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# The determinants of population self-control

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DISCUSSION PAPER

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# The Determinants of Population Self-Control*

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**Abstract:** This paper demonstrates that structural factors can shape people's self-control. We study the determinants of adult self-control using population-representative data and exploiting two sources of quasi-experimental variation—Germany's division and compulsory schooling reforms. We find that former East Germans have substantially higher levels of self-control than West Germans and provide evidence for suppression as a possible underlying mechanism. An increase in compulsory schooling had no causal effect on self-control. Moreover, we find that self-control increases linearly with age. In contrast to previous findings for children, there is no gender gap in adult self-control and family background does not predict self-control.

**Keywords:** determinants of self-control; quasi-experiments; German division; compulsory schooling reforms; population-representative evidence; Brief Self-Control Scale
**JEL Classifications:** *D90, C26*

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## 1 Introduction

The incentive to self-regulate is shaped by numerous factors outside the control of individuals and their families. In particular, local environments and cultural influences combine to provide the context in which people's self-control develops. Children, for example, develop the capacity for self-control sooner in cultures which highly value self-control (Oh and Lewis, 2008). Those growing up in socially cohesive neighborhoods have more capacity for self-control (Pratt et al., 2004), with neighborhood effects operating primarily through their role in either helping or hindering parents' efforts to instill self-control in their children (Stults and Swagar, 2021). Political, social, and economic institutions also matter because they determine the "rules of the game" (North, 1990)—often with powerful consequences. Bernheim et al. (2015), in particular, argue that poverty can damage self-control by trapping people in low-asset environments that undermine the ability to exercise self-control.

Researchers have repeatedly demonstrated the importance of self-control for people's life outcomes. People with more self-control have healthier lifestyles, higher educational attainment, more labor market success, enhanced financial well-being, and higher levels of life satisfaction (see, e.g., Cobb-Clark et al., 2022; Duckworth and Seligman, 2005; Kaur et al., 2015; Moffitt et al., 2011; Tangney et al., 2004). There is also extensive evidence regarding the individual- and family-level drivers of children's and adolescents' self-control. However, our understanding of the structural determinants of self-control is much more limited and is based on correlational evidence. "Structural factors" broadly refer to the political, economic, social, cultural, and environmental conditions and institutions that affect individuals but are largely beyond their control. This lack of evidence on the consequences of structural factors in shaping self-control is unfortunate given the potential for clever policy design to improve outcomes for vulnerable groups by changing the context in which they operate. Moving beyond individuals and families to consider self-control across society as a whole is important since life outcomes like educational attainment, labor market success, and health status that are intimately linked to self-control can also drive a society's overall productivity and hence living standards.

In this article, we investigate the structural determinants of self-control. Exploiting quasi-experimental variation generated by two key structural changes—Germany's division (e.g., Alesina and Fuchs-Schündeln, 2007; Becker et al., 2020) and a series of major educational

reforms (e.g., Pischke and von Wachter, 2008)—allows us to provide causal evidence on the structural influences underpinning self-control. Our data come from the 2017 Innovation Sample of the German Socio-Economic Panel (SOEP-IS). The SOEP-IS data are population-representative and now include a well-established measure of trait self-control—the Brief Self-Control Scale (BSCS) (Tangney et al., 2004). The Self-Control Scale is the most widely used measure of trait self-control in psychological research (Hoyle and Davisson, 2016).

Our analysis proceeds in two parts. We begin by analyzing several individual-level determinants of self-control including age, gender, and parental background. Each is plausibly exogenous, allowing for a causal interpretation. The results from this analysis not only provide important context for understanding the structural reforms we consider, they make a contribution by extending previous studies that rely on small and often selective samples of children and adolescents. Our data, in contrast, provide population-representative evidence on adult self-control. Understanding self-control at a societal level is crucial for investigating open empirical questions around, for example, the link between gender differences in self-control and the gap in men's and women's outcomes or Gottfredson and Hirschi's (1990) conjecture that self-control increases across the life cycle.

We find that there is substantial heterogeneity in adult self-control. Self-control increases linearly with age, confirming Gottfredson and Hirschi's (1990) hypothesis. Interestingly, the self-control distribution is virtually identical for men and women suggesting that gender differences in self-control are not a source of gender gaps in health-related behaviors, education, or labor market outcomes. This absence of a gender gap in adult self-control contrasts with the disparities found in girls' and boys' ability to self-regulate (see, e.g., Silverman, 2003; Matthews et al., 2009). Similarly, our results indicate that family background is a much weaker predictor of self-control in adulthood than in childhood (Miech et al., 2001; Noble et al., 2005). Together, these findings highlight the importance of population-representative empirical evidence for adults in enhancing our understanding of heterogeneity in self-control.

We then turn to consider the structural determinants of self-control. Our key contribution is to demonstrate that structural factors can have a causal effect in shaping a population's capacity for self-control. Exploiting the quasi-experimental variation generated by Germany's division after World War II, we document that, more than 30 years after Germany's reunification,

former East German residents have self-control scores that are 0.25 standard deviations (std.) higher than former West German residents. This is a substantial effect that corresponds to the same effect as being 15 years older. It is, perhaps, not surprising that living in the suppressive, communist regime of the former German Democratic Republic (GDR) has led to higher levels of self-control. After all, GDR residents had to gauge carefully what they said and did in order to avoid risking their educational and labor market prospects, protect their families, and preserve their individual freedom (Fulbrook, 2008). Investigating this potential mechanism further, we analyze the consequences of local-level government surveillance as a specific measure of the regional differences in the level of suppression. Specifically, we match data on the number of so-called 'unofficial informers' for the GDR's Ministry for State Security to the SOEP-IS data. We find that, at the local level, a higher number of unofficial informers is associated with an increase in self-control, providing suggestive evidence that the suppression in the GDR is a possible driver of the higher levels of self-control among former East German residents in comparison to their counterparts in West Germany.

