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Time preferences and political regimes: Evidence from reunified Germany

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Abstract

We use the separation and later reunification of Germany after World War II to show that a political regime shapes time preferences of its residents. Using two identification strategies, we find that former residents of the *German Democratic Republic* exhibit a significantly less pronounced present bias when compared to former residents of the *Federal Republic of Germany*, whereas measures of patience are statistically indistinguishable. Interpreting the years spent under the regime as a proxy for treatment intensity yields consistent results. Moreover, we present evidence showing that present bias predicts choices in the domains of health, finance, and education, thereby illustrating lasting repercussions of a regime's influence on time preferences.

Keywords: Time preferences; Political regime; Germany; Natural experiment; SOEP

JEL: D02, D12

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1 Introduction

Time preferences are crucial to almost all major life choices such as saving for the future, educational decisions, and both labor market and health-related behavior (e.g., DellaVigna and Paserman 2005, Falk et al. forthcoming, Golsteyn et al. 2014, Koch et al. 2015, Meier and Sprenger 2010, Sutter et al. 2013). At a national level, the time preferences of a country's residents are a key driving factor of domestic economic development (Dohmen et al. 2018). As a result, a variation in time preferences will have very important implications at both the individual and societal level.

This paper explores whether the socialist regime of the German Democratic Republic (GDR) has caused a lasting variation in time preferences of its former residents. We use survey measures from the German Socio-Economic Panel (SOEP) that are significantly correlated with time preference information from incentivized experiments. To identify the time preference effect of the GDR's political regime, we use two identification strategies. First, following the standard approach in the literature (e.g., Alesina and Fuchs-Schündeln 2007, Dragone and Ziebarth 2017), we treat the reunification after more than four decades of separation as a natural experiment and present findings from ordinary least squares regressions. To strengthen these results, we test for robustness against bias from local unobservables applying the procedure by Oster (forthcoming). Second, we adopt a geographic regression discontinuity design with border-segment fixed effects, thereby addressing concerns about unobserved local heterogeneity by comparing only people who have lived close to the former FRG/GDR border.

Traditionally, economists explain behavior for *fixed* preferences (e.g., Stigler and Becker 1977, Golsteyn and Schildberg-Hörisch 2017). Meier and Sprenger (2015) and Hardardottir (2017) provide evidence in favor of the hypothesis that time preferences are rather stable over the short- and medium-term.[1] In contrast, we propose that the exposure to the GDR's political regime over a very long time horizon influenced its former residents' time preferences in the spirit of the literature arguing that preferences are endogenous to institutions (e.g., Bowles 1998, Fehr and Hoff 2011).

With respect to time preferences, we can, at an individual level, distinguish between *present bias* and *patience* using our experimentally validated survey measures, via reference to the well-established (β, δ) model as a parsimonious representation of time preferences that accounts for the empirically important present bias (e.g., DellaVigna 2009, Laibson 1997, O'Donoghue and Rabin 2015). In the (β, δ) model, the parameter β represents a bias for the present – measuring how *now* is evaluated relative to *later* – whereas parameter δ is the discount factor used in standard exponential discounting. Wang et al. (2016) identify information about time

[1] There is similar evidence, for example, regarding social preferences in Carlsson et al. (2014).

preferences in 53 countries, finding that present bias is a global phenomenon and much more heterogeneous across countries than measures of patience. Similarly, Falk et al. (forthcoming) describe notable variation in preferences within- and between-countries. By highlighting the implications of past institutions on today's time preferences, our analysis seeks to contribute to an explanation of the origins of such heterogeneity in economic preferences.

According to our empirical results, the GDR's socialist regime significantly diminished the present bias of its former citizens in the long run. In contrast, the regime's influence on patience is insignificant.[2] When we split the sample into birth cohorts, we find results that are consistent with the proposition that a long exposure to the socialist regime during the years in which time preferences are particularly malleable was necessary for a notable effect on present bias at the individual level.

Against this background, we emphasize that the GDR's socialist regime continues to shape the lives of its former citizens to this day and will continue to do so into the future. The lasting footprint is due, inter alia, to the intergenerational transmission of time preferences and social norms (e.g., Brosig-Koch et al. 2011, Brown and van der Pol 2015, Chowdhury et al. 2018, Gauly 2017). To assess the importance of our result of regime influence on present bias for economic behavior, we show the predictive power present bias has for personal choices in the health, finance, and education domains.

Our paper is the first to scrutinize the influence of political regimes on time preferences, taking advantage of the German separation and reunification as a treatment variation. The paper is thus related to contributions that search for past institutions' footprints on behavior and studies that similarly make use of the German separation and reunification as a natural experiment.

There is recent literature on the long-term persistence and long-lasting effects of institutions. For example, Becker et al. (2016) rely on the Life in Transition Survey to establish that a historical affiliation with the Habsburg empire increases current levels of trust and reduces corruption in courts and police in Central and Eastern Europe today. In other lines of inquiry, Acemoglu et al. (2001) relate colonization styles to present economic performance, Nunn and Wantchekon (2011) explain present levels of trust in Africa with references to the slave trade, and Voigtländer and Voth (2012) find that pogroms in medieval times predict anti-Semitic violence in Nazi Germany. In comparison to these and related important contributions (see Bisin and Verdier 2011 for a recent survey), our analysis considers a more recent set of institutions, but still considers the possibility of formative institutional impact on individuals.

[2]The difference in the means of patience is smaller than the one for present bias, but in the anticipated direction. The same holds for the signs of the (insignificant) GDR regression coefficient in the estimation for the level of patience. Our data is thus consistent with the relatively greater variability of present bias across countries as reported in Wang et al. (2016).

There are a number of recent contributions using the German separation and reunification to form a better overall understanding of attitudes and economic behavior. For example, Brosig-Koch et al. (2011) present experimental findings indicating that East Germans exhibit less solidarity at the private level than West Germans (thereby revisiting the research question of Ockenfels and Weimann 1999). Consistent with the idea that East Germans perceive the state to be the responsible actor in that domain, Alesina and Fuchs-Schündeln (2007) show that East Germans support redistribution and state intervention more than West Germans. These authors also highlight that the difference will remain for a considerable period of time. These lasting consequences may be attributed to social norms and intergenerational transmission. Necker and Voskort (2014) study intergenerational transmission of attitudes, focusing on East and West Germany and the responses of parents and children to value questions (e.g., whether owning a house is important). Bauernschuster and Rainer (2012), Beblo and Görges (2018), and Campa and Serafinelli (forthcoming) are interested in the long-run impact of regime differences for labor market participation, and Friehe and Mechtel (2014) in potential implications for the relevance of conspicuous consumption. In contrast, the present paper seeks to identify regime repercussions on economic preferences that, in turn, influence many kinds of behavior.

Our paper is closely related to Rainer and Siedler (2009) and Heineck and Süssmuth (2013) who examine differences between East and West Germans regarding the level of trust, which can be interpreted as a measure of social preferences (e.g., Becker et al. 2012). Like our study, their contributions rely on the recent German history for identification of causal effects and the SOEP as one data source.[3] Rainer and Siedler (2009) and Heineck and Süssmuth (2013) find that East Germans show higher levels of social distrust. The latter paper additionally considers risk preferences of former GDR and FRG citizens, and concludes that East Germans are similarly risk averse (after a brief period of lower risk aversion).

Economic preferences and personality traits are complementary when it comes to explaining heterogeneity in life outcomes and behavior (e.g., Humphries and Kosse 2017). Consistent with this, when considering correlations between six personality measures and time preferences in a large SOEP data set, Becker et al. (2012) find only two significant instances: one in which the correlation with conscientiousness is only of small magnitude, and another with agreeableness correlated with a medium magnitude. However, there exist conceptual relationships. For example, Almlund et al. (2011: p. 70) refer to a conceptual relationship between conscientiousness and time preferences. In our robustness checks, we thus seek to test whether the GDR treatment effect also shows up in selected personality traits which are related to time preferences. For this purpose, we build on Friehe et al. (2015) by providing refined ordinary least squares and new geographic discontinuity design results for both conscientiousness and agreeableness

[3]Rainer and Siedler (2009) primarily build on the German General Social Survey (ALLBUS).

in one of our robustness checks. Friehe et al. (2015) analyze how the GDR regime influenced personality traits within an ordinary least squares framework.

The structure of the paper is as follows. Section 2 presents the data. In Section 3, we will explain our research design. Section 4 presents our empirical analysis including robustness checks. Section 5 explores some important implications from our main result. Section 6 concludes.

2 Data

Our analysis is based on the German Socio-Economic Panel (SOEP), a nationally representative longitudinal data set created in 1984. Our working sample contains respondents who were either born in Germany or immigrated before 1949 and have provided valid information about where they lived in 1989 (i.e., the year before reunification). These data selection criteria follow from the treatment of the separation and reunification of East and West Germany as a natural experiment.

Specifically, we use the information on patience and impulsivity from the SOEP waves 2008 and 2013. The question regarding patience asks: "Are you generally an impatient person, or someone who always shows great patience?". Answers are coded on an 11-point scale, with 0 denoting "very impatient" and 10 "very patient". The question concerning impulsivity asks: "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 a long time - in other words, are you very impulsive?". Answers are again coded on an 11-point scale, with 0 denoting "not at all impulsive" and 10 "very impulsive".

