reemployed at $t = 0$ do not return to their initial pre-job loss level of risk aversion before $t = 1$, probably because they need to reestablish in the new job first (i.e. survive probation, qualify for a permanent contract) in order to regain the employment security that takes away their uncertainty about the future.

Parallel trend assumption The credibility of our results depends on the assumption that treated and control observations would not show different trends in risk attitude, if the treated were prevented from losing work. Reassuringly, we can at least confirm that they display similar trends prior to our pre-treatment reference point in time $t = -2$. As Figure 3.1 shows, redefining ΔGRA as the change from the third-last year ($t = -3$) to the second-last year ($t = -2$) before job loss does not indicate a difference.[10] Moreover, unlike in the year after $t = -2$, we do not observe a significant change in concerns about job security from $t = -3$ to $t = -2$.

[10] Also, the changes in GRA from $t = -5$ to $t = -4$ and from $t = -4$ to $t = -3$ do not differ significantly between treated and control units, whereby the size of the treatment group that allows us to go as far back in time is small.

3.6 Robustness

3.6.1 Model modifications

Thus far, we have included our set of control variables in a linear model. To relax the functional form assumption, we can implement a non-parametric weighting procedure called 'Entropy Balancing' (Hainmueller, 2012). An algorithm re-weights the previously defined control group upon the condition that its means in observable characteristics matches those of the treatment group, i.e. a synthetic control group is built. The results are summarized in Table 3.A3 (Column (2), see Appendix 3.A). Compared to our preferred specification from Table 3.2 (Column (2)), the treatment coefficient hardly changes.

As another sensitivity check, we address the scale of measurement of the willingness to take risks, of which we need to assume cardinality to interpret our OLS estimates. Alternatively, we apply an ordered probit model covering the same covariates. This also produces a negative effect of job loss on risk tolerance (see Column (3) of Table 3.A3).

3.6.2 Does anticipation lead to selection?

The fact that people anticipate a loss of employment to some extent might challenge a causal interpretation of our treatment effect. A non-random group of people might deliberately select out of a failing business. The design of the SOEP questionnaire should, at least partly, resolve this issue, as it does not require respondents to have stayed with the closing firm until the very end when indicating plant closure as reason for the termination of an employment relationship. If someone left the firm only because she anticipated the plant closure in advance, she probably indicates this reason and is thus part of our treatment group. In line with this conjecture, the treatment group contains workers who did not even spend a month of unemployment after job loss (12.3 % of all treated).

To discuss potential patterns of selection further, we again refer to the descriptive statistics displayed by Table 3.1. If highly risk-averse people selected out of an endangered company, the remaining sample of people who stay until the end and become part of the treatment group would appear relatively risk-prone. However, leaving a failing firm seems to be a risky choice, too, as the length of unemployment and the characteristics of future jobs are partly unknown, so that people who stay could also make for a relatively risk-averse sample. The data point in neither direction, as workers' willingness to take risks at $t = -2$ varies insignificantly between treated and control units.

Characteristics that do differ between the two groups are considered by the regression analysis. Hence, only unobserved reasons for selection remain an issue unless they are sufficiently correlated with the variables considered by the model. To assess the relevance of this potential threat to our empirical strategy, we analyze whether our control variables account for each other, assuming that they should likewise account for as yet unobserved characteristics. We employ a probit estimation of the propensity to lose work from $t = -1$ to $t = 0$ because of a plant closure, considering the characteristics displayed in Table 3.1 as explaining variables. The results are reported in Table 3.A4 (Appendix 3.A). Column (1) shows that many single characteristics that differ significantly between treated and controls are not associated with the probability of job loss anymore once all of the explaining variables are considered simultaneously (e.g. education, company size, hourly net wage). Only being employed in the food/domestic, sales or hotel sector and being a resident of a high unemployment area continue to predict the experience of a plant closure. Hence, our control variables may capture differences in unconsidered characteristics, too. This justifies a causal interpretation of the difference-in-difference effect, while the issue of anticipation should be kept in mind as a possible caveat.