Reforms of the German education system allow us to investigate whether the amount of compulsory education provided to students has a causal effect on their self-control. School environments inherently involve a certain amount of self-discipline. A priori, it seems possible that a longer exposure to the school environment may strengthen an individual's self-control. If so, this provides an important explanation for the well-documented positive association between people's capacity for self-control and their educational attainment (see, e.g., Alan and Ertac, 2015; Cobb-Clark et al., 2022). Establishing the direction of causality is empirically challenging, however. We make progress by using a series of educational reforms that exogenously increased the compulsory years of schooling. Using a generalized difference-in-differences and an instrumental variable approach, we find that longer exposure to the school environment did not result in greater self-control. This suggests that educational attainment may be a consequence rather than a cause of self-control. Additional exposure to the school environment seems to be not enough to raise students' capacity for self-control.

## 2 Data

### 2.1 The Socio-Economic Panel Innovation Sample

Our analysis draws on data from the Innovation Sample of the German Socio-Economic Panel (SOEP). The SOEP is an annual household panel survey that is representative of the German population (Goebel et al., 2019). Its Innovation Sample (SOEP-IS) was established in 2011 as a means of exploring new and novel survey items (Richter and Schupp, 2015). In 2017, SOEP-IS respondents were for the first time administered the Brief Self-Control Scale (BSCS) (see Tangney et al., 2004), making the SOEP-IS the first of only two large-scale population-representative datasets to contain a direct measure of people's trait self-control.[1]

Our data make it possible to study self-control at the population level. Previous studies have linked self-control to behavior using cohort-representative data sources such as the National Longitudinal Survey of Youth (NLSY; Nofziger, 2008; Perrone et al., 2004) and the Health and Retirement Study (HRS; Mezuk et al., 2017; Schlafmann, 2020). In contrast, the SOEP-IS data are representative of the entire adult population by design. Moreover, the BSCS has been psychometrically validated and is specifically designed to capture trait self-control across domains (Tsukayama et al., 2012; Tangney et al., 2004). Lacking similar measures, previous researchers have instead turned to proxies of self-control derived from domain-specific measures of behavioral and attitudinal problems. Embedding the BSCS in a rich panel study, like the SOEP, allows us to study the role of key demographic characteristics such as age, gender, and parental background in shaping adults' self-control.[2] Finally, changing political regimes and reforms to the education system make Germany a particularly interesting context for studying the structural determinants of self-control.

Our estimation sample is selected as follows. In 2017, 2,090 SOEP-IS respondents first surveyed in 2012 and 2013 were administered the BSCS. We necessarily drop 129 respondents (6.2 percent) who failed to provide complete information for all 13 items of the BSCS. This leaves us with a final estimation sample of 1,961 individuals from 1,269 households that we augment with additional biographic data.[3]

[1] The other is the Household, Income and Labour Dynamics in Australia (HILDA) Survey.

[2] Details of our measures are discussed in the relevant sections below. Table A1 in the Appendix provides an overview.

[3] In Appendix A.1 of Cobb-Clark et al. (2022), we show that our final estimation sample of respondents with valid BSCS scores is also population-representative.

### 2.2 The Brief Self-Control Scale

Following the standard approach for measuring personality traits in personality psychology and economics (e.g., Almlund et al., 2011; Borghans et al., 2008; Heckman et al., 2021), we construct a measure of trait self-control using responses to a validated battery of questions about people's capacity for self-control.[4] Survey-based measurement has the advantage of allowing us to embed our self-control measure in a population-representative survey. Meta-analysis reveals that survey-based measures of self-control also have the advantage of having higher convergent validity than do task-based measures (Duckworth and Kern, 2011).

More precisely, we construct a measure of trait self-control using the brief (13-item) version of the well-established Self-Control Scale (Tangney et al., 2004). The 13-item scale is highly correlated with the full 36-item version (Tangney et al., 2004), but is more suitable for large-scale representative surveys. The BSCS is designed to be domain-general (Tangney et al., 2004) and indeed has high predictive validity across remarkably diverse domains (de Ridder et al., 2012), internal consistency (Tangney et al., 2004; Bertrams and Dickhäuser, 2009), as well as test–retest reliability both after three (0.87, Tangney et al., 2004) and seven weeks (0.82, Bertrams and Dickhäuser, 2009).

The 13 items in the BSCS are introduced by the following instructions: "Using the scale provided, please indicate how much each of the following statements reflects how you typically are." People respond using a five-point Likert scale ranging from 1 ("not at all") to 5 ("very much"). Items assess, for example, whether people can resist temptation, can work towards long-term goals, or are lazy. Table 1 provides a list of all 13 items. Importantly, most of the BSCS items do not specifically refer to self-control, reducing the chances of deliberate non-response or social desirability-induced response bias. We construct an aggregate measure of self-control by standardizing each individual item and summing over all 13 items. To facilitate

[4] An alternative approach models self-control limitations as time-inconsistent discounting (e.g., Phelps and Pollak, 1968; Laibson, 1997; O'Donoghue and Rabin, 1999). In order to measure self-control limitations, people's incentivized choices in economic experiments are observed when they are confronted with monetary or effort trade-offs over time in multiple price list or convex time budget elicitation tasks (e.g., Andreoni and Sprenger, 2012; Andreoni et al., 2015; Augenblick et al., 2015; Augenblick and Rabin, 2019). See Cobb-Clark et al. (2022) for a more detailed discussion of the two measurement approaches and conceptualizations of self-control problems. In Cobb-Clark et al. (2021), we show that the survey-based BSCS used in the present study characterizes individuals in a way that is consistent with the conceptual framework of O'Donoghue and Rabin (1999).

interpretation, we standardize the aggregate scores to be mean zero and standard deviation one.[5] Figure A1 in the Appendix lists each item and shows the distribution of both the individual items as well as the aggregated self-control score, highlighting a substantial degree of variation in self-control.

**—Insert Table 1 here—**

## 3 Individual-Level Determinants of Self-Control

We begin by investigating several key individual-level determinants of self-control, focusing in particular on people's demographic characteristics and family background. We center our attention on these factors because they are arguably exogenous, giving our estimates a causal interpretation. Previous studies of the role of age, gender, and parental education in shaping self-control are largely based on samples of children or adolescents. In contrast, our analysis investigates their role in shaping self-control in the adult population.

We regress our measure of self-control simultaneously on four individual-level determinants: age, gender, and paternal and maternal education. The resulting estimates are reported in Table 2.[6] Column 1 presents our baseline results, while the estimates in column 2 additionally control for fixed effects capturing people's state of residence in 2017.