Our theoretical reference with respect to time preferences is the (β, δ) model. In that framework, intertemporal preferences from the perspective of period t can be represented by $U^t = u_t + \sum_{\tau=t+1}^{T} \beta\delta^\tau u_\tau$ with δ as the standard discount factor, such that $\beta = 1$ corresponds to exponential discounting and while $\beta \in (0, 1)$ reflects present bias (e.g., O'Donoghue and Rabin 2015).[4] Answers to the question regarding patience represent our proxy of the δ component of the (β, δ) model. The patience information was validated by Vischer et al. (2013) by reference to experimentally elicited information on time preferences. Answers to the question regarding impulsivity represent our proxy of the β component of the (β, δ) model. This interpretation is sensible for three reasons. First, our chosen SOEP impulsivity question matches items in the Barratt impulsiveness scale (Patton et al. 1995) and the Tangney et al. (2004) self-control scale. The terms "present bias" and "self-control issues" are often used synonymously (see,

[4]Present-biased preferences thus give special importance to immediate payoffs and allow for time-inconsistent choice (e.g., O'Donoghue and Rabin 1999).

e.g., DellaVigna 2009). Indeed, since greater self-control leads to a greater congruence between actions an agent would like to undertake and those actually undertaken, it may be interpreted as a less pronounced present bias (e.g., Ameriks et al. 2007). Second, answers to the impulsivity question correlate with experimentally elicited present bias (see Appendix A for details of our analysis).[5] Along the same lines, Burks et al. (2012) demonstrate a significant correlation between experimental measures of impulsivity and present bias conditional on experimental information on patience and a survey measure of impatience. Third, present bias information from answers to the impulsivity question correlates – as it should – with behaviors critically influenced by self-control issues such as smoking (see Section 5). Accordingly, we use the SOEP impulsivity measure as a proxy of present bias in our empirical work. Note, as example, that Fourage et al. (2014) similarly refer to the patience and impulsivity questions in terms of the (β, δ) model.

The covariate of key interest in our empirical analysis is a dummy variable that is equal to one (zero) if the respondent was a resident of the GDR (FRG) in 1989. We include a host of further covariates that may be relevant to the formation of time preferences but are not themselves influenced by the individual's time preferences (see Table 1). The age of the respondent is included (Bishai 2004). Gender is included as a dummy variable equal to one when the respondent is male and zero otherwise, as it has been shown to bear on time preferences (Bishai 2004). Because preferences are very much shaped by one's parents and environment during childhood (e.g., Kosse et al. 2016), we also include information about whether or not the respondent was raised in a small, medium-sized, or large community and about the educational background of the parents (for which we use dummy variables for the highest degrees obtained by the father and the mother).[6]

Table 1 presents summary statistics by treatment status for the (non-standardized) variables used in our empirical analysis. The descriptive results indicate that former GDR and FRG residents differ with respect to some covariates, suggesting adjustments for covariate differences in our empirical specifications. Imbens and Wooldridge (2009) point out that differences in the observable characteristics of the treatment group and the control group might lead to sensitive estimation results in a linear regression framework. They propose that the imbalance of the covariate distributions should be assessed by testing whether or not the scale-free normalized difference between treatment and control group covariate means exceeds 0.25 (as a rule of thumb). The normalized differences in our data are less than 0.25 for all but one covariate in

[5]Pinger (2017) also finds that survey measures and experimental measures of present bias are significantly related. She uses the special experimental SOEP submodul 2006 and a survey measure of present bias that is not available in the main SOEP waves.

[6]Including the educational background of the parents is also important because own cognitive ability relates to time preferences, and is itself strongly influenced by parents (Anger and Heineck 2010, Anger and Schnitzlein 2017, Bishai 2004, Dohmen et al. 2010).

Column (3) of Table 1 (and in this single instance, *Mother: No vocational degree*, the difference is very close to 0.25).

	(1) FRG		(2) GDR		(3) Absolute Value
	Count	Mean	Count	Mean	Normalized Difference
Age	27,784	51.11	11,208	49.95	0.050
Male	27,784	0.467	11,208	0.457	0.015
Raised in a large city	27,784	0.232	11,208	0.195	0.063
Raised in a medium city	27,784	0.169	11,208	0.180	0.019
Raised in a small city	27,784	0.211	11,208	0.242	0.052
Mother: Secondary lower school	27,784	0.648	11,208	0.542	0.154
Mother: Secondary intermediate school	27,784	0.171	11,208	0.273	0.175
Mother: Secondary higher school	27,784	0.0656	11,208	0.0750	0.026
Mother: No vocational degree	27,784	0.365	11,208	0.198	0.267
Mother: Vocational degree	27,784	0.475	11,208	0.561	0.123
Mother: Technical school	27,784	0.00576	11,208	0.0469	0.183
Mother: University degree	27,784	0.0381	11,208	0.0622	0.078
Father: Secondary lower school	27,784	0.611	11,208	0.519	0.131
Father: Secondary intermediate school	27,784	0.128	11,208	0.224	0.179
Father: Secondary higher school	27,784	0.132	11,208	0.111	0.045
Father: No vocational degree	27,784	0.110	11,208	0.0562	0.139
Father: Vocational degree	27,784	0.675	11,208	0.652	0.035
Father: Technical school	27,784	0.0127	11,208	0.0412	0.125
Father: University degree	27,784	0.105	11,208	0.114	0.019

Notes: We use SOEP data from 2008 and 2013. Our working sample contains respondents who were either born in Germany or immigrated before 1949 and have provided valid information about where they lived in 1989 (i.e., the year before reunification). For each covariate, the normalized difference is defined as the difference in averages by treatment status, scaled by the square root of the sum of variances.

Table 1: Descriptive Statistics

3 Research Design

We propose that any differences in the time preferences of former GDR and FRG residents measured after the country's reunification are related to how the two different political regimes *treated* their citizens during the 40-plus years of separation. We pursue two identification strategies to present robust evidence for the hypothesized causality that we explain in some detail in Section 3.2 after we have discussed the malleability of time preferences across a lifespan and explain hypotheses concerning the influence of the regime in this regard in Section 3.1.

3.1 The Formation of Time Preferences and Potential Regime Influence

Time preferences as well as other individual attributes are heavily influenced by parents through genetics, parental investment, and the choice of childhood environments (e.g., Anger and Schnitzlein 2017, Cunha and Heckman 2007, Dohmen et al. 2012). According to the literature on cultural transmission, parents invest into the formation of the values and preferences of their children' in order to maximize welfare (e.g., Bisin and Verdier 2011, Bulte and Horan 2011, Tabellini 2008).[7] Doepke and Zilibotti (2008), for example, focus on parental desire to instill patience. Parents may – at a private cost – choose to instill preferences different from their own when these preferences serve their children better. This signifies that parents raised in a past political regime may choose to induce values and preferences optimally adjusted to the current political regime. In a dynamic context, parents may have different expectations about the evolution of the regime, which would contribute to preference heterogeneity among children. With respect to the intergenerational transmission of time preferences, Chowdhury et al. (2018) find evidence in a developing-country context and Gauly (2017), Kosse and Pfeiffer (2013), and Brenoe and Epper (2018) do so for developed countries. The latter highlight that there is evidence that this transmission is mainly due to parental investment instead of genetics. This supports the cultural transmission approach, which also very much emphasizes that the interaction of individuals with teachers, friends, peers, and role models critically influence the formation of preferences (e.g., Behabib and Bisin 2011, Bisin and Verdier 2011).[8]

The malleability of cognitive and non-cognitive skills is particularly high in early childhood years (e.g., Fehr et al. 2008). For example, Knudsen et al. (2006) refer to evidence from different disciplines including economics and neurobiology. Cunha and Heckman (2007) highlight that most measurable skill gaps open up at early ages and show near parallelism during school years. Deckers et al. (2017) similarly maintain that differences in cognitive and non-cognitive abilities emerge early in life, alluding to critical periods for preference formation.[9] With respect to time preferences, we can refer to evidence presented in Sutter et al. (2013, 2015) and Bettinger and Slonim (2007). Sutter et al. (2013) study how experimentally-elicited time preferences are related to real-life outcomes for a sample of individuals of ages 10 to 18. They find that time preferences are already relatively stable during this time period. In contrast, Sutter et al.

[7] Zumbuehl et al. (2013) and Dohmen et al. (2012), for example, provide strong empirical support for the cultural transmission approach.

[8] Evolutionary selection also plays a role in the formation of preferences but is less relevant for our context. The literature is surveyed by Robson and Samuelson (2011), for example.

[9] Delaney and Doyle (2012) present evidence on the relationship between psychometric measures proxying time preferences and socioeconomic differences that is consistent with the findings reported in Deckers et al. (2017).

(2015) analyze choices of children between the ages of 3 and 6 and find that time preferences are malleable during this period. Consistent with this, Bettinger and Slonim (2007) report that, whereas younger children have higher discount rates than adults, individual discount rates by the age of 16 years are fairly similar to those of adults. However, as holds true for other attributes, time preferences may be subject to change at later points as well. The *impressionable years* hypothesis proposes that core attitudes, beliefs, and values crystallize during a period of great mental plasticity in early adulthood and remain largely unaltered thereafter (Krosnick and Alwin 1989). Giuliano and Spilimbergo (2014) overlay the impressionable years hypothesis to life experience data, focusing on the range 18 to 25 years of age and the repercussions from experiencing a recession during those years. With regard to later changes (i.e., those in middle age), it is usually argued that they lack economic significance (e.g., Cobb-Clark and Schurer 2013). In contrast, changes at old age may be important again (e.g., Specht et al. 2014).

Using our data, we can explore the extent to which our analyzed time preference measures change and whether this differs across age cohorts. In general, in exploring the differences between each subject's responses in 2008 and 2013 for the 65% of the sample for whom we have observations in both years, we find that the median differences are zero, that 23% (54%) of respondents have a difference of zero (of an absolute level less than or equal to one) with regard to impulsivity, and that 25% (57%) of respondents have a difference of zero (of an absolute level less than or equal to one) with respect to patience. To further describe the stability, we follow the approach of Cobb-Clark and Schurer (2013) and consider how the difference between the measurement in 2013 and the one in 2008 at the level of the individual is a function of the individual's age (see Figure 1).[10] Overall, our data is very much consistent with the argumentation presented above. There are meaningful changes in early adulthood – as emphasized by the impressionable years hypothesis, for example – and there tends to be some variability in old age again. However, in middle age, stability is very high, particularly for our present bias measure. When it comes to intra-individual changes as portrayed in Figure 1, Schildberg-Hörisch (2018), for example, argues that it is meaningful to think about economic preferences as a latent trait that is measurable only with some noise induced by contemporaneous emotions.

[10]Non-parametric local mean smoothing is applied to estimate the changes by age.