3.6.3 Firm size, sector of industry, job-to-job transitions and individual dismissals

Two concerns induce us to separate the sample by firm size. On the one hand, the death of a failing business that employs only a few people might have been influenced by the individual employee, which would question the exogeneity of her job loss. On the other hand, if major corporations close a few plants only, they will potentially be able to offer employees a different position at another plant. Those who nevertheless leave the firm, and are hence identified as a case of job loss in our data, might then be selected in some respect. To address any concerns regarding firm size, we rerun our estimations based on a sample that excludes both the largest (> 2000 employees) and the smallest category of firms (≤ 5 employees). The effect of job loss on the willingness to take risks hardly differs from the whole sample, which confirms the robustness of our results (see Table 3.A5 in Appendix 3.A).

Dividing the sample by sectors of industry allows us to shed light again on anticipation and the potentially resulting issue of selection (see Section 3.6.2). According to Blanchard *et al.* (2014), a shadow of death before firms close occurs, in particular, in manufacturing industries (e.g. food/textile, wood, paper chemicals, metal and non-metal production, recycling and construction; 'non-service sector' in the following). In contrast, service sector employees (e.g. trade, hotels, restaurant, communication, transport, financial services and real estate), which account for a large share of our treatment group (about 51 %), have less time to anticipate the event. We nevertheless find a treatment effect in this group (see again Table 3.A5) that is arguably no different from the whole sample and the non-service sector employee sample. Hence, having the chance to anticipate the plant closure, and therefore selecting out of a failing business, does not seem to alter the treatment effect. As another check, we exclude those treated from the estimation who report an immediate job-to-job transition, i.e. without any unemployment in-between, as they could have been able to anticipate the job loss early on. The results remain practically the same (see again Table 3.A5).

Up to here, our results differ from those of Sahm (2012) who finds no impact of unemployment on risk attitude. One explanation why our approach produces findings different from those of Sahm (2012) may be that she, unlike us, includes the endogeneity of individual dismissals in her data. In fact, adding dismissals other than for the reason of a plant closure to our treatment group induces the negative impact of displacement on stated risk preference to disappear (see Table 3.A5 in the Appendix). Applying again the probit estimations discussed in Section 3.6.2 to the probability of experiencing any kind of dismissal by employer from $t = -1$ to $t = 0$ substantiates concerns regarding the selectivity of such a sample. Even if all characteristics are simultaneously controlled for, single indicators do not lose their predictive power in explaining the experience of individual dismissal (see Appendix 3.A, Table 3.A4). This supports our strategy to focus on plant closures only.

3.6.4 Behavioral validity

To check the behavioral validity of our results, we look at the subgroup of people that are observed as quickly reemployed after job loss (by the SOEP interview that marks $t = 0$). Theoretically, those who suffer the most from this life event should aim at fixing the damage the soonest. Hence, the more risk-averse people become, the quicker they should take up a new job (cf. Feinberg, 1977). In fact, when the treated are separated by their labor force status at the first interview after the job loss ($t = 0$), we find the strongest effects on ΔGRA from $t = -2$ to $t = 0$ for those who have already entered regular employment again (see Table 3.A1, Columns (5) and (6) in Appendix 3.A). As we control for education and labor market productivity (hourly wage) before job loss, this result should not originate from demand effects. Note that, empirically, a high level of skills does not accelerate job finding anyways (Cairó and Cajner, 2018). We conclude that the impact of job loss on risk aversion is substantial enough to translate into actual behavior, since it increases with the probability to accept a job offer sooner.

3.7 Conclusion

Our empirical investigation reveals that the event of a job loss temporarily reduces workers' self-reported willingness to take risks. While the immediate loss of income, emotions or parallel life events cannot explain this finding, lowered and less certain future incomes may play a key part. This becomes most apparent by the decline in risk tolerance that we observe before job loss when neither monetary nor non-monetary benefits have been lost yet. For objectively justified reasons, job loss may increase the perception of future income risks for some time and reduce the willingness to take risks accordingly.

We complement the literature on the effects of unemployment by adding another repercussion of job loss, namely increasing risk aversion. Our results differ from previous inquiries into the impact of unemployment on risk attitude, which do not identify an effect, or find the opposite pattern for long-term unemployed people. We suppose that this is due to different empirical strategies. Exploiting data on plant closures, we are able to circumvent many issues of selection that concerned those previous studies.