**—Insert Table 2 here—**

Previous research on the development of self-control as people age largely focuses on children and adolescents.[7] The way that self-control evolves into middle- and old-age remains a matter of debate. Our results suggest that age is an important determinant of adults' self-control as well; each additional year of age results in a 0.016 std. increase in the capacity for self-control. Moreover, the relationship between age and self-control is highly linear across the life cycle (i.e., ages 17 to 92). In particular, Figure 1 depicts the point estimates from a regression of

[5]Standardizing only the aggregated score, but not the single items before aggregation, does not change the results.

[6]Appendix Table A2 displays the correlations between self-control and these determinants.

[7]Many of the specific skills necessary to exercise behavioral self-control begin to emerge in infancy with some (e.g., inhibitory control) developing earlier than others (e.g., executive skills) (Lengua et al., 2015). Most children rapidly develop the capacity for self-control between the ages of 3 and 7 (Montroy et al., 2016). Still, during adolescence the development of self-control is not a linear process. Adolescent-specific changes in brain circuity leave teenagers more sensitive to environmental cues (both negative and positive)—and less able to regulate their responses—than both children and adults (Casey, 2015).

self-control on all controls (similar to column 2 of Table 2) and a full set of three-year age bins. Given the cross-sectional nature of our data, it is not possible to identify age effects separately from cohort effects. Nonetheless, the strong linearity in the age pattern of self-control suggests that the relationship is largely driven by aging rather than a selection effect in which those with higher self-control are less likely to die prematurely. Adults' capacity for self-control appears to continue to evolve even as they enter old age. The linear increase of self-control in age in a nationally representative, adult sample is evidence in favor of the hypothesis by Gottfredson and Hirschi (1990, p. 107) in their General Theory of Crime that self-control is likely to increase with age since "socialization continues to occur throughout life".

**—Insert Figure 1 here—**

There is also evidence of gender differences in children's capacity for self-control. Gender gaps in self-control emerge as early as age 3 (Cameron Ponitz et al., 2008), persist into kindergarten (Matthews et al., 2009), and at least partially explain why eighth-grade girls receive higher grades despite doing worse on IQ and only marginally better on standardized achievement tests (Duckworth and Seligman, 2006). In contrast, we find no evidence of gender differences in self-control among the adult population (see Table 2). The slight developmental advantage that girls have during childhood and adolescence in self-regulation and delaying gratification (Silverman, 2003) is no longer evident once they reach adulthood. Interestingly, Falk et al. (2018) find that adult women are less, not more, patient than men on average across the world, though the gender disparities are small.

Although there is evidence that children's capacity for self-control increases with maternal education and families' socioeconomic status (Miech et al., 2001; Montroy et al., 2016; Noble et al., 2005; Evans, 2003), our results indicate that the effect of parental years of education is statistically insignificant, suggesting that family background may be a much weaker predictor of self-control in adulthood than it appears to be in childhood.

Importantly, the estimated effects of age, gender, and parental education are not sensitive to controlling for state of residence in 2017 fixed effects (see column 2, Table 2). Taken together, our results highlight that some of the disparity in self-control associated with family background and gender that is commonly observed in childhood disappears in adulthood.

## 4 The Structural Determinants of Self-Control

Our analysis of the structural determinants of self-control extends the previous literature and provides a novel perspective on the broader influences shaping a population's capacity for self-control. Understanding the influence of structural factors requires that we observe structural change. Consequently, we exploit two quasi-experiments—Germany's division after World War II and reforms of Germany's compulsory schooling—to draw causal inferences about the potential for structural factors to influence self-control.

### 4.1 The Division of Germany after World War II

To investigate the role of the broader political, economic, and social context in shaping self-control, we exploit the quasi-experiment generated by Germany's division after World War II (see, e.g., Alesina and Fuchs-Schündeln, 2007; Becker et al., 2020, for a more detailed discussion). For more than four decades, between 1949–1990, Germany was divided into two separate states consistent with the zones the Allied Forces controlled after World War II. The United States, the United Kingdom, and France established the Federal Republic of Germany—a liberal democracy with a free market—in the Western part of the occupied zones. The Soviet Union, on the other hand, established the German Democratic Republic (GDR) in the Eastern part of the occupied zones—a communist state with a one-party system and a state-run, plan-based economy. This division had fundamental consequences for the way that East and West Germans' lives unfolded after the war.

As our estimation sample includes respondents who lived in both states until Germany's reunification in 1990, we are able to compare the level of self-control between people who were subject to vastly different political, social, and economic systems.

#### 4.1.1 Self-Control in the German Democratic Republic

We begin by regressing self-control (measured in 2017) on indicators for the place of residence in 1989 (i.e., shortly before the fall of the Berlin Wall) and age in column 1 of Table 3. The omitted reference category in Table 3 is having lived in West Germany in 1989 and we consider indicators for: (i) having lived in East Germany in 1989 (i.e., in the former GDR); (ii) having lived outside of Germany in 1989; and (iii) being born after 1989.[8] We find that those who were

[8] We exclude eight respondents for whom the place of residence in 1989 is unknown.

GDR residents in 1989 have significantly higher levels of self-control even about 30 years after Germany's reunification than those who were not. Specifically, former East German residents have self-control scores that are 0.25 std. higher than former West German residents (column 1 of Table 3), which corresponds to the same effect as being 15 years older. Those who lived outside of Germany score 0.20 std. higher than former West German residents. 96 percent of this group are people with an immigrant background who only moved to Germany after 1989. The remaining 4 percent are likely to be German expatriates who returned after 1989.

**—Insert Table 3 here—**

One possibility is that people's self-control is heightened the longer they were exposed to the GDR regime. We investigate this possibility by examining self-control patterns across birth cohorts. To the extent that it is the length of exposure that matters, we would expect the East-West gap in self-control to be greatest for people born earliest. We test this hypothesis by re-estimating our models allowing for an interaction between age (continuous) and residential location in 1989 in Table 3. We find that the estimated age–location interaction is positive, but statistically insignificant and economically unimportant, ruling out a simple linear exposure story (see Table 3, column 2).