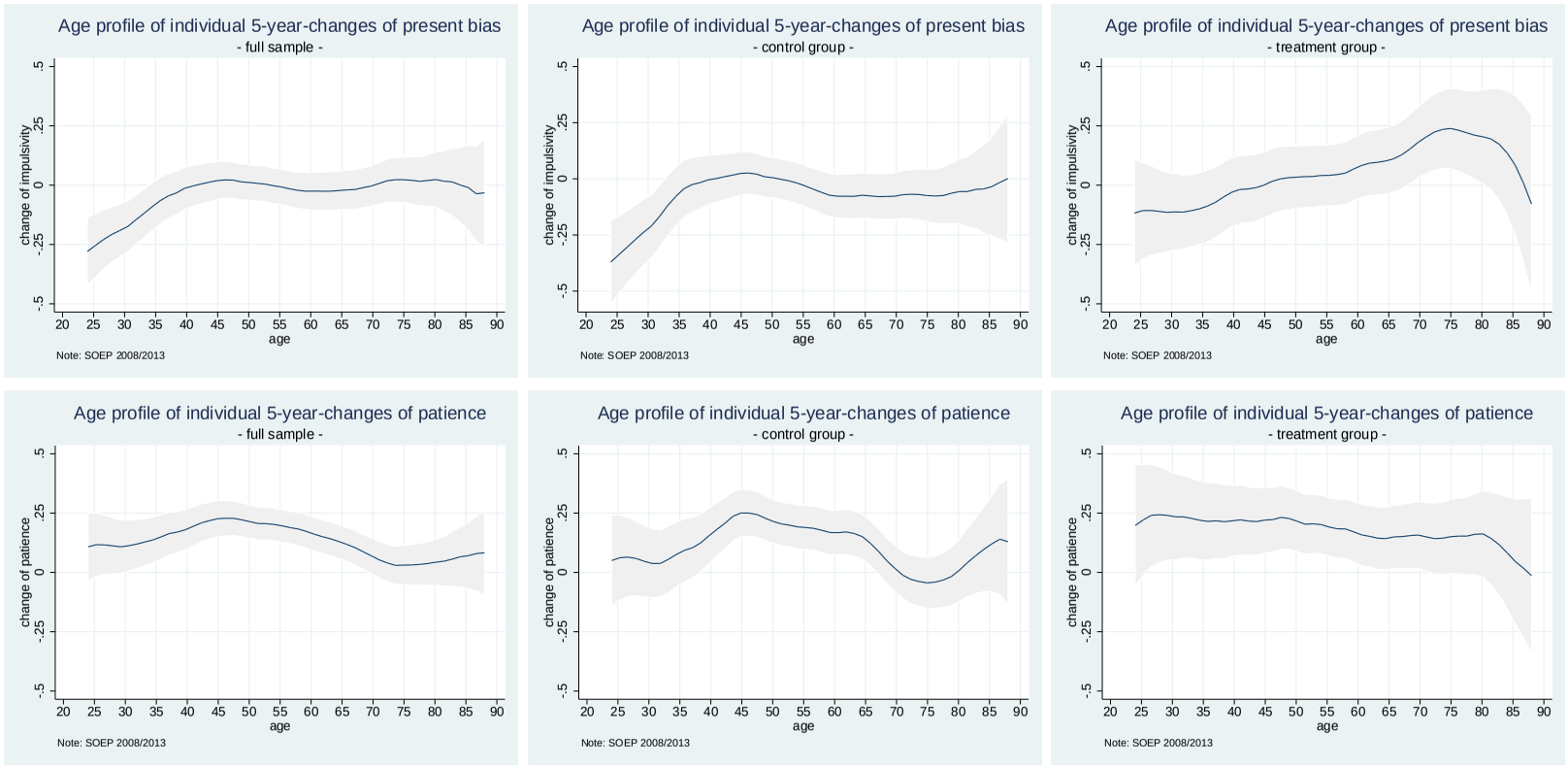

Figure 1: Differences between Time Preference Measure 2013 and 2008 Across Cohorts (95% Confidence Intervals Are Displayed).

Economists have traditionally used exogenous preferences to explain how behavior changes when constraints change. Focusing on patience, Becker and Mulligan (1997) present a model in which time-consistent individuals can invest into their own time preferences to overcome impatience as a potential human fallibility, where the investment involves time and effort spent on active appreciation of future pleasures.[11] A high investment in patience is more likely the lower current marginal utility of wealth (as investment implies giving up current consumption) and the higher future utility of future wealth (as greater patience makes future utilities more salient in the present). Their setup uses the exponential discounting model such that introducing a potential present bias that can also be influenced by investment moves it closer to our setup. Endogenous present bias as an additional channel for the decision-maker is very realistic due to evidence that people can improve their self-control by investment in conscientious behavior (e.g., Baumeister and Tierney 2012, Mischel 2014, Muraven 2012). Importantly, it adds a strategic rationale for investment to the discussion by Becker and Mulligan (1997): An individual sophisticated about the present bias may invest to improve upon future decisions made at a later point in life.

Against this background, we now conjecture a possible regime influence on time preferences. With the regime's astute consideration of the early life development of preferences and values, political education in the GDR started in day-care centers and was very important in school. The ultimate goal of the GDR's political education was creating socialist citizens characterized by a high sense of duty and self-discipline (e.g. Litz 2007). The regime idealized a forward-looking perspective. For example, Walter Ulbricht announced *ten commandments* during the fifth SED party congress in June 1958 including the admonishment that people should live sparingly and strive for improvement of their performance in the future. This indoctrination thus inculcates greater patience and a less pronounced present bias of former GDR residents. The GDR's massive state-security service, which relied extensively on both a network of unofficial collaborators and extreme forms of repression, strongly incentivized GDR residents to control their impulses. Indeed, people were on constant alert regarding statements they could make and actions they could undertake without endangering their personal freedom or their physical wellbeing (e.g., Bruce 2010, Fulbrook 2005). The cultural transmission model would thus predict that parents go to extreme lengths to instill self-control in their children because it was of overriding importance for life in the GDR. This accords with the incentives to invest in own preferences as analyzed and presented by Becker and Mulligan (1997). With respect to patience, GDR citizens had to cope with shortages in a myriad of life domains including housing, vacations, and consumption goods. The model presented by Becker and Mulligan (1997)

[11] In this context, it is interesting that Chen (2013) documents that people who speak languages that do not delineate the future tense, and thus allowing "tomorrow" to be a continuation of "today" instead of forcing it to be a new day altogether – are more patient.

explicitly accounts for the possibility that GDR residents put forth more effort to increase their appreciation of the future in order to optimally respond to the shortages and delivery times. The cultural transmission model would similarly inform a higher relative expectation of patience among former GDR residents as a result of parental investment decisions. On a different note, when comparing the political regimes in the GDR and the FRG, it unambiguously results that the GDR ensured more stable, less heterogeneous economic positions of individuals. This resulted because, among other factors, the GDR featured no official unemployment and a very compressed wage distribution.[12] Sozou (1998) and Dasgupta and Maskin (2005) provide theoretical setups showing that uncertainty about either the occurrence or the precise timing of payoffs in the future can produce preferences consistent with present bias. The *relative* absence of notable uncertainty regarding the individual economic position in the GDR thus suggests that the GDR's political regime may have lowered the present bias of its former residents *relative* to that of FRG residents.

In summary, there is evidence that points to malleability of preferences particularly in early life. Moreover, we find that different approaches lead to the hypothesis that former GDR residents are characterized by a less pronounced present bias and greater patience when compared to former FRG residents. Next, we address how we test these hypotheses with the data available.

3.2 Identification Strategy: The German Separation and Reunification as a Natural Experiment

Our identification strategies rely on treating the German separation into the FRG and the GDR as a *natural experiment*. Fuchs-Schündeln and Hassan (2016) forcefully argue that the separation of Germany was exogenous to the preferences of the affected populations and the economic conditions in East and West Germany at the time. Ideally, the following conditions are fulfilled: (i) the treatment was exogenous, (ii) East and West Germans (i.e., the treated and the non-treated) were comparable when the treatment was imposed, (iii) there was no selective migration throughout the treatment period, and (iv) there was *trackable* migration after the reunification. We will now address these issues in turn.

Imposition of the Treatment The imposition of the political regimes was truly exogenous. The geographical division of Germany was a result of the agreement between the United Kingdom, the United States, and the Soviet Union on the partition of postwar Germany in 1944,

[12]The political regime of the GDR vigorously highlighted differences between the FRG and the GDR with respect to unemployment, for example. The salience of such differences for its residents was aided by the use of propaganda in school books or shows on television (e.g., Saunders 2007).

which split the territory into three sectors of roughly equal population size.[13] Redding and Sturm (2008) highlight that the decisions determining the partition of East and West Germany are unlikely to be correlated with the prewar characteristics of respective regions. Moreover, Wolf (2009) states that by the end of the Weimar Republic in 1933, Germany was an economically well-integrated area, such that the separation into East and West Germany that existed between about 1946 and 1989 could not have been predicted in 1939.

Comparability of *East Germans* and *West Germans* Before the Treatment A key identifying assumption is that former GDR and FRG residents did not differ from each other with regard to time preferences prior to the German separation. While there is no representative data on this issue, there is documented similarity between East and West Germans with respect to a host of other variables prior to the separation. Alesina and Fuchs-Schündeln (2007) explain that the regions that became the FRG and the GDR were similar in terms of pre-World War II average per-capita income levels and the amount of destruction experienced during World War II. East and West Germany were also similar in terms of the proportions of the working population involved in various industries (Schäfgen 1998), the political orientations of voters at the turn of the century (Alesina and Fuchs-Schündeln 2007), and population densities (Hubert 1998). Comparability with respect to per-capita income levels and the amount of war-related destruction is important for our design, for example, as there is some evidence that the availability of financial resources impacts intertemporal decision-making (e.g., Carvalho et al. 2016). Similarly, the split of the working population may be relevant to time preferences as Nguyen (2011) finds some relationship between time preferences and occupations. Based on such evidence, Fuchs-Schündeln and Hassan (2016), among others, conclude that the two parts of Germany were *indistinguishable* prior to the separation.

Selective Migration During the Separation Period After the two political regimes had been imposed, around three million people emigrated from the GDR to the FRG before the Berlin Wall was built in August 1961 (e.g., Heidemeyer 1994, Hubert 1998). There was very little migration from the GDR to the FRG after 1961 or from West to East Germany. Intellectuals and entrepreneurs were overrepresented among the sample of migrants (e.g., Heidemeyer 1994).[14] Migration during our treatment period may confound our interpretation of results

[13] Later on, the US and the UK handed over small parts of their sectors to France (e.g., Burchardi and Hassan 2013).