Theoretically, our findings are in line with standard economic theory about the role of endowment for absolute risk aversion. The key assumption in this respect is decreasing absolute risk aversion. In addition, our theoretical line of argument neglects the possibiliy that the risk preference parameter in the utility function changes in response to a job loss. Hence, this alternative explanation for our results cannot be ultimately ruled out. We nevertheless regard a change in local risk preference as the more likely explanation, since the results fit the corresponding theoretical considerations, which are derived from plausible assumptions. Following this view, future research on the determinants, or on the effects of risk attitude, may consider that the general risk attitude might measure absolute risk aversion rather than an underlying preference parameter only.

Irrespectively of whether our findings reflect a change in underlying preference or local preference, they imply, in line with many previous studies, that risk attitude is not stable over time and can thus not be assumed to be exogenous in empirical research. Similar to other events, the impact of job loss is thereby of a transitory nature only. Hence, people may have a time-invariant level of risk attitude from which they deviate under certain circumstances, but leap back to soon after (Schildberg-Hörisch, 2018).

Another implication of our findings is that a recession might reinforce itself due to its impact on risk aversion. People who lose work become more risk-averse and therefore less likely to mitigate the consequences of the recession by switching occupations, becoming self-employed or investing in risky projects. This implication is amplified by the fact that people who are about to lose work already become more risk-averse. If this result extends to increasing job insecurity in general, even those people who do not eventually lose work, but increasingly have to fear that event during a recession, might avoid risks more often. One might therefore speculate whether counter-cyclical risk tolerance is mediated by job insecurity (Cohn *et al.*, 2015; Bucciol and Miniaci, 2015, 2018).

Moreover, our results shed light on the influence of public assistance for unemployed workers when it comes to risk-taking. Recall that the immediate income loss does not explain the effect of job loss on stated risk tolerance, and that people with low future income expectations do not show the effect. Both results suggest that the current level of the German public unemployment insurance, together with other income sources, covers the immediate income loss fairly well, in line with the idea of a welfare state as a means to encourage risk-taking (Sinn, 1995; Bird, 2001). However, even the generous German welfare state cannot cover the losses of future incomes that, in particular, highly productive employees are afraid of. It is therefore not surprising that these people, whose risk aversion increases the most strongly in the wake of a job loss, are also the first to accept a job offer to avoid the risk of long-term unemployment (cf. Feinberg, 1977). They rather take the bird in the hand than search for the two in the bush.

3.A Appendix: supplementary tables and figures

Table 3.A1: Effect of job loss on GRA by socio-demographic subgroups and employment status after job loss

Subgroup	(1) Age ≤ 44	(2) Age > 44	(3) Women	(4) Men	(5) Employed in $t = 0$	(6) Not employed in $t = 0$
Job loss between $t = -1$ and $t = 0$	-0.651***	-0.258	-0.422*	-0.510**	-0.582***	-0.349
	(0.207)	(0.227)	(0.242)	(0.198)	(0.200)	(0.236)
Constant	0.271***	0.231**	0.239**	0.308***	0.254***	0.251***
	(0.087)	(0.090)	(0.094)	(0.075)	(0.062)	(0.062)
Controls	yes	yes	yes	yes	yes	yes
Number of treated	104	99	77	126	109	94
Observations	13,211	11,898	9,919	15,190	25,015	25,000
Adj. R-squared	0.026	0.024	0.026	0.024	0.025	0.025

Source: SOEP 2004-2014, INKAR 2004-2012.
Note: The table presents OLS estimates of the change in GRA between $t = -2$ and $t = 0$ for several subgroups. Robust standard errors in parentheses, *p<0.1, **p<0.05 and ***p<0.01. The control variables are specified as in Table 3.2, Column (2). Education level measured on ISCED scale. Subgroups separated at sample mean in $t = -2$, i.e. 44 years of age.