Alternatively, self-control may respond to the specific circumstances in which one grows up. Figure 2 depicts the difference in the average self-control of former East versus West Germans, by birth cohort, highlighting four distinct historical periods in which each was born: (i) before 1949 (when the GDR was established); (ii) 1949–1961 (when the Berlin Wall was built); (iii) 1962–1990 (while the Berlin Wall existed, the Berlin Wall fell in November 1989); and (iv) post-1990 (when Germany was reunified).[9] We find that the East-West gap in self-control is absent only among those born after Germany's reunification.[10] Otherwise, East-West differences in self-control appear remarkably constant across birth cohorts.

**—Insert Figure 2 here—**

[9]For those born after the reunification, we use the place of residence in 2017 to assign them to either East or West Germany in Figure 2. People who lived outside of Germany in 1989 are excluded in Figure 2.

[10]As Becker et al. (2020) stress, such a quick convergence after reunification is reassuring as it suggests that possible concerns about pre-World War II differences between the two German states and pre-1961 German-German migration that could challenge the interpretation of Germany's division as a natural experiment are less warranted.

The effect of having lived in the GDR on self-control remains significant even when conditioning on respondents' current location (see Table 3, columns 3 and 4). Thus, it appears that it is residence in the GDR—irrespective of its length or timing—which is linked to higher self-control. We can only speculate about reasons for this finding. It may be, for example, that the penalties for having limited capacity for self-control may have been sufficiently high to give people an incentive to quickly develop the capacity for self-control.

Our results are consistent with previous evidence that Germany's division had a role in influencing important aspects of people's preferences and personality.[11] Our finding that living in the GDR also heightened people's capacity to exercise self-control is new, though perhaps not surprising. The GDR was characterized by civic suppression. Given the regime's high level of control over education, employment, and even private consumption (i.e., the state granted the right to buy a car), the regime could—and did—use its power to punish people who openly criticized the state or the ruling Socialist Unity Party (SED). Many parents therefore encouraged their children to avoid the potential penalties imposed by the GDR's regime by suppressing their own thoughts and desires (Jahn, 2014). There was an incentive for people to be circumspect in their options and behavior so as not to jeopardize their educational and career opportunities, preserve their individual freedom (Bruce, 2012; Fulbrook, 2008), and protect their families and friends (Jahn, 2014).

#### 4.1.2 The Role of Local Government Surveillance

Civic suppression represents one important channel through which living in the GDR may have affected self-control. We investigate this possibility by considering the relationship between self-control and the level of local government surveillance, a key measure of suppression. We measure local government surveillance by the number of so-called 'unofficial informers' (*Inoffizielle Mitarbeiter*) of the Ministry for State Security of the GDR, also known as the Stasi. Unofficial informers were ordinary citizens who, while living their daily lives, also reported the

[11] Previous research on preferences and other personality traits has documented disparities between East and West Germans in their preferences for: redistribution and state intervention (Alesina and Fuchs-Schündeln, 2007); solidarity (Ockenfels and Weimann, 1999; Brosig-Koch et al., 2011); trust and reciprocal behavior (Lichter et al., 2021b), trust, cooperation and risk attitudes (Rainer and Siedler, 2009; Heineck and Süssmuth, 2013; Neugart, 2016); locus of control, neuroticism, conscientiousness, impulsivity, and openness (Friehe and Pannenberg, 2020; Friehe et al., 2015; Kleinjans and Gill, 2018).

behavior of their co-workers, neighbors, friends, and even family members to their local contact person at the Stasi. While some reports centered on criticism of the local party representative or the party leadership, others were based on more personal matters. There was a great deal of geographic diversity in the concentration of unofficial informers. Specifically, the number of unofficial informers in the 1980s ranged between less than one and more than 10 per 1,000 inhabitants across the GDR counties (see Figure 3).

**—Insert Figure 3 here—**

The number of unofficial informers comes from Lichter et al. (2021a,b) who use data from Müller-Enbergs (2008) and include recent updates.[12] Yearly information is not available due to missing years; consequently, the measure captures the average number of informers over the years 1980–1988. At the same time, evidence indicates that the number of informers was quite stable over this time period, suggesting that little is lost by ignoring any time variation. Information on the number of unofficial informers is available at the level of the GDR counties (see Figure 3). Since we do not have direct information on the respondents' county of residence before they entered the SOEP sample (only after reunification), we use the current county of residence as a proxy (accessed via SOEPremote, a special mode of online access for sensitive information). Naturally, we restrict the sample to only those currently residing in a federal state that belonged to the GDR and also reporting to have lived in the GDR in 1989. This way, we can match 299 individuals in our data who lived in East Germany in 1989; an additional 16 individuals cannot be matched due to county border reforms that lead to incongruencies in the two sources of data (government surveillance intensity and SOEP-IS).

The average number of unofficial informers across the individuals in our sample is 3.3 per 1,000 inhabitants. While this may not seem like a particularly large number, each unofficial informer could spy on dozens of people. Moreover, as nobody knew who worked as an unofficial informer, the existence of this large network of informers resulted in a system of widespread mistrust in the society. In fact, this mistrust was not only a byproduct of the surveillance, but part of the suppression itself.

The relationship between government surveillance intensity and self-control is plotted in Figure 4. The figure shows (on the $y$-axis) the average self-control of individuals experiencing

[12] A government agency, the Stasi Records Agency, has restored shredded Stasi files on informers and their targets.

the informer density falling into the specific bin given on the $x$-axis.[13] The size of the circles reflects the relative number of observations (shown inside each circle) in each surveillance density bin. The estimate of linear fit stems from a regression model where the dependent variable is standardized self-control and the variable of interest is the number of unofficial informers per 1,000 inhabitants. We control for year of birth fixed effects and federal state fixed effects. Age is not only a determinant of self-control (see Table 2), but may also vary across counties. For instance, more rural counties may have an older population and the need to operate large networks of unofficial informers might have been less urgent in those counties. Controlling for state fixed effects is a conservative approach that minimizes the chances that variation in government surveillance intensity is picking up other regional variation that may correlate with self-control.