[14] However, Schäfgen (1998: 58), for instance, asserts that the structure of society with respect to educational achievement and implied differences between social groups remained relatively stable and comparable in East and West Germany.

when migrants' time preferences were systematically different from those of non-migrants.[15] When implementing the first identification strategy, we include information about an individual's parents, in an attempt to address potential selection effects arising from migration after the imposition of the GDR's socialist regime but before the erection of the Berlin Wall. For a subsample of the data, we can actually account for individuals who migrated to West Germany during the treatment period and find results consistent with our main results.

Migration After the Reunification After the fall of the GDR's socialist regime, we have full data with regard to migration between East and West Germany.[16] In particular, our data set allows us to differentiate between East Germans who lived in the GDR in 1989 and continue to live in that region of Germany today from those who lived in the GDR in 1989 but moved to the west before their participation in the SOEP survey. Similarly, we can track movements from West Germany to East Germany.

In summary, we believe that the identifying assumption that differences in time preferences were shaped by the respective experiences of populations under the two political regimes (paralleling the approach taken by Alesina and Fuchs-Schündeln (2007) and others) seems justified.

3.2.1 Ordinary Least Squares Regressions and Test of Selection on Unobservables

Based on our discussion above, we simply compare former GDR with former FRG residents using ordinary least squares regressions with (arguably) exogenous covariates as our first identification strategy. The covariates used in our regression exercises are summarized in Table 1. Note that including information pertaining to the survey respondent's parents allows us to control for the characteristics of the individuals responsible for a potential migration decision from East to West during the years of the German separation. Nevertheless, one might still be concerned that our results may be prone to bias from local unobservables, for example, since we do not have information on the distribution of time preferences before the separation of Germany. Therefore, we bolster our results from ordinary least squares regressions by testing against omitted variables bias using the procedure suggested by Oster (forthcoming).

3.2.2 Geographic Regression Discontinuity Design

In our setting, the GDR treatment changes discontinuously at the former FRG/GDR border. The basic idea of using a geographic regression discontinuity design (GRDD) is that individuals

[15] Migration choices may be related to time preferences. For example, Goldbach and Schlüter (2018) present evidence that migration from developing countries is more likely for patient individuals.

[16] See, for example, Hunt (2006) and Fuchs-Schündeln and Schündeln (2009) for a description and analysis of migration after the end of the GDR.

who lived in former FRG counties close to the border represent good comparisons for individuals who lived in GDR counties close to the border, since spatially close counties were in all likelihood more similar to each other before the political division. Accordingly, the procedure considers only observations within a specific bandwidth around the border and – with respect to the observations within the bandwidth – attaches more weight to individuals living closer to the FRG/GDR border. We will consider three different bandwidths: The mean squared error optimal bandwidth suggested by Imbens and Kalyanaraman (2012) as well as two multiples of this optimal bandwidth (0.75, 1.25).

The running variable in our empirical model is the Euclidian distance from the centroid of each respondent's county of residence and the border.[17] Following Gelman and Imbens (forthcoming), for example, we estimate the following local linear regression with a triangular kernel

$$y_{icbt} = \alpha + \beta GDR_i + \gamma_1 \text{Euclid}_c + \gamma_2 \text{Euclid}_c * GDR_i + \delta_b + \epsilon_{icbt}, \quad (1)$$

where the dependent variable y_{icbt} is the time preference measure of individual i living in county c along the border segment b at time t. GDR_i is an indicator for having lived in the GDR in 1989 (i.e., before reunification). Euclid_c is the Euclidian distance as defined above and the interaction $\text{Euclid}_c * GDR_i$ allows for different slopes of the regression function on both sides of the FRG/GDR border. By including δ_b, we incorporate fixed effects for eight equally-sized border segments to allow for heterogeneous levels of the outcome of interest along the FRG/GDR border (as in Dell 2010).[18] Finally, ϵ_{icbt} is an error term that can be considered as a random error generating variation in y_{icb} around the local regression line. We compute both standard errors clustered at the individual level (due to the unbalanced nature of our working sample) and standard errors clustered at the county level (to take into account the discreteness of our running variable; see, e.g., Lee and Card 2008).

Our GRDD is similar to those presented in the literature, but we want to stress two important differences.[19] First, our outcome measures stem from the years 2008 and 2013, allowing for observed sorting around the former border after reunification but before our years of observation. To account for this concern, we drop survey respondents who moved from West to East or from East to West Germany within the relevant post-reunification period in our GRDD specifications.[20] Second, the potential role of specific government policies during the period of

[17] We incorporate 396 different counties in the estimating sample for our GRDD analysis.

[18] We exclude Berlin from our working sample for two reasons. First, we cannot calculate a reasonable Euclidian distance for people living in West Berlin (as there was both a border surrounding Berlin and the Berlin wall border). Second, we suspect strong selective migration in both parts of Berlin during the German separation for various reasons (e.g., political and cultural ones).

[19] For example, Becker et al. (2016) also assess the long-run effects of a border that existed in the past.

[20] We thereby lose about 4.4 % of our estimating sample (of which 3.6 % represent East-West movers). The results from our ordinary least squares regression exercises that we will report in Section 4.1 are robust to this

the German separation must be acknowledged. Specifically, West Germany started a sizable subsidization scheme in 1971 to counteract potential disadvantages from being cut-off from adjacent markets due to the FRG/GDR border and to curtail possible emigration. The FRG thereby created the *Zonenrandgebiet* including all districts that have 50 percent of either their area or their population within a distance of 40 km to the border to the GDR or Czechoslovakia (see, e.g., von Ehrlich and Seidel forthcoming). In the GDR, access to areas close to the border was always very restricted and there is evidence that people who were considered politically unreliable (and others) were forced to move further away from the border. Moreover, the area that was in a bandwidth of about 5 km was categorized as prohibited area implying very strict rules with regard to such aspects as cultural life and visitation rights, on the one hand, and privileges in terms of food stamps etc. on the other (e.g., BStU 2012). As a result of these place-based policies, selective sorting along the border during the separation period may be expected on both sides. To account for this concern, we also employ *donut* specifications in our empirical work. Specifically, we first present results for the optimal bandwidths h^* ($0.75/1.25 \times h^*$) according to the criterion suggested by Imbens and Kalyanaraman (2012). In addition, we show parameter estimates from donut specifications with optimal bandwidths h^* ($1.25 \times h^*$), where we do not consider observations from Western (East) counties with a centroid less than 40 (10) km from the FRG/GDR border.[21]

4 Empirical Results

Our findings on the influence of the GDR's socialist regime on time preferences are presented in this section. After presenting descriptive evidence on the matter, we will turn to results from our two identification strategies and then discuss findings from robustness checks.

In Table 2, we see that the treatment group's mean of the proxy for present bias is notably lower than that of the control group, whereas the treatment group's mean of the proxy for patience does not differ significantly. Moreover, when we use indicator variables, which are equal to one when the response on the respective preference measure was higher than six, we find similar patterns. In both cases, t-test results indicate that present-bias is significantly less pronounced in the treatment group.

change in the sample. Results are available upon request.

[21] In their study about work attitudes of females, Campa and Serafinelli (forthcoming) also apply a GRDD approach to the natural experiment represented by the German separation/reunification episode, similarly including donut specifications.

	Present Bias	Patience
	Answer	
Control group (FRG)	5.12	6.10
Treatment group (GDR)	4.95	6.13
	Dummy Variable= 1 when Answer > 6	
Control group (FRG)	0.29	0.50
Treatment group (GDR)	0.25	0.49

Notes: We use SOEP data from 2008 and 2013. Dummy variable is equal to one when the response on the respective time preference measure was higher than six. The numbers of observations are as follows: Present bias & FRG: $N = 23,169$, present bias & GDR: $N = 8,701$, patience & FRG: $N = 23,184$, patience & GDR: $N = 8,695$.

Table 2: Means for SOEP Proxies of Present Bias and Patience

4.1 Results from Ordinary Least Squares Regressions

Using ordinary least squares regressions, we find that former GDR citizens exhibit significantly lower levels of present bias (Column (1) in Table 3). The sign of the treatment effect is consistent with the idea that institutional aspects of the GDR (such as the induced economic stability and the impulse-control incentivized by the massive state-security service) diminished present bias. The size of the GDR treatment effect is comparable to that of the gender effect, and the difference between the two is not significantly different from zero. In contrast, we do not find a lasting influence of the GDR's political regime on patience. The estimated parameter is positive, as expected, but not significant.

	(1)	(2)
	Present Bias	**Patience**
GDR	-0.197**	0.005
	(0.030)	(0.032)
Age	0.014**	0.022**
	(0.005)	(0.005)
Age^2	-0.000**	-0.000**
	(0.000)	(0.000)
Male	-0.272**	-0.009
	(0.027)	(0.029)
Raised in a large city	0.217**	-0.103*
	(0.037)	(0.040)
Raised in a medium-sized city	0.161**	-0.030
	(0.039)	(0.042)
Raised in a small city	-0.002	0.045
	(0.036)	(0.039)
Mother: Secondary lower school	0.088	-0.120$^+$
	(0.062)	(0.066)
Mother: Secondary intermediate school	0.119$^+$	-0.155*
	(0.068)	(0.074)
Mother: Secondary higher school	-0.023	-0.123
	(0.094)	(0.102)
Mother: No vocational degree	-0.069	0.060
	(0.053)	(0.056)
Mother: Vocational degree	0.040	0.010
	(0.052)	(0.056)
Mother: Technical school	-0.073	0.056
	(0.105)	(0.115)
Mother: University degree	-0.112	0.021
	(0.103)	(0.112)
Father: Secondary lower school	-0.144*	0.110$^+$
	(0.060)	(0.065)
Father: Secondary intermediate school	-0.173*	0.095
	(0.068)	(0.074)
Father: Secondary higher school	-0.170*	0.098
	(0.082)	(0.090)
Father: No vocational degree	0.095	0.097
	(0.070)	(0.075)
Father: Vocational degree	0.105$^+$	0.070
	(0.055)	(0.060)
Father: Technical school	0.084	-0.091
	(0.102)	(0.114)
Father: University degree	0.138	0.065
	(0.085)	(0.091)
N	33,109	33,119
R^2	0.016	0.007

Notes: We use SOEP data from 2008 and 2013. Parameter estimates come from ordinary least squares regressions. The dependent variable is the SOEP measure for *present bias* and *patience*, respectively. Robust standard errors are clustered at the individual level and presented in parentheses; $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 3: Time Preferences and GDR: Ordinary Least Squares Regressions

Matching the results of earlier studies using the natural experiment of the German separa-

tion and reunification on the dynamics of the East-West divide (e.g., Brosig-Koch et al. 2011, Rainer and Siedler 2009), we find that there is no notable convergence with regard to present bias when considering the five-year lag between 2008 and 2013. More specifically, including an interaction term of GDR and the year 2013 in our specifications from Table 3, we find that there is no remarkable change in the estimated parameters of interest and that the coefficient of the interaction term is never significantly different from zero (see Table 4).