Table 3.A2: OLS estimations of anticipation and reversion of a job loss on GRA

ΔGRA between	(1) $t=-3$ and $t=-2$	(2) $t=-2$ and $t=-1$	(3) $t=-1$ and $t=0$	(4) $t=0$ and $t=1$
Job loss between $t=-1$ and $t=0$	0.030 (0.235)	-0.464** (0.202)	0.005 (0.183)	0.457*** (0.167)
Pre-treatment socio-demographics				
Age in years	-0.002 (0.003)	0.001 (0.002)	0.003 (0.002)	-0.001 (0.002)
Monthly HH income (log)	-0.042 (0.055)	0.017 (0.047)	0.000 (0.046)	0.028 (0.049)
ISCED level (ref. level 3)				
Level 1	-0.001 (0.416)	0.080 (0.372)	0.268 (0.326)	-0.334 (0.327)
Level 2	0.036 (0.091)	0.046 (0.075)	0.005 (0.073)	0.021 (0.077)
Level 4	0.002 (0.066)	0.066 (0.056)	-0.042 (0.055)	0.059 (0.059)
Level 5	-0.005 (0.069)	0.031 (0.057)	-0.089 (0.056)	0.050 (0.059)
Level 6	-0.015 (0.056)	0.061 (0.047)	-0.061 (0.046)	-0.004 (0.048)
Years of unemployment	-0.007 (0.017)	0.013 (0.015)	0.007 (0.014)	-0.012 (0.015)
Local unemployment rate (%)	0.008 (0.009)	0.001 (0.007)	0.006 (0.007)	-0.007 (0.007)
Male	-0.014 (0.048)	0.030 (0.041)	0.045 (0.040)	-0.009 (0.043)
Children in household: yes	-0.021 (0.046)	0.024 (0.039)	0.040 (0.038)	-0.048 (0.040)
Married	-0.033 (0.044)	0.029 (0.038)	0.003 (0.037)	0.030 (0.039)
Migration background	-0.098 (0.078)	-0.003 (0.065)	0.037 (0.062)	-0.107 (0.065)
East Germany	-0.033 (0.062)	0.029 (0.053)	0.002 (0.053)	0.090 (0.058)
Pre-treatment job characteristics				
Net hourly wage (Euros)	0.001 (0.004)	-0.003 (0.004)	0.001 (0.003)	-0.002 (0.004)
Tenure in years	-0.000 (0.002)	0.001 (0.002)	0.001 (0.002)	-0.000 (0.002)
Occupational autonomy (ref. level 3)				
Level 1	0.013 (0.078)	0.012 (0.066)	-0.076 (0.064)	0.037 (0.067)
Level 2	0.015 (0.052)	0.032 (0.044)	-0.041 (0.042)	-0.068 (0.045)
Level 4	-0.008 (0.056)	-0.054 (0.046)	0.031 (0.046)	0.008 (0.048)
Level 5	0.033 (0.118)	-0.043 (0.096)	-0.147 (0.096)	0.200** (0.098)
Company size (ref. 20 to 200 emp.)				
up to 20 employees	0.035 (0.052)	-0.016 (0.044)	-0.001 (0.043)	0.028 (0.045)
more than 200 employees	0.040 (0.044)	-0.019 (0.038)	-0.004 (0.037)	0.025 (0.039)
Weekly working hours	0.002 (0.003)	-0.003 (0.002)	0.002 (0.002)	-0.002 (0.002)
Part-time contract	0.084 (0.072)	-0.055 (0.061)	0.034 (0.061)	-0.035 (0.064)
Sector of industry (ref. manufact.)				
Extraction, exploitation	0.035 (0.089)	-0.044 (0.078)	0.001 (0.076)	0.000 (0.081)
Construction	0.100 (0.077)	-0.035 (0.067)	0.074 (0.066)	0.043 (0.069)
Trade, transport	-0.121 (0.077)	-0.042 (0.069)	-0.038 (0.066)	-0.133* (0.069)
Services	0.054 (0.063)	0.007 (0.052)	0.050 (0.050)	0.018 (0.052)
Media, finance, real estate	-0.056 (0.056)	-0.068 (0.047)	-0.042 (0.046)	-0.002 (0.048)
Administration, education health	-0.015 (0.063)	-0.010 (0.054)	0.053 (0.052)	-0.010 (0.057)
Constant	0.489*** (0.080)	0.127* (0.069)	0.148** (0.067)	-0.422*** (0.071)
Year dummies (ref. 2012)	yes	yes	yes	yes
Number of treated	86	135	135	130
Observations	12,334	17,093	17,093	15,692
Adj. R-squared	0.033	0.033	0.021	0.035