**—Insert Figure 4 here—**

Informer density and self-control are positively related. The estimated coefficient of unofficial informer density is 0.159 ($p = 0.033$) (see Table 4). That is, an increase in surveillance by one unofficial informer per 1,000 inhabitants, about one-third of the mean, is associated with an increase in average self-control of about 0.16 std. The pattern in Figure 4 confirms that the relationship between the number of unofficial informers and self-control is not driven by outliers and is generally linear within the usual range of the unofficial informer density. Using a quadratic specification to capture the relationship between unofficial informers and self-control does not provide a better fit compared to the linear model (results available upon request).

**—Insert Table 4 here—**

Re-estimating our model using a standardized measure of unofficial informers allows us to draw comparisons between our results and a handful of studies that investigate the effect of government surveillance in the GDR on personality traits. These results are reported in the bottom row of Table 4. We find that a one standard deviation increase in the number of

[13]To account for control variables in the figure, we make use of the Frisch–Waugh–Lovell (FWL) Theorem. This is, we regress self-control and the number of unofficial informers separately on the year of birth and state fixed effects and take the residuals of self-control and the unofficial informer density. The self-control residual is again standardized and plotted on the $y$-axis. We add the mean value of the unofficial informer density to the unofficial informer density residual to ensure a meaningful range of the $x$-axis. The coefficient of the unofficial informer density residual on the self-control residual (plotted in Figure 4) is identical to the coefficient of the unofficial informer density when self-control is regressed on the unofficial informer density and year of birth and state fixed effects (reported in Table 4).

unofficial informers relates to a 0.19 std. increase in self-control. Our effect size is roughly comparable to Lichter et al. (2021b) who find that a one standard deviation increase in surveillance density decreases individuals' reciprocal behavior by 0.18 and trust by 0.10 std. In contrast, surveillance is not significantly related to locus of control or negative reciprocity (Friehe et al., 2015).

In our view, the positive correlation between more intensive government surveillance and greater capacity for self-control is suggestive evidence that the suppression in the GDR is a possible driver of the higher levels of self-control among former East versus West German residents. This result is not causal, of course. First, we exploit only geographic variation in the intensity of surveillance across GDR counties and the density of informers may itself be a result of dissident behavior in the local area (see Lichter et al., 2021b). Although this cannot be ruled out, the number of informers over time (where available) is rather constant (Lichter et al., 2021b). In our view, this lack of adjustment over time makes it less likely that the number of informers is a function of the local tendency to dissent (Friehe et al., 2015). Second, we assign the unofficial informer density to individuals using their county of residence in 2017. If the tendency to relocate after reunification correlates with self-control, our point estimate might be biased. However, if people with higher self-control were more likely to relocate, our estimate would provide a lower bound of the true effect. It is also the case that our sample size of approximately 300 people makes a certain degree of caution inevitable when interpreting the results. For these reasons, we view the heightened self-control associated with increasing population surveillance as suggestive evidence that civic suppression may partly drive the overall difference in the self-control of East and West Germans.

It is important to stress, however, that government surveillance, although one important aspect of the GDR's political system, is by far not the only way that East and West Germany differed. In East Germany, product choice was more limited, for example, while travel was restricted and incomes were lower. All of these may have contributed to people developing more self-control simply because they could not consume as freely as many of their West German counterparts growing up in a capitalist society.

#### 4.1.3 Discussion

In sum, the comparison of East and West Germany provides insights into the compound structural effect of having lived in the GDR. While the intensity of government surveillance is one plausible mechanism explaining higher self-control among residents of the former GDR, self-control does not vary systematically with length and timing of exposure to the GDR. Thus, it may be that people's self-control adapted quickly to the broader context in which they were operating.

It is important to note, however, that any increased capacity for self-control among former GDR residents does not imply that we should necessarily observe better life outcomes for them today. Self-control is one significant predictor of life outcomes among many others (see, e.g., Cobb-Clark et al., 2022) such that our results are not incompatible with the observation that today East Germans face, on average, lower wages, a higher unemployment rate, and lower household wealth, for example (BMWi, 2017). East Germans' higher levels of self-control seem to at most only partly compensate for other more adverse circumstances such as macroeconomic conditions.

### 4.2 The Compulsory Schooling Reforms

We now turn to consider whether people's self-control is shaped by the amount of education they have received. Although people's capacity for self-control is almost certainly a key factor in their educational attainment (Duckworth et al., 2010), it is also possible that a longer exposure to the school environment—in which there are high returns to discipline (being on time, not skipping class, doing homework)—strengthens students' self-control. Accounting for this type of reverse causality is empirically challenging. We are able to make progress, however, by analyzing a series of structural reforms of the German school system: Between 1956 and 1969, West German states implemented reforms that introduced a mandatory ninth grade in basic schools. This allows us to exploit arguably exogenous variation in the time people spend in compulsory schooling.[14]

[14] Aside from the school environment per se, the content of what is taught may also enhance the formation of self-control. However, the curricula in the additional ninth grade of German basic schools, i.e., the extension of compulsory schooling that we study, do not explicitly mention non-cognitive skills or personality development. They have a strong focus on political upbringing, political history, and European integration (Margaryan et al., 2021, online appendix).

Compulsory schooling reforms are a common feature of the educational systems in many industrialized countries (see, e.g., Harmon, 2017, for a review). In Germany, the governments of the federal states—as opposed to the federal government—are in charge of education policy. Before the reforms in Germany, students attending basic school (*Hauptschule*) in a certain state graduated after a total of eight years of education: four years of elementary schooling from the age of 6 to 10 plus four years in basic school. Alternatively, students graduating with the compulsory schooling reforms in place were required to take a total of nine years of schooling before they were allowed to leave school.[15]

The discussion to introduce a mandatory ninth grade for basic school students in Germany dates back to the immediate aftermath of World War II. Weak labor market conditions were the main reason for extending the time spent in school initially, e.g., when the first states implemented their compulsory schooling reforms in 1949. By the time more states introduced the mandatory ninth grade on a state-wide level, the economic situation was no longer the main reason (see Pischke and von Wachter, 2008); a lack of maturity in 14-year-olds and an increased demand for skilled workers had become more pressing issues. In the so-called Hamburg Accord of 1964, the prime ministers of all West German states agreed to introduce a compulsory ninth grade and all states had introduced it by 1969. The staggered introduction results in a series of reforms, one in each of the eleven West German states.