	(1)	(2)
	Present Bias	**Patience**
GDR	-0.223**	-0.028
	(0.036)	(0.039)
Year 2013	0.092**	0.063*
	(0.025)	(0.026)
GDR × 2013	0.056	0.071
	(0.044)	(0.045)
N	33,109	33,119
R^2	0.016	0.007

Notes: We use SOEP data from 2008 and 2013. Parameter estimates come from ordinary least squares regressions. The dependent variable is the SOEP measure for *present bias* and *patience*, respectively. All covariates from Table 3 are part of the empirical model. Robust standard errors are clustered at the individual level in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 4: Time Preferences and GDR: No Signs of Convergence

One might wonder whether unobservables that affect the distribution of time preferences (such as East-West migration before 1961) lead to omitted variable bias. To address this concern, we estimate bounds for our GDR treatment effect from Table 3 based on the approach suggested by Oster (forthcoming). The crucial assumption of Oster's approach is that selection on observables is informative about selection on unobservables and that we can learn something from the change of coefficients when we add additional observable variables to a regression of the outcome on the treatment indicator. If we are furthermore willing to assume (1) that observable and unobservable covariates are equally correlated with the GDR treatment and (2) that the R^2 of the hypothetical regression of our outcomes on GDR treatment as well as both observed and unobserved controls is 1.3 times the R^2 of our preferred specification, we can calculate bounds for the treatment effect. Applying Oster's formulas to the problem at hand, we obtain the following bounding set for the treatment effect: $[-0.203, -0.197]$. Since the estimated bounds do not include zero, the result suggests robustness against omitted variable bias.

4.2 Results from Geographic Regression Discontinuity Design

To best communicate our Geographic Regression Discontinuity Design results, we include graphical illustrations of the potential GDR treatment effects. Figures 2-3 show sample averages of the outcome variables within bins, fourth-order polynomials fitted separately on both sides of the FRG/GDR border as well as the optimal bandwidths according to Imbens and Kalyanaraman (2012), which are also used in our GRDD estimations (dashed vertical lines). The plots reveal a clear discontinuity of present bias at the erstwhile border. In contrast, the plot for patience does not suggest a systematic change at the border. This is in accordance with our previous results.

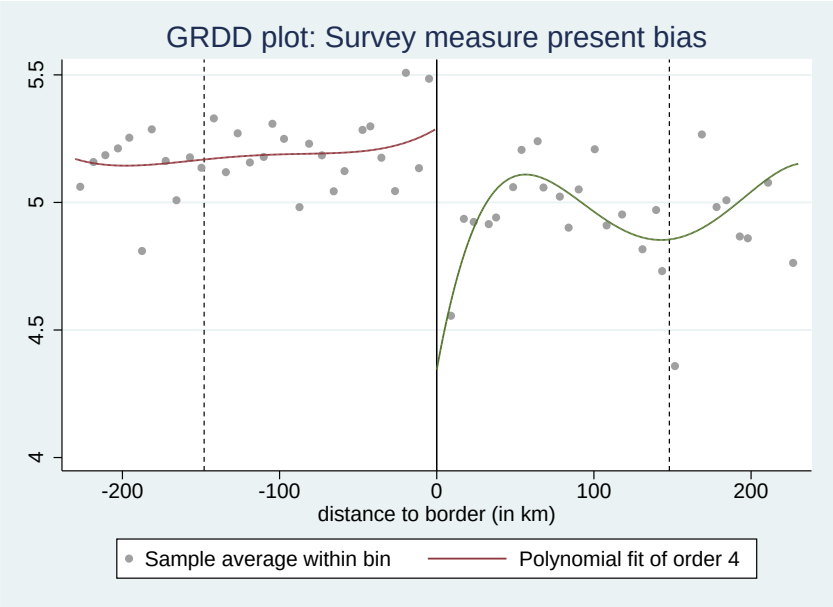

Figure 2: GRDD Plot for Present Bias (where negative numbers refer to distance of FRG counties to the border and dashed vertical lines represent optimal bandwidth according to Imbens and Kalyanaraman (2012))

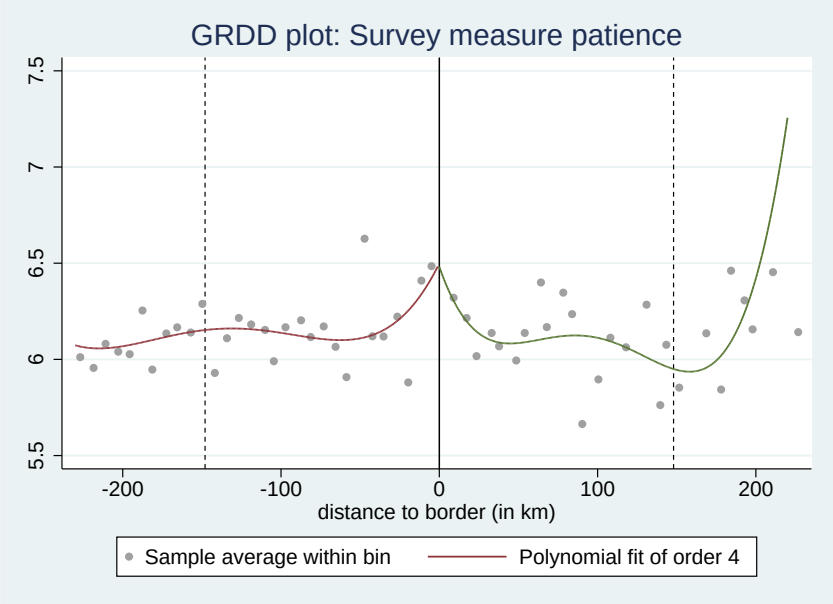

Figure 3: GRDD Plot for Patience (where negative numbers refer to distance of FRG counties to the border and dashed vertical lines represent optimal bandwidth according to Imbens and Kalyanaraman (2012))

The GRDD plots provide a clear discontinuity only for present bias. Hence, in Table 5, we present the results of our various GRDD specifications only for present bias. Overall, we find a GDR treatment effect relatively similar to the one documented in Section 4.1 across all specifications. However, the magnitude of the estimated GDR treatment effect is somewhat larger in comparison to the ordinary least squares estimates. Possibly, living closer to the FRG/GDR border strengthens the GDR treatment effect. The donut specifications are meant to address issues of non-random selection along the border, and their results are clearly in line with our main result.

	(1)	(2)	(3)
Bandwidth	h^*	$1.25h^*$	$0.75h^*$
	Main Results		
GDR	-0.461	-0.401	-0.550
	$(0.091)^{**}$	$(0.081)^{**}$	$(0.107)^{**}$
	$[0.106]^{**}$	$[0.097]^{**}$	$[0.124]^{**}$
N	14,888	19,367	11,265
R^2	0.006	0.005	0.008
	Donut Results		
GDR	-0.379	-0.298	
	$(0.130)^{**}$	$(0.105)^{**}$	
	$[0.137]^{**}$	$[0.114]^{**}$	
N	12,963	17,442	
R^2	0.003	0.003	

Notes: We use SOEP data from 2008 and 2013. Parameter estimates are based on Specification 1 detailed in Section 3.2.2. The dependent variable is the present bias measure. Robust standard errors clustered at the individual (county) level are reported in parentheses (square brackets); $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. The donut specification relies on observations within a bandwidth of h^* or $1.25h^*$ around the border excluding observations closer than 40 km (10 km) on the Western (Eastern) side of the border.

Table 5: Present Bias and GDR: Geographic Regression Discontinuity Design

4.3 Exploring Variation in Treatment Intensity: Duration of Regime Exposure

We consider the effect of the number of years spent under socialism on present bias by defining four groups according to respondent year of birth and running regressions for each cohort (Columns (1)-(4) in Table 6). The results from our ordinary least squares regressions in the upper half of the table support the simple intuition that both having spent a long period of time under the GDR regime and having received indoctrination during daycare, school, and youth organization attendance entailed important effects on the present bias of a regime's citizens. In light of our discussion of the malleability of preferences in Section 3.1, it is important to note that we can be sure that the relevant, formative period of life was experienced subject to the

GDR regime only with respect to the second cohort, but also – with due qualifications – with respect to the first and third cohort. With respect to the youngest cohort in Column (4), our result from the ordinary least squares regression is positive, small, and insignificant. In our GRDD results, we find a hump-shaped pattern when looking at coefficient magnitudes, implying a greater present bias regime effect on individuals with more time under regime policies and those with an education that was begun and completed under the regime. This is consistent with the argumentation concerning the formation of time preferences presented in Section 3.1.