Source: SOEP 2009-2014, INKAR 2009-2012.
Note: The table presents OLS estimates of the change in GRA between the points in time mentioned in the header. Robust standard errors in parentheses, $^{*}p<0.1$, $^{**}p<0.05$ and $^{***}p<0.01$. Pre-treatment socio-demographics and job characteristics measured in $t=-2$ in all presented estimations. Continuous variables are mean-centered.

Table 3.A3: Model modifications

	(1) OLS	(2) Entropy Balancing	(3) Ordered Probit
Job loss between $t = -1$ and $t = 0$	-0.474***	-0.479***	-0.236***
	(0.154)	(0.157)	(0.072)
Constant	0.253***	0.090***	–
	(0.062)	(0.017)	
Controls	yes	yes	yes
Observations	25,109	25,109	25,109
Adj. / Pseudo R-squared	0.025	0.012	0.006

Source: SOEP 2004-2014, INKAR 2004-2012.
*Note: The table presents estimates of the change in GRA between $t = -2$ and $t = 0$, obtained from different estimation techniques as described by the table header. Robust standard errors in parentheses, *p<0.1, **p<0.05 and ***p<0.01. The control variables are specified as in Table 3.2, Column (2). Entropy Balancing considers all control variables as matching variables in the re-weighting procedure. Column (3) presents regression coefficients, not marginal effects.*

Table 3.A4: Probit estimates on probability of future job loss by plant closure

Dependent variable	(1) Plant closure between $t=-1$ and $t=0$		(2) Dismissed by employer between $t=-1$ and $t=0$	
GRA at $t=-2$	0.017	(0.013)	0.014	(0.009)
Pre-treatment socio-demographics				
Age in years	0.004	(0.003)	0.014***	(0.003)
Monthly HH income (log)	-0.078	(0.062)	-0.159***	(0.057)
ISCED level (ref. level 3)				
Level 1	0.149	(0.259)	0.127	(0.207)
Level 2	0.134	(0.092)	0.038	(0.078)
Level 4	-0.037	(0.094)	0.007	(0.086)
Level 5	0.021	(0.091)	0.113	(0.083)
Level 6	-0.039	(0.078)	0.007	(0.067)
Years of unemployment	-0.005	(0.020)	0.036***	(0.014)
Local unemployment rate (%)	0.020*	(0.011)	0.015**	(0.006)
Male	0.023	(0.074)	0.010	(0.058)
Children in household: yes	-0.069	(0.058)	0.015	(0.049)
Married	0.054	(0.071)	-0.094*	(0.049)
Migration background	0.084	(0.079)	-0.147*	(0.077)
East Germany	-0.096	(0.078)	-0.076	(0.065)
Pre-treatment job characteristics				
Net hourly wage (Euros)	0.006	(0.005)	-0.016*	(0.009)
Tenure in years	-0.001	(0.003)	-0.027***	(0.003)
Occupational autonomy (ref. level 3)				
Level 1	0.010	(0.083)	0.055	(0.070)
Level 2	0.059	(0.058)	-0.002	(0.056)
Level 4	0.028	(0.075)	-0.137*	(0.074)
Level 5	-0.300	(0.224)	-0.481**	(0.189)
Company size (ref. 20 to 200 emp.)				
Up to 20 employees	0.040	(0.050)	0.232***	(0.049)
More than 200 employees	-0.083	(0.059)	-0.131**	(0.057)
Weekly working hours	-0.003	(0.004)	-0.002	(0.003)
Part-time contract	-0.065	(0.102)	-0.060	(0.070)
Sector of industry (ref. manufacturing)				
Extraction, exploitation	-0.140	(0.152)	-0.021	(0.110)
Construction	0.110	(0.126)	0.140**	(0.060)
Trade, transport	0.093	(0.110)	0.045	(0.067)
Services	0.302***	(0.077)	-0.092*	(0.056)
Media, Finance, real estate	0.072	(0.083)	-0.041	(0.052)
Administration, education health	-0.229**	(0.115)	-0.258***	(0.069)
Constant	-2.555***	(0.124)	-2.052***	(0.090)
Year dummies (ref. 2012)	yes		yes	
Observations	25,109		25,481	
Pseudo R-squared	0.032		0.091	