Our analysis is constrained to West Germans as they were affected by the education reforms. The timing of the reforms across West German states is presented in Table 5. The first graduation year affected by the introduction of the ninth grade in basic schools is reported in column 1. As children begin school at the age of six in all states, basic school graduation before the reforms usually occurred around age 14. We can use state and birth cohort information (see column 2) to identify whether a person was affected by the introduction of the ninth grade.[16] Although basic schools constituted only one of the three secondary school

[15]Compulsory schooling legislation in Germany does not require students to receive a graduation degree. Instead, students have to stay in school for the number of years it regularly takes to receive a basic school graduation degree. In other countries, students are not allowed to leave school before a certain age (e.g., in the UK) or compulsory school attendance is set through the school starting age and child labor laws (e.g., in the US). Despite the different ways of implementation, changes in these requirements are referred to as compulsory schooling reforms.

[16]We do not know respondents' state of residence when in school and use the current state of residence as a proxy instead. As noted by Pischke and von Wachter (2008), people with basic

tracks in West Germany in the period under consideration, most students (around 77 percent) attended basic schools, see column 3 of Table 5. The other two school tracks—intermediate schools (*Realschulen*)[17] and academic schools (*Gymnasien*)—required 10 and 13 years of total schooling for graduation, respectively, throughout the entire period. Individuals are considered to be affected by the compulsory school law change if they currently live in the state listed in the leftmost column of Table 5 and were born in the year stated in column 2 or later. Since our data only include the current state of residence, but no retrospective information on the state of birth or school graduation, we measure compulsory schooling exposure with error. To minimize any missassignment, we drop respondents who report having lived outside West Germany in 1989. In columns 4 and 5, we report the overall number of observations and the number of observations affected by the reforms across states.

**—Insert Table 5 here—**

#### 4.2.1 Estimation Sample

Our estimation sample is selected as follows. We (i) include all individuals born in the interval of $\pm 15$ years around the first cohort affected by the reform in their state; (ii) exclude individuals born before 1940 (even if this falls into the 15 year window around the relevant reform) because they started schooling in the Third Reich and may be less comparable to individuals born later; (iii) exclude individuals from the "city states" of Berlin, Bremen, and Hamburg as those states consist of a single city only, making the reform assignment based on the state of residence in 2017 less reliable; (iv) exclude individuals from the states of Saarland and Schleswig-Holstein as all observed respondents were affected by the reforms in these rather small and less populated states;[18] and (v) only include individuals who either attended a basic school (*Hauptschule*) or an intermediate school (*Realschule*). While only the former school type was directly affected by the reforms, previous evidence suggests that some students may have decided to attend an

schooling have a lower tendency to move out of their home state relative to those with higher levels of education.

[17]This group also includes comprehensive schools (*Gesamtschulen*), which were, however, numerically irrelevant in the time period we consider.

[18]The generalized difference-in-differences and instrumental variable estimators we present below would allow us to include these respondents, but they would serve as a control rather than treatment group. The state fixed effects would absorb the treatment status, see Goodman-Bacon (2021).

intermediate school as a result of the reforms (Cygan-Rehm, 2022). These selection rules result in an estimation sample of 415 respondents.

#### 4.2.2 Difference-in-Differences Analysis

One can think of these schooling reforms in a generalized difference-in-differences setting of the following form:

$$\text{Self-control}_{ist} = \beta_0 + \beta_1 \text{Z}_{ist} + \beta_2 \text{F}_{ist} + \boldsymbol{\gamma}_s + \boldsymbol{\omega}_t + \varepsilon_{ist}, \tag{1}$$

where $\text{Z}_{ist}$ is an indicator (=1) if an individual $i$ in state $s$ in year $t$ was affected by its state's compulsory schooling reform, and 0 otherwise. In addition, $\boldsymbol{\gamma}_s$ and $\boldsymbol{\omega}_t$ are state and year fixed effects, respectively. With this specification, each untreated individual is used in the control group for treated individuals. This avoids arbitrary choices in the selection of a control group. Finally, we follow the literature on compulsory schooling reforms in Germany and also control for an indicator for whether the respondent is female, $\text{F}_{ist}$.

This generalized difference-in-differences model has the same interpretation as the 2×2 difference-in-differences estimator, where the outcome is regressed on a treatment group indicator (captured by the state fixed effects in the generalized version), a post-reforms indicator (captured by the year fixed effects), and a treatment–post interaction term (equivalent to $\text{Z}_{ist}$ in equation (1)). Using the generalized difference-in-differences estimator has the advantage that it yields a single point estimate that is based on a larger pooled sample rather than several state-specific estimates.

Difference-in-differences estimates of the impact of the ninth basic school grade on self-control using our preferred estimation sample are reported in column 1 of Table 6. The point estimate of −0.004 is close to zero and not statistically significant, indicating that the introduction of the ninth grade had no effect on the self-control of people affected by the reforms. There is a lack of estimation precision given our sample size (n = 415), however, implying that the 95 percent confidence interval ranges from −0.760 to 0.751. Thus, we cannot exclude potentially large effect sizes in either direction. Still, a point estimate of basically zero suggests that the ninth grade in basic school does not seem to increase self-control.

**—Insert Table 6 here—**

### 4.2.3 Instrumental Variable Analysis

We now turn to studying the consequences of schooling reforms for people's self-control in more depth by using an instrumental variable (IV) approach. Results from the difference-in-differences analysis of compulsory schooling reforms described above are equivalent to those from the reduced-form in an IV approach. Specifically, in an IV approach, the compulsory schooling reforms indicator $Z$ acts as the instrument for years of schooling. In the first stage, we estimate the effect of the reforms on years of schooling, i.e., the endogenous outcome of interest. In the second stage, self-control is regressed on instrumented years of schooling. The difference-in-differences approach, in contrast, estimates the direct effect of the additional ninth grade on self-control in a single reduced-form estimation. Given the discrete nature of the reforms we consider, the interpretation of the reduced-form estimate is straightforward. The advantage of the IV analysis is that, by conducting the estimation in two separate stages, we may learn more about why additional compulsory schooling does not appear to result in increased self-control.