	(1)	(2)	(3)	(4)
	Birth year <46	Birth year 46-60	Birth year 61-75	Birth year 76-89
Ordinary Least Squares Regressions				
GDR	-0.209**	-0.236**	-0.185**	0.014
	(0.058)	(0.057)	(0.060)	(0.068)
N	8,815	9,270	9,085	5,939
R^2	0.025	0.011	0.017	0.017
Geographic Regression Discontinuity Design				
GDR	-0.454	-0.458	-0.661	-0.522
	(0.184)*	(0.163)**	(0.173)**	(0.236)*
	[0.205]*	[0.156]**	[0.189]**	[0.225]*
N	3,950	4,253	4,423	1,863
R^2	0.011	0.008	0.014	0.017

Notes: We use SOEP data from 2008 and 2013. The dependent variable is the SOEP measure for *present bias*. OLS: All covariates from Table 3 are part of the empirical model. Robust standard errors are clustered at the individual level. GRDD: Robust standard errors are clustered at the individual (county) level in parentheses (square brackets); $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 6: Present Bias and GDR Treatment: Cohorts Results

The prior literature has reported similar findings for other economic preferences. For example, Alesina and Fuchs-Schündeln (2007) report that greater support for a government role is more pronounced the longer the subject has lived under the socialist regime. Similarly, Heineck and Süssmuth (2013) find that any divergence between former GDR and FRG residents with respect to risk preference is more pronounced for older individuals.

4.4 Robustness Checks

In this section, we provide a series of robustness checks to demonstrate that our main results are not affected by further observed heterogeneity, measurement issues, or modeling choices.

Differences During the Treatment Period We suggest that the political regime differences contributed to the observed divergence in present bias. These regime differences had

substantial consequences for the economic development in both countries during the treatment period. In an attempt to take these economic differences into account, we conduct two analyses. First, we include the average GDP growth rate during the *impressionable years* of members of both treatment and control groups in our ordinary least squares regressions.[22] The basic idea is that time preferences are formed by average GDP growth experience in early adulthood, since a more or less prosperous economic environment might impact on the extent of the individual bias for the present.[23] Following the literature, we define these distinctive years as the period between 18 and 25 years of age and restrict our working sample to respondents who start with their impressionable years during the treatment period. Note that the applied age range is consistent with the period in which time preferences are malleable according to our working sample (Section 3.1). The ordinary least squares results in the upper part of Table 7 show that the coefficient of the GDR treatment dummy is significant and negative, and that the extent of present bias is smaller for former GDR citizens when higher average GDP growth rates were experienced during their impressionable years. Second, we consider a proxy for heterogeneous wealth during the treatment period. In particular, we include a dummy variable of house ownership status in 1990 before reunification in our regression exercises.[24] The results in the lower part of Table 7 reveal that we find a significant negative treatment effect again and that house ownership has differential effects on present bias.

[22]The impressionable years hypothesis proposes that individuals are susceptible to attitude change during late adolescence and early adulthood and that susceptibility drops radically thereafter. Evidence confirming the hypothesis is presented, for example, in Krosnick and Alwin (1989). Giuliano and Spilimbergo (2014) provide a recent application in economics. The GDP data for the years 1947 to 1989 for East and West Germany and from 1990 to 1991 for West Germany are from Ritschl and Spoerer (1997). The GDP data from 1990 to 1991 for East Germany are from Sleifer (2006). These data are available at: `histat.gesis.org/histat/`. The data for the period 1992 to 2017 stem from the German Statistical Offices (see `statistik-bw.de/VGRdL/tbls/?lang=en-GB`.

[23]Note that we cannot use unemployment as an economic indicator, since unemployment was officially equal to zero in the former GDR.

[24]As a qualifier, we attest that home ownership may be a bad control variable. At least in the GDR, home ownership was related to the individual willingness to achieve this end, which might have been influenced by the level of self-control.

Average GDP Growth Rate	
GDR	-0.1341*
	(0.0553)
Average GDP Growth Rate	-0.0160
	(0.0104)
Average GDP Growth Rate × GDR	-0.0446*
	(0.0196)
N	31,568
R^2	0.0124
Home Ownership	
GDR	-0.3315**
	(0.0798)
Home Ownership	-0.1462$^+$
	(0.0765)
Home Ownership × GDR	0.2089$^+$
	(0.1231)
N	7,407
R^2	0.0148

Notes: We use SOEP data from 2008 and 2013. Parameter estimates from ordinary least squares regressions. The dependent variable is the SOEP measure for *present bias*. All covariates from Table 3 are part of the empirical model. Robust standard errors are clustered at the individual level in parentheses; $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 7: Present Bias and GDR Treatment: Taking Pre-Reunification Differences Into Account

Differences After the Treatment Period We rely on measurements of time preferences from 2008 and 2013. The two parts of Germany were reunified in 1990. Attributing any difference in time preferences to the GDR's socialist regime presupposes that the life experiences of East and West Germans after 1990 were either comparable, observably and measurably different, or had insignificant effect on time preferences. One potentially important reason why former GDR and FRG residents had different post-reunification experiences is that the post-reunification economic performance of East Germany dramatically lagged West Germany's. In particular, this becomes manifest in the dramatic increase in the official unemployment rate in East Germany up to roughly 20% after reunification. To test whether the economic turmoil after reunification had an impact on time preferences measured in 2008 and 2013, we use the salient unemployment experience of former GDR citizens compared to FRG citizens as an indicator. We conduct a linear mediation analysis with the outcome present bias, in which we attempt to disentangle direct regime effects on present bias and "indirect" effects via different

unemployment trajectories in West and East Germany, the latter at least partly triggered by the GDR treatment. In particular, we use the following system of linear equations:

$$\text{unempexp}_{it} = \alpha_1 + \beta_1 GDR_i + \omega_1 x_{it} + \epsilon_{it,1}, \tag{2}$$

$$\text{present bias}_{it} = \alpha_2 + \beta_2 GDR_i + \gamma \text{unempexp}_{it} + \nu \text{unempexp}_{it} \times GDR_i + \omega_2 x_{it} + \epsilon_{it,2}, \tag{3}$$

where unempexp_{it} stands for years of unemployment experienced by individual i (resulting from an *involuntary* job loss) at time t and x_{it} stands for the vector of covariates also used in the ordinary least squares regressions above. The average mediation effect for former GDR citizens is given by $\delta = \beta_1(\gamma + \nu)$, indicating the indirect regime effect on the individual extent of present bias of former GDR citizens transmitted via the own unemployment experience after reunification. The average direct effect is given by $\zeta = \beta_2 + \nu[\alpha_1 + \beta_1 + \omega_1 E(x_i)]$, indicating the direct effect of the GDR's political regime on present bias given the years of unemployment. To disentangle the total causal effect into the two causal channels defined above, two crucial assumptions must hold (e.g., Huber 2014, Imai et al. 2010 a,b): (1) conditional independence of the GDR treatment, that is, there are no unobserved confounders jointly affecting both the GDR treatment and the years of unemployment experience and/or time preferences conditional on the vector of covariates. (2) conditional independence of the mediator, that is, conditional on GDR treatment and the vector of covariates, unemployment experience is *effectively* randomly assigned. With respect to assumption (1), we acknowledge again that the German history of separation and reunification represents a natural experiment. To ensure assumption (2) in our empirical analysis below, we compare only those members of the control and the treatment group who have either experienced no unemployment spell at all or whose first job loss after the German reunification was due to a plant closure. The main argument is that plant closures are an arguably exogenous event from the perspective of the individual worker (Anger et al. 2017, Marcus 2013), in particular for former GDR citizens who worked for former state-owned companies.

Table 8 shows the estimated average direct and indirect effects of the GDR treatment on present bias for the treatment group of former GDR citizens. Column (1) presents the results for the working sample described above, Column (2) documents the results for a subgroup of the working sample who spent all of their impressionable years either in the GDR or the FRG. The direct effect of the GDR treatment on present bias is significantly negative in both specifications, supporting our main results. The size of the estimated indirect effect is small and significant in Column (2) (but only at the ten percent level). In this case, the ratio of the indirect and the direct effects amounts to roughly ten percent, indicating that the direct regime effect remains important when we try to take indirect, yet salient post-reunification experiences

into account.

	(1) Full Sample	(2) Impressionable years sample
Direct effect	-0.1923**	-0.2405**
	(0.0423)	(0.0545)
Indirect effect	0.0119	0.0204+
	(0.0079)	(0.0123)
Ratio indirect/direct	-0.062	-0.085+
	(0.0405)	(0.0502)
N	18,654	11,436

Notes: We use SOEP data from 2008 and 2013. In Column (2), we focus on subjects who spent all of their impressionable years either in the GDR or the FRG. Estimated direct and indirect effects are calculated from parameter estimates of equation system (2) and (3) based on linear structural equation modeling. Robust standard errors are clustered at the individual level. z statistics in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 8: Present Bias, Unemployment, and GDR Treatment: Results from a Mediation Analysis

In addition to the mediation analysis using unemployment, we consider an identifiable subgroup who had a relatively "smooth transition" in economic terms, that is, individuals who – during GDR times – had a social network with links into the FRG.[25] This subgroup is likely to have had relatively better post-reunification experiences based on the argument that Burchardi and Hassan (2013) establish for West Germans with social ties in East Germany. The positive income impact of social ties that Burchardi and Hassan report can also be found in our data for East Germans with links in the FRG. Focusing on this subpopulation of East Germans, we find that they also display a lower present bias. The coefficient using ordinary least squares is -0.26^{**}, while it is about -0.33^{**} when using the GRDD approach.[26]

Migration During the Treatment Period There was significant migration from East to West Germany after the treatment imposition and before the erection of the Berlin Wall, which is potentially problematic for our identification. For subjects in our sample who participated in at least in one of the survey years 1990 to 1993, we can reconstruct whether or not they migrated from East to West Germany during the separation period. Applying our identification strategies to the resulting subsample, we obtain results that are consistent with our main results (see Tables 12-13 in Appendix B).

[25]The information about social ties stems from a question addressed at participants of the new East German SOEP sample in spring 1990. At the household level, participants were asked whether they have had relatives or friends in the former FRG.

[26]Regression results are available upon request.