Source: SOEP 2004-2014, INKAR 2004-2012.
*Note: The table presents probit estimates of the propensity to experience a job loss in one to two years' time. Robust standard errors in parentheses, *p<0.1, **p<0.05 and ***p<0.01. Continuous variables are mean-centered. Pre-treatment socio-demographics and job characteristics measured in* $t=-2$ *in all presented estimations.*

Table 3.A5: Effect of job loss on GRA by pre-treatment job characteristics, job search and any kind of dismissal by employer

Subgroup	(1) Firm of five to 2,000 employees	(2) Non-service sector employees	(3) Service sector employees	(4) No job-to-job transition after job loss	(5) Including any dismissal by employer
Job loss between $t = -1$ and $t = 0$	-0.504**	-0.440*	-0.489**	-0.437***	-0.015
	(0.202)	(0.256)	(0.206)	(0.165)	(0.086)
Constant	0.229***	0.336***	0.074	0.240***	0.242***
	(0.071)	(0.091)	(0.096)	(0.063)	(0.061)
Controls	yes	yes	yes	yes	yes
Number of treated	144	88	105	178	687
Observations	17,480	12,163	9,798	25,084	25,593
Adj. R-squared	0.025	0.025	0.026	0.025	0.025

Source: SOEP 2004-2014, INKAR 2004-2012.
*Note: The table presents OLS estimates of the change in GRA between $t = -2$ and $t = 0$. Robust standard errors in parentheses, *p<0.1, **p<0.05 and ***p<0.01. The control variables are specified as in Table 3.2, Column (2).*

Chapter 4

Biased by success and failure: how unemployment shapes locus of control

This chapter is published as Preuss, Malte and Juliane Hennecke (2018): Biased by success and failure: how unemployment shapes locus of control, *Labour Economics* 53, p. 63–74, DOI: 10.1016/j.labeco.2018.05.007.

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English short summary

Economic preferences and personality traits are fundamental explanatory factors in understanding individual decision-making. They explain the heterogeneity within human behavior and are the reason why individuals differ in their actions although the preliminaries are the same. Labor market behavior, educational choices, investment decisions as well as fertility and health outcomes are only a few examples in which inherent characteristics play a key role. These findings rely on one joint assumption: preferences and personality traits do not change across the working age. The point in time when preferences are defined and measured is thus irrelevant. However, if this assumption is violated, theoretical models and empirical studies face the threat of endogeneity biases: preferences do not only affect life's outcomes, life's outcomes may also affect preferences. Testing the exogeneity assumption is thus obligatory. Herein, the present thesis makes its contribution and presents three different studies on the stability of economic preferences and personality traits.

The first study in this thesis focuses on the stability of time preferences. So far, evidence on their stability is scarce and considerably restricted by very short time frames, very small sample sizes, or both. The Dutch Household Survey enables these obstacles to be circumvented and the long-term stability of time preferences within a representative sample to be analyzed. By relying on the 'consideration of future consequences' scale – a behaviorally validated survey measure on time preferences – this thesis finds that time preferences have, compared to other economic attitudes, a relatively low intra-individual stability. However, the analysis reveals that individuals' valuation of future utility neither varies with age nor changes persistently with past life experiences. Similar findings result from a replication of the analysis with the German Socio-Economic Panel and its ultra-short survey items on patience and impulsiveness. The thesis, therefore, comes to the conclusion that time preferences are stable in the long run but subjected to measurement issues.