**Identification:** There are necessarily several maintained assumptions underpinning our IV model. As in the difference-in-differences approach, the IV approach requires that the self-control of those affected and unaffected by the schooling reforms follows the same time trend. Effectively, this common trend assumption requires that the schooling reforms were independent of self-control, i.e., we assume that reforms were not introduced earlier in some states because political decision-makers were worried about a decline in self-control.[19] This assumption is likely to hold. If non-cognitive skills were the intended target of the introduction of the ninth grade, they would likely have been included in the states' curricula which was, however, not the case (Margaryan et al., 2021, online appendix). To interpret the estimated effect of the reforms as the consequence of additional years of schooling on self-control, we also need to assume that the reforms affected self-control exclusively through years of schooling and not

[19]Several other studies also use the German compulsory schooling reforms to estimate the effect of education on non-pecuniary outcomes; see Kemptner et al. (2011), Dahmann and Schnitzlein (2019), and Begerow and Jürges (2021) for (mental) health, Cygan-Rehm and Maeder (2013) for fertility, Siedler (2010) and Margaryan et al. (2021) for political participation and preferences, respectively, and Kamhöfer and Schmitz (2016) for crystallized intelligence. A common assumption in this literature is that the reforms were not triggered by different trends in those outcomes across states over time.

through any other institutional change. As the reforms did not affect other aspects of the educational system (e.g., school funding or teacher qualification) beyond increasing the length of compulsory schooling, this exclusion restriction seems plausible.[20] We need to make two additional assumptions regarding the first stage. Specifically, the reforms needed to be relevant enough to cause changes in the years of schooling students got and all students affected by the reforms need to either have reacted in the same way (i.e., receiving more education) or not at all. Given that more than three-quarters of students were affected by the reforms (see Table 5) and that the reforms were binding, these maintained assumptions seem uncontroversial. Moreover, the estimated effect of the reforms in the first stage suggests that the compulsory schooling reforms are indeed a powerful predictor of the years of schooling that individuals in our sample received (see below).

**Results:** We implement our IV approach by estimating the following model using two-stage least squares (2SLS):

$$\mathrm{S}_{ist} = a_0 + a_1 \mathrm{Z}_{ist} + a_2 \mathrm{F}_{ist} + \boldsymbol{\gamma}_s + \boldsymbol{\omega}_t + \xi_{ist}, \tag{2}$$

$$\text{Self-control}_{ist} = \alpha_0 + \alpha_1 \hat{S}_{ist} + \alpha_2 \mathrm{F}_{ist} + \boldsymbol{\gamma}_s + \boldsymbol{\omega}_t + \epsilon_{ist}. \tag{3}$$

Equation (2) gives the first stage effect of the reforms indicator $Z$ on years of schooling $S$. Equation (3) gives the second stage effect of the first stage fitted values for years of schooling $\hat{S}$ on self-control. To see the connection to the difference-in-differences estimate, we can think of the second stage estimate as the Wald estimator that gives the (reduced form) effect of the compulsory schooling reforms on self-control, "normalized" by the (first stage) effect the reforms had on years of schooling.

Our results are presented in Table 6. The reforms of compulsory schooling resulted in schooling levels increasing by 0.689 years.[21] The first stage $F$-statistic is over 17 (well-above the rule-of-thumb value of 10), implying that the reforms were clearly relevant for years of

[20] Some of the birth cohorts affected by the compulsory schooling reforms may also have been affected by the independent, one-off implementation of a shorter school year to harmonize the school year calendar across states. We account for this in a robustness check in Table 7.

[21] The reforms had no effect on students who visited an intermediate school independent of the reforms (always-takers). They typically increased years of schooling for basic school students by one year or, in some cases, by two years if a student decided to visit an intermediate instead of basic school because of the reforms (the compliers of the reforms were potentially among both groups of students).

schooling. Thus, the lack of a reduced-form effect in the difference-in-differences analysis does not stem from the failure of the reforms to shift the years of schooling students received. Instead, the explanation lies in the fact that years of schooling are unrelated to self-control. Our second stage effect of years of schooling on self-control (column 3) is—like the difference-in-differences estimate—very close to zero ($-0.006$). Overall, the IV results confirm that years of schooling do not seem to increase self-control.[22]

#### 4.2.4 Robustness

We present the results of several robustness checks around both model specification and sample restrictions in Table 7. In panel (a), we add a control for the one-off implementation of so-called 'short school year' reforms that harmonized school year calendars across the states, see Pischke (2007). Our estimates remain very similar to those in our preferred specification. In panel (b), we add state-specific linear trends to the set of control variables, see Stephens and Yang (2014). Instead of using variation in schooling across states over time, adding state-specific linear trends equates to comparing deviations from a linear trend in school attainment across states over time. While the second stage and the reduced form estimates increase, they are still small and not statistically significant. In panel (c), we vary the sample. Specifically, we consider additional birth cohorts by increasing the time window around the first cohort affected by the reforms to $\pm 20$ years. This increases the sample size, but has the consequence that the earlier and later cohorts are now more likely to differ in ways other than being affected by the compulsory schooling reform. The point estimates of the second stage and reduced form remain very close to zero and are insignificant. Taken together, the results of these robustness checks are in line with those of our main analyses: estimated effects are statistically insignificant and economically small.

**—Insert Table 7 here—**

[22]Given the typical 2SLS setup with a first stage that is itself estimated, the 95 percent confidence interval is even wider than the one associated with the reduced-form difference-in-differences estimate. As before, this large confidence interval does not allow us to completely rule out the possibility of rather large effects.

#### 4.2.5 Discussion

Taken together, the evidence presented here provides no support for the notion that additional time spent in school increases self-control. This is consistent with previous evidence that within-individual changes in self-control over time predict subsequent changes in students' grade point averages, but not the reverse (Duckworth et al., 2010).