Migration After the Treatment Period After 1989, notable migration from East to West Germany, and some movement from West to East Germany took place (e.g., Hunt 2006, Fuchs-Schündeln and Schündeln 2009). The SOEP provides us with information on both directions of migration between West and East Germany after reunification. When we add dummy variables indicating whether someone has moved from East Germany to West Germany or vice versa in our ordinary least squares approach, the estimated treatment effect is -0.21^{**} $[s.e. : 0.03]$ and the two ordinary least squares coefficients of the migration dummy variables are not significantly different from zero at $\alpha = 0.05$.[27]

Measurement Of Time Preferences In Section 3.1, we documented that present bias in particular is remarkably stable in middle age, while we found notable variation in early adulthood and old age. This explains why we use all available time preferences information in our regression exercises. However, it is interesting to check whether our results are robust to considering only one observation per person. To this end, we consider (1) the average value of measurements of present bias and (2) only the latest available information on present bias. With respect to the average value, we obtain estimated treatment effects of -0.22^{**} $[s.e. : 0.03]$ using ordinary least squares and significantly negative GDR treatment effects in the range $[-0.70^{**}, -0.48^{**}]$ in our GRDD approach depending on the chosen bandwidth. Considering the latest available information, we find a significantly negative GDR treatment effect on present bias of -0.20^{**} $[s.e. : 0.03]$ applying ordinary least squares and significantly negative GDR treatment effects in the range of $[-0.72^{**}, -0.48^{**}]$ using our GRDD specification.

Regional Effects We argue that we are capturing the influence of GDR's socialist regime. It may be conjectured that the estimated average treatment effects mask heterogeneity across GDR regions. To explore this aspect, we consider specifications that comprise state-specific effects for each state from East Germany (applying the post-reunification demarcation of states that was almost identical to that between 1945 to 1952) and a dummy variable indicating 1 for survey respondents without state information. Our results indicate some (albeit no considerable) heterogeneity across states (see Table 14 in Appendix B). One interpretation of the limited heterogeneity across states is that it is due to limited regional variation in treatment intensity. Many, if not most critical characteristics of the GDR regime (e.g., the indoctrination in educational facilities) applied rather uniformly to all regions. In terms of regional effects, we may also take into account that East and West Germany were and continue to be very

[27] Remember that, in our GRDD estimations, we exclude survey respondents who moved from West to East or from East to West Germany within the relevant post-reunification period.

different in terms of religion.[28] This itself may be an outcome of the political regime differences in East and West Germany (since religiosity was suppressed in the GDR). Before treatment imposition, residents who lived in the area of what became the GDR were mostly Protestant, whereas residents of what became the FRG were either Protestant or Catholic, as explained by Bauernschuster and Rainer (2012). We restrict our sample to regions that were unambiguously Protestant – that is, we omit data from Bavaria, Baden-Wuerttemberg, Northrhine-Westfalia, Rhineland-Palatinate, and Saarland – and find that our results from Section 4.1 are robust to this consideration.[29]

Influence on Related Personality Traits Economic preferences and personality traits are considered to be complements when explaining important life outcomes and behavior (e.g., Humphries and Kosse 2017). Nevertheless, there are some conceptual relationships. For example, Almlund et al. (2011: p. 70) hypothesize that time preferences and conscientiousness are most likely related. Becker et al. (2012) find a small correlation in a large SOEP data set between time preferences and conscientiousness and a medium-sized one between time preferences and agreeableness.[30] Based on this argumentation and results, it is interesting to explore whether conscientiousness and agreeableness are also influenced by the GDR's regime. We build on and extend Friehe et al. (2015) for this purpose.[31] Table 9 presents the results using our first identification strategy.

[28]Religion may be relevant to time preferences. For instance, Doepke and Zilibotti (2014) argue that religious beliefs such as Protestantism can be seen as complementary drivers of patience and work ethic.

[29]Regression results are available upon request.

[30]The psychological literature defines conscientiousness as the tendency to be organized, responsible, and hard working and agreeableness is defined as the tendency to act in a cooperative, unselfish manner (e.g., Almlund et al. 2011: p. 11).

[31]The SOEP comprises information about conscientiousness and agreeableness in 2005 and 2009 in the form of respondents' self-positioning on scales ranging from 1 to 7 as an answer to how well specific statements describe their personality. We generate measures of agreeableness and conscientiousness by standardizing the sum of the scores of the dimension-specific questions whereby higher values indicate a stronger intensity of the particular trait (e.g. being more agreeable).

	(1) Conscient-iousness	(2) Agree-ableness
GDR	0.0984**	0.0285*
	(7.29)	(2.09)
N	35,417	35,536
R^2	0.048	0.039

Notes: We use SOEP data from 2005 and 2009. Parameter estimates are from ordinary least squares regressions. The dependent variable is the respective standardized personality measure. The specification includes all covariates from Table 3. Robust standard errors are clustered at the individual level. t statistics in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 9: GDR Treatment Effects on Conscientiousness and Agreeableness: Results Using Ordinary Least Squares Regressions

The estimated GDR treatment effect indicates that former GDR citizens have a higher conscientiousness score than former FRG citizens. Since a higher degree of conscientiousness is usually associated with higher self-control, the result is consistent with the observed lower present bias of former GDR residents (i.e., our main result). The GDR treatment effect is also significant and positive for the trait agreeableness. Gill and Prowse (2016), for example, provide evidence that more agreeable players learn faster and perform better in strategic interactions. Most likely, better performance in strategic interactions is positively linked to better self-control and therefore to less present bias.

When we turn to the second identification strategy, we find that the GRDD plots do not reveal a clear discontinuity at the former border between the FRG and the GDR with respect to both personality measures. In view of the small correlations between time preferences and these two personality traits reported in Becker et al. (2012), this outcome does not surprise.

Figure 4: GRDD Plots for Conscientiousness and Agreeableness (where negative numbers refer to distance of FRG counties to the border and dashed vertical lines represent optimal bandwidth according to Imbens and Kalyanaraman (2012))

Probit Regression In Table 2, we presented descriptive evidence using a dummy variable that is equal to one when the answer to the question referring to present bias was higher than six. Using this information as the dependent variable in a probit regression yields an average partial effect for the GDR regime amounting to -0.048^{**}. To offer an interpretation of our result, the probability that an individual shows present bias is reduced by 17.5% when the individual has experienced the GDR regime instead of the FRG.

5 Exploring Implications of GDR Experience

Both of our identification strategies have shown that native Germans who lived in the GDR in 1989 (still) differ from those who lived in the FRG in terms of present bias. In this section, we explore some potential economic consequences of this long-lasting impact of the GDR's political regime, using outcome measures from the SOEP that are commonly understood to be determined by present bias (e.g., Meier and Sprenger 2010, O'Donoghue and Rabin 2006). Specifically, we will test whether our survey measures of present bias predict health related outcomes (body mass index, obesity, and smoking); savings and credit uptake behavior; and education outcomes (specifically, whether or not the individual obtained a college or a university degree) while controlling for GDR treatment status.

We report two sets of results in Table 10. First, we use two measures of present bias in pooled ordinary least squares and pooled Probit regression exercises to analyze the association between present bias and selected economic outcomes. Second, we use the lagged value of present bias reported in 2008 as an instrument (as exclusion restriction) in various econometric specifications in an attempt to address the possibility of unobserved confounding between present bias and our selected economic outcomes. In particular, we use a two-stage-least-square approach, a control

function approach, and a recursive two-equations system depending on the nature of both the outcome and the present bias variable. In all econometric specifications, we are interested in the predictive power of present bias on the economic outcome of interest. Information on the outcome measures in the upper part of Table 10 stem from 2008 and 2013 for smoking, saving, credit, and education, and from 2008 and 2014 for BMI and obesity. In the lower part of Table 9, information on the economic outcomes stem from 2013 and 2014.

In line with the literature, we find that our survey measures of present bias have predictive power for all outcomes considered in almost all cases. A greater present bias is associated with a larger BMI, increased likelihood of being obese, greater propensity to be a smoker, decreased probability of saving regularly (only in the pooled specifications), raised probability of having to pay back a consumer credit, and a lowered chance of obtaining a college or university degree. For example, being present biased (survey response > 6) correlates to an increase in the probability to smoke by about five to fifteen percentage points, depending on the econometric specification. Compared to the raw mean of smoking status in our working sample, this implies a notable change of 20% to 58%. Furthermore, we observe significant differences between the control and the treatment group for all outcomes. To illustrate the importance of the GDR treatment effect on the outcomes of interest relative to the effect of present bias, let us consider the probability of having to pay back a credit: Being a former citizen of the GDR implies an increase in the probability of an outstanding credit obligation by roughly 7 percentage points, while present bias alone correlates with an increase in the same credit obligation probability by about 2 to 8 percentage points depending on the specification. Hence, when taken at face value, the effect of having lived in the GDR is quantitatively at least as important for the prediction of credit behavior as present bias, allowing for the fact that, on average, former GDR citizens are less present biased than former FRG citizens.

6 Conclusion

Time preferences are integral to modern economics. They are critical to a host of decisions, many of which have key impacts on important life outcomes (Golsteyn et al. 2014). There is considerable heterogeneity regarding time preferences and relatively little knowledge about the sources of this variety (Falk et al. forthcoming, Wang et al. 2016). Intergenerational transmission and the institutional environment likely play important roles. In this line of inquiry, the present study contributes by exploring whether the GDR's socialist institutional environment has left a notable and lasting footprint on its former citizens' time preferences.