The second study focuses on the determinants of risk-taking. Using German panel data, we find that people become more risk-averse when losing work. The immediate income loss does not mediate this effect. Risk aversion also seems unrelated to the loss of non-monetary benefits of work. However, the study finds that risk aversion responds more strongly to losing work the more future income is at stake, and the effect manifests itself on the eve of job loss even when people have not yet suffered from the consequences of the event. Lower future income expectations and more uncertainty about future incomes may thus explain the effect of job loss on risk attitude. Nevertheless, the effect is not persistent. After some time, individuals turn back to their initial level of risk attitude.

The last chapter of this thesis tests the stability of locus of control, a measure that depicts how much people believe in their ability to affect life outcomes. Using the German Socio-Economic Panel, we find that a job loss due to a plant closure has no long-lasting effect on locus of control. The common assumption of its stability is thus not rejected. However, during unemployment, control perception decreases significantly. The effect holds true independent from unemployment duration or socio-demographic characteristics and vanishes as soon as the unemployed find a new job. In conclusion, measurement of locus of control is affected by unemployment but not the trait itself. Using this trait as the explanatory variable can thus lead to biased estimations if this temporary deviation in measurement is not accounted for.

In conclusion, the present thesis neither rejects the stability assumption nor claims that preferences or personality are perfectly stable. All measures analyzed change with time. But, interpreting this instability as proof of endogenous preferences or personality traits appears unjustified. Each of the studies proposes alternative, less controversial interpretations of instability.

Deutsche Zusammenfassung

Ökonomische Präferenzen und Persönlichkeitsmerkmale sind zentrale erklärende Variablen wenn individuelle Entscheidungen betrachtet werden. Sie sind ursächlich für die Heterogenität im menschlichen Verhalten und Grund dafür, warum sich Individuen in ihren Handlungen voneinander unterscheiden, obwohl sie gleichen Voraussetzungen gegenüberstehen. Entscheidungen im Berufsleben, zum Bildungsweg, zu Investitionen, zur Familienbildung oder zum Gesundheitsverhalten sind nur einige Beispiele, bei denen individuelle Vorlieben und Eigenschaften eine zentrale Rolle spielen. Diesen Ergebnissen liegt jedoch die Annahme zugrunde, dass Präferenzen und Persönlichkeit über das Erwerbsalter hinweg stabil sind. Dadurch ist es nämlich irrelevant, zu welchem Zeitpunkt die individuellen Merkmale definiert und gemessen werden. Wenn diese Annahme jedoch verletzt ist, werden sowohl theoretische Modelle als auch ökonometrische Schätzungen durch Endogenität bedroht. Denn dann sind Handlungen nicht nur eine Konsequenz von Präferenzen, sie könnten Präferenzen in gleicher Weise formen. Wegen diesen tiefgreifenden Implikationen sind detaillierte Tests zur Exogenitätsannahme notwendig und verpflichtend. Hier leistet die vorliegende Dissertation ihren Beitrag und präsentiert drei verschiedene Studien zu der Stabilität von Präferenzen und Persönlichkeitsmerkmalen.

Die erste Studie fokussiert sich auf Zeitpräferenzen und deren Stabilität. Bisher ist die Evidenz in diesem Feld sehr begrenzt und häufig durch sehr kleine Stichproben oder sehr kleine Zeitrahmen limitiert. Mit Hilfe einer niederländischen Haushaltsbefragung kann die vorliegende Dissertation die Einschränkungen der vorherigen Literatur vermeiden und die langfristige Stabilität von Zeitpräferenzen in einer repräsentativen Stichprobe analysieren. Unter Verwendung der „Consideration of future consequences“-Skala – einem verhaltensvalidierten Umfrageinstrument zu Zeitpräferenzen – zeigt die Studie, dass Zeitpräferenzen im Vergleich zu anderen Persönlichkeitsmerkmalen oder Präferenzen eine relativ hohe Instabilität aufweisen. Allerdings lässt sich diese Instabilität nicht auf spezifische Ereignisse zurückführen. Weder das Alter noch drastische Lebensereignisse üben einen persistenten Effekt aus. Ähnliche Ergebnisse lassen sich mit Hilfe des deutschen Sozio-ökonomischen Panels und seinen zwei ultra-kurzen Fragen zu Zeitpräferenzen finden. Die Studie schlussfolgert deshalb, dass Zeitpräfenzen in der langen Frist stabil sind, allerdings durch Messfehler verzerrt werden.