	Body Mass Index		Obesity		Smoking		Saving		Credit		Education	
	OLS		Probit		Probit		Probit		Probit		Probit	
GDR	0.489**	0.490**	0.028**	0.028**	0.030**	0.030**	-0.053**	-0.053**	0.072**	0.072**	-0.120**	-0.121**
	(0.073)	(0.073)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.005)	(0.005)	(0.011)	(0.011)
Present bias	0.069**		0.004**		0.012**		-0.006**		0.007**		-0.012**	
	(0.014)		(.001)		(.001)		(0.001)		(0.001)		(0.002)	
Dummy present bias		0.331**		0.020**		0.043**		-0.024**		0.021**		-0.050**
		(0.064)		(0.005)		(0.005)		(0.006)		(0.005)		(0.010)
F-Test/ LR-Test (df=22)	107.10**	107.11**	586.29**	585.62**	1470.91**	1459.16**	533.78**	528.90**	1496.18**	1492.06**	946.49**	941.70**
N	31,210		31,210		31,516		32,705		32,635		7,646	
Allowing for unobserevd confounders: Instrument/exclusion restriction: lagged present bias (t-5)												
	2SLS	2SLS	Probit-CF	Probit-RS	Probit-CF	Probit-RS	Probit-CF	Probit-RS	Probit-CF	Probit-RS	Probit-CF	Probit-RS
GDR	0.501**	0.536**	0.029**	0.031**	0.027**	0.030**	-0.033**	-0.034**	0.071**	0.075**	-0.157**	-0.153**
	(0.105)	(0.107)	(0.009)	(0.010)	(0.009)	(0.010)	(0.010)	(0.010)	(0.008)	(0.009)	(0.019)	(0.018)
Present bias	0.213**		0.009**		0.030**		-0.006		0.013**		-0.039**	
	(0.057)		(0.005)		(0.004)		(0.005)		(0.004)		(0.010)	
Dummy present bias		1.485**		0.079*		0.147**		-0.028		0.077*		-0.222**
		(0.364)		(0.033)		(0.029)		(0.032)		(0.030)		(0.057)
F-Test/ LR-Test (df)	34.43**	33.81**	213.28**	1168.04**	584.04**	1583.16**	165.32**	1142.88**	657.31**	1656.74**	369.52**	634.10**
	(22)	(22)	(22)	(44)	(22)	(44)	(22)	(44)	(22)	(44)	(22)	(44)
N	9,639		9,639		9,764		10,468		10,440		2,260	

Notes: We use SOEP data from 2008, 2013, and 2014. Outcome measures in the upper part of the table stem from either 2008 and 2014 (BMI, Obesity) or 2008 and 2013 (Smoking, Saving, Credit, Education) and in the lower part of the table from either 2013 or 2014. Body Mass Index: BMI-formula; Obesity: Dummy variable is equal to 1 if BMI $\geq$ 30; Smoking: Dummy variable is equal to 1 if "yes". Saving: Dummy variable is equal to 1 if respondents save some money every month. Credit: Dummy variable is equal to 1 if respondents pay back a consumer credit. Education: Dummy variable is equal to 1 if respondents have a college or university degree. The dummy variable for present bias is equal to one when the response was higher than six on a ten-item scale. The specifications include all covariates from Table 3. Average partial effects (APE) are presented. Probit-CF: Control-function approach with lagged present bias (t-5) as exclusion restriction. Probit-RS: Recursive probit two-equation system with lagged present bias (t-5) as exclusion restriction. Both parts of the table: Columns (1)-(2): F-Test overall significance; Columns (3)-(12): LR-Test overall significance. Additional information on the lower part of the table: Columns (1)-(2): F-Test excluded instrument first stage: 1606.26^{**}; 814.6^{**}. Columns (3), (5), (7), (9), (11): Wald test of exogeneity (df=1): 0.30, 24.52^{**}, 0.37, 1.19, 4.75^{*}. Columns (4), (6), (8), (10), (12): LR-test of rho=0 (df=1): 3.51^{+}, 13.66^{**}, 0.02, 3.75^{+}, 6.27^{*}. Standard errors are clustered at the individual level if pooled data are employed and presented in parentheses. Probit-RS: Bootstrapped standard errors with 500 replications. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 10: Present Bias and Direct and Indirect Treatment Effects

Our results suggest that – indeed – the GDR's socialist regime significantly and enduringly diminished the present bias of its former citizens. We find survey-based evidence indicating influence of political institutions on time preferences. Significant differences in East and West Germans' levels of present bias remain two decades after reunification and the data does not suggest convergence.

The reported level of patience of former GDR citizens is comparable to that of former FRG citizens. The relatively small variation is directionally aligned with our hypothesis about higher patience among former GRD residents (but insufficient to generate significant differences) and consistent with small variation in patience levels across much more diverse countries (Wang et al. 2016).

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Appendix

A Exploring the relationship of the SOEP proxy for present bias (impulsivity) and an experimental measure of present bias

In 2006, experiments were conducted with a subsample of the SOEP to elicit their time preferences. In the experiment that we rely upon for our experimental measure of present bias, 526 individuals were asked to indicate their 20 choices from 20 sets of two alternatives, where the alternatives differed in their level of payout and their payout period. The difference in the payout period was fixed at one month. In a first task, participants were presented with the option of either receiving 200 euros immediately or some payment $x > 200$ euros in one month. In a second task, the decision was between 200 euros in 12 months and some payment $x > 200$ euros in 13 months. The level of x took on 20 values that were presented in an ascending order and the subjects were asked to state their choice for each of the 20 sets of two alternatives (Richter and Schupp 2014). After the experiment, a randomly selected subgroup of participants (11%) received 200 euros or a random $x > 200$ euros depending on their choice in the experiment.

The set of alternatives at which individuals switch from preferring the sooner payment to preferring the later payment gives us their switching point and is interpreted as a point of indifference. We observe valid switching points for 374 participants aged 18 to 80 years. These switching points allow us to calculate an experimental measure of present bias. For illustration, suppose that an individual is indifferent between the 200 euros immediately and x euros in one month (i.e., that $u(200) = \beta\delta u(x)$, where we use the notation from Footnote 2) and that this individual is indifferent between 200 euros in 12 months and y euros in 13 months (i.e., that $u(200) = \delta u(y)$). For this individual, we can infer $\beta = u(y)/u(x)$.

In a next step, we use an ordinary least squares specification to estimate the correlation between the experimental measure of present bias and our survey proxy for present bias (i.e., the SOEP measure for impulsivity) using age, age squared, and gender as additional covariates. For a subset of 322 of the 374 experiment participants with valid switching points and information about the place of residence in 1989, the SOEP survey in 2008 contains information on our proxy for present bias. We find a significant correlation between our survey measure for present bias and the experimental measure of present bias. The correlation remains significant and nearly unchanged when we add age, age squared, gender, and survey information on patience and risk (see Table 11 for complete results). Tobit and robust regression exercises yield similar results. Considering only subjects that were FRG residents in 1989, we find that the results from the

pooled sample re-emerge. Considering only subjects that were GDR residents in 1989, we find that the sign and the size of the coefficient remain intact. However, these coefficients are not statistically significant, which may be related to the very small number of observations.

	(1) Pooled sample	(2) Pooled sample	(3) Pooled sample	(4) FRG sample	(5) FRG sample	(6) FRG sample	(7) GDR sample	(8) GDR sample	(9) GDR sample
Impulsivity	-0.001*	-0.002*	-0.001+	-0.002*	-0.002*	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age		-0.001	-0.001		-0.001	-0.001		-0.000	0.000
		(0.001)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)
Age^2		0.000	0.000		0.000	0.000		-0.000	-0.000
		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
Male		0.001	0.002		0.001	0.002		0.002	0.003
		(0.003)	(0.003)		(0.003)	(0.003)		(0.007)	(0.007)
Patience		-0.001	-0.001		-0.001	-0.001		-0.001	-0.001
		(0.001)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)
Risk			-0.001			-0.001			0.001
			(0.001)			(0.001)			(0.002)
N	322	322	322	277	277	277	45	45	45
R^2	0.016	0.024	0.030	0.017	0.025	0.032	0.009	0.045	0.052

Notes: We use experimental data from 2006 and SOEP survey data from 2008. Parameter estimates come from ordinary least squares regressions. The dependent variable is the experimental measure for *present bias*. Robust standard errors are documented; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 11: Correlation of Impulsivity and Experimental Measure of Present Bias

B Robustness Checks

	Present bias
GDR	-0.178**
	(0.060)
Moved West during Separation	0.060
	(0.211)
Moved West	-0.147
	(0.170)
Moved East	-0.117
	(0.334)
Age	0.020
	(0.016)
Age^2	-0.000*
	(0.000)
Male	-0.302**
	(0.054)
Raised in a large city	0.237**
	(0.077)
Raised in a medium city	0.157+
	(0.083)
Raised in a small city	-0.033
	(0.075)
Mother: Secondary lower school	0.183
	(0.144)
Mother: Secondary intermediate school	0.157
	(0.164)
Mother: Secondary higher school	-0.192
	(0.242)
Mother: No vocational degree	-0.072
	(0.101)
Mother: Vocational degree	-0.081
	(0.103)
Mother: Technical school	0.116
	(0.178)
Mother: University degree	0.151
	(0.310)
Father: Secondary lower school	-0.332*
	(0.136)
Father: Secondary intermediate school	-0.266+
	(0.161)
Father: Secondary higher school	-0.156
	(0.194)
Father: No vocational degree	0.256*
	(0.127)
Father: Vocational degree	0.214*
	(0.105)
Father: Technical school	0.224
	(0.161)
Father: University degree	-0.041
	(0.201)
N	8,341
R^2	0.018

Notes: We use SOEP data from 2008 and 2013 for respondents who also participated in one of the survey years 1990 to 1993. Parameter estimates are from ordinary least squares specifications. The dependent variable is the SOEP *present bias* measure. The specification includes all covariates from Table 3. Robust standard errors are clustered at the individual level and reported in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 12: Taking Account of East-West Migration before 1989: Ordinary Least Square Regressions

	Present bias
GDR	-0.771
	$(0.230)^{**}$
	$[0.204]^{**}$
N	2,621
R^2	0.020

Notes: We use SOEP data from 2008 and 2013 for respondents who also participated in one of the survey years 1990 to 1993. Parameter estimates are based on Specification 1 detailed in Section 3.2.2. The dependent variable is the SOEP *present bias* measure. Robust standard errors clustered at the individual (county) level are reported in parentheses (square brackets); $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 13: Taking Account of East-West Migration before 1989: GRDD Regressions

	Present bias
GDR × Brandenburg	-0.446**
	(.100)
GDR × Meck. West. Pom.	-0.323*
	(.126)
GDR × Saxony	-0.201**
	(.071)
GDR × Saxony-Anhalt	-0.381**
	(.096)
GDR × Thuringia	-0.125
	(.090)
GDR × Berlin	-0.350*
	(.158)
GDR × state info miss	-0.149**
	.035
N	33,109
R^2	0.0163

Notes: We use SOEP data from 2008 and 2013. Parameter estimates are from ordinary least squares specifications. The dependent variable is the SOEP *present bias* measure. The specifications include all covariates from Table 3. Robust standard errors are clustered at the individual level and presented in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 14: Present Bias and GDR Treatment: GDR-states Interactions