In ihrer zweiten Studie betrachtet die vorliegende Dissertation die Determinanten von Risikoverhalten. Unter Verwendung deutscher Panel-Daten zeigt sie, dass Personen risikoaverser werden, sobald sie ihren Arbeitsplatz verlieren. Allerdings erklärt weder der damit einhergehende Einkommensverlust noch der Verlust nicht-monetärer Vorteile aus Arbeit diese Beobachtung. Jedoch nimmt der Effekt eines Arbeitsplatzverlusts zu, je mehr zukünftiges Einkommen auf dem Spiel steht. Zudem ändern Individuen bereits vor dem eigentlichen Arbeitsplatzverlust ihre Risikobereitschaft, obwohl die Konsequenzen noch gar nicht eingetreten sein können. Unsicherheit über die Zukunft wird daher als Ursache identifiziert, weshalb Personen ihre Risikobereitschaft durch einen Arbeitsplatzverlust ändern. Der Effekt ist jedoch nicht persistent. Nach einiger Zeit kehren die Individuen zu ihrer ursprüngliche Risikobereitschaft zurück.

Das letzte Kapitel dieser Dissertation widmet sich der Stabilität von Kontrollüberzeugung. Dieses Persönlichkeitsmerkmal spiegelt die Wahrnehmung von Individuen wider, ob sie ihr Leben selbst steuern und ihren Erfolg beeinflussen können. Unter Verwendung deutscher Panel-Daten zeigt die Studie, dass ein unfreiwilliger Arbeitsplatzverlust im Durchschnitt keinen Effekt auf die Zielvariable hat. Die Exogenitätsannahme wird also nicht verworfen. Allerdings zeigt sich während der Arbeitslosigkeit ein signifikanter Effekt. Dieser ist sowohl von der Dauer der Arbeitslosigkeit als auch von verschiedenen sozio-demographischen Variablen unabhängig. Zudem verschwindet er, sobald die Person in ein Beschäftigungsverhältnis zurückkehrt. Die Studie kommt deshalb zu dem Fazit, dass lediglich die Messung von Kontrollüberzeugung durch Arbeitslosigkeit verzerrt wird. Es kann daher zu Schätzfehlern kommen, sofern für die temporäre Abweichung nicht korrekt berücksichtigt wird.

Zusammenfassend verwirft diese Dissertation die Stabilitätsannahme nicht. Sie argumentiert allerdings ebenso wenig, dass Präferenzen oder Persönlichkeitsmerkmale absolut stabil sind. Alle analysierten Maße verändern sich im Erwerbsalter. Diese Veränderungen als Endogenität zu interpretieren, würde jedoch zu weit greifen. Dafür stehen stets alternative, weniger invasive Erklärungen zur Verfügung, die zuerst angewandt werden können. Allerdings wird ebenso deutlich, dass die Verwendung von Präferenzen und Persönlichkeitsmerkmalen in der empirischen Wirtschaftsforschung sehr fehleranfällig ist.

Vorveröffentlichungen

Die folgende Liste enthält alle Vorveröffentlichungen. Darunter sind auch Versionen der Kapitel, die zum Teil stark überarbeitet wurden, bevor sie Eingang in die vorliegende Dissertation fanden. Zudem wurde Kapitel 4 im Jahr 2017 unter einem anderen Titel veröffentlicht (Biased by success and failure: how unemployment shapes stated locus of control). Kapitel 1 und 2 wurden vorab nicht veröffentlicht.

Kapitel 3: Income in jeopardy: how losing employment affects the willingness to take risks (mit Clemens Hetschko)

- FU Berlin School of Business & Economics Discussion Paper No. 2015/32
- SOEP Discussion Paper No. 813 (2015)

Kapitel 4: Biased by success and failure: how unemployment shapes locus of control (mit Juliane Hennecke)

- Labour Economics Vol. 53 (2018), S. 63–74, DOI: 10.1016/j.labeco.2018.05.007
- FU Berlin School of Business & Economics Discussion Paper No. 2017/29
- SOEP Discussion Paper No. 943 (2017)