Of course, the absence of an effect of the additional ninth grade in basic school on self-control does not rule out the possibility that other aspects of education do enhance self-control. In particular, the nature of the reforms we consider imply that our results reflect the effect of additional schooling for students who were affected by the reforms, i.e., those who would have left basic school before completing ninth grade (Imbens and Angrist, 1994). Even though the students attending basic school made up about three-quarters of the German population attending school (see Table 5), this does not necessarily imply that additional intermediate, academic-track, or higher education does not increase self-control.[23] Perez-Arce (2017), for example, find that university education has a causal effect in increasing patience, pointing to a relationship between education and time preferences more generally. It is also possible that schooling has the potential to influence self-control through specific curricula and pedagogical choices or specific school-based interventions targeting self-control.

## 5 Conclusions

Self-control is shaped not only by people's personal circumstances and family background, but also by the broader context in which they live their lives. Political, economic, social, cultural, and environmental conditions and institutions all combine to either help or hinder people in making choices consistent with their long-term goals. Our interest lies in understanding the scope for structural factors to determine levels of self-control at a population level. We in-

[23] While basic schools were the most important secondary school track in terms of students, the number of intermediate and academic schools increased in the time under review, see Jürges et al. (2011) and Kamhöfer and Schmitz (2016). Regressing self-control on the number of intermediate and academic schools per 1,000 km$^2$ in a given state, the point estimates are 0.000 (standard error 0.022) and 0.017 (standard error 0.039), respectively. These reduced form estimates suggest that secondary schooling in intermediate and academic schools (as measured by the number of those schools per 1,000 km$^2$ in a given state) does not have a large effect on self-control either.

vestigate this issue by exploiting unique population-representative data on self-control and the quasi-experimental variation generated by key structural changes in Germany.

Our main contribution is to demonstrate that structural factors can have a role in determining people's self-control. This represents an important extension of previous studies focused solely on the individual, family, or neurological foundations of self-control. Specifically, we use the quasi-experiment generated by Germany's division to demonstrate that former East German residents have substantially higher levels of self-control than West Germans. Both share the same cultural and historical background, but lived in very different economic, political, and social environments for more than 40 years. We demonstrate that, among GDR residents, there is a positive correlation between more intensive government surveillance and heightened self-control indicating that civic suppression may be one mechanism behind East Germans' greater self-control. Our conclusion is that people seem to become more self-controlled if structural factors make the costs of low self-control particularly high. Interestingly, the higher levels of self-control among those East Germans who were socialized in the GDR persist nearly 30 years after reunification, even though structural factors in East and West Germany have largely converged over this period.

We also study the influence of structural factors on people's self-control through the lens of exposure to the educational system. Our investigation of a series of educational reforms which exogenously raised the years of schooling that students received reveals that additional compulsory schooling had no discernible effect on self-control. Thus, there seems to be no reverse causality between years of schooling and self-control, suggesting that educational achievement is a consequence rather than a determinant of self-control. Our take-away is that, while enhancing self-control through school-based interventions is feasible, success is likely to rely on specifically tailored curricula (Alan and Ertac, 2018; Alan et al., 2019; Breitkopf et al., 2022; Piquero et al., 2016; Sorrenti et al., 2020). Simply increasing the length of time students are required to spend in formal education seems to be insufficient in increasing the capacity for self-control.

Our focus on the self-control capacity of adults also represents an important contribution. Previous studies have largely focused on heterogeneity in children's and adolescents' self-control. Our results highlight that some of the disparity in self-control associated with family back-

ground and gender that is commonly observed in childhood disappears in adulthood. Given the numerous choices that adults make not only for themselves, but also for their families, employers, and communities, it is imperative that we study the determinants of self-control in adult populations as well as in children.

In this regard, our contribution is enhanced by the fact that we provide—for the first time—population-representative evidence on the determinants of self-control. Population-representative data are extremely useful in shedding light on how self-control develops over the life cycle. We find that older people have higher levels of self-control which is good news for aging societies given the link between greater self-control and improved health, better labor market performance, reduced criminality, and greater overall well-being (see, e.g., Cobb-Clark et al., 2022; Duckworth and Seligman, 2005; Kaur et al., 2015; Moffitt et al., 2011; Tangney et al., 2004).

Future research exploring these conclusions in more depth and in other contexts would be especially valuable. We need to know more about the range of structural factors that are likely to determine self-control. The COVID-19 pandemic, for example, may leave an imprint on people's self-control since the public-health measures adopted to fight its spread (e.g., social distancing, face masks, etc.) require a degree of constant self-control. More broadly, we need to understand the potential for policy initiatives to enhance self-control by redefining the "rules of the game" and altering the structural context in which people operate.

## Tables

Table 1: Brief Self-Control Scale

| | Item |
|---|---|
| 1. | I am good at resisting temptation. |
| 2. | I have a hard time breaking bad habits. (reversed item) |
| 3. | I am lazy. (reversed item) |
| 4. | I say inappropriate things. (reversed item) |
| 5. | I do certain things that are bad for me, if they are fun. (reversed item) |
| 6. | I refuse things that are bad for me. |
| 7. | I wish I had more self-discipline. (reversed item) |
| 8. | People would say I have iron self-discipline. |
| 9. | Pleasure and fun sometimes keep me from getting work done. (reversed item) |
| 10. | I have trouble concentrating. (reversed item) |
| 11. | I am able to work effectively towards long-term goals. |
| 12. | Sometimes, I cannot stop myself from doing something, even if I know it is wrong. (reversed item) |
| 13. | I often act without thinking through all the alternatives. (reversed item) |

*Notes:* The Brief-Self-Control Scale is taken from Tangney et al. (2004). Questions marked as "reversed item" enter the final self-control score reversed. The questions are asked in two blocks (block 1: questions 1–6 and 9–13; block 2: questions 7 and 8) separated by other questions. Figure A1 in the Appendix presents the distribution of responses to the items.

Table 2: Individual-level determinants of self-control

| | (1) | (2) |
|---|---|---|
| | Dependent variable: self-control | |
| | without controls | with state FE |
| Age (in years) | 0.016*** | 0.016*** |
| | (0.001) | (0.001) |
| Female | 0.041 | 0.045 |
| | (0.040) | (0.040) |
| Dad > basic school | 0.026 | 0.016 |
| | (0.056) | (0.056) |
| Mom > basic school | −0.032 | −0.044 |
| | (0.057) | (0.058) |
| Constant | −0.869*** | −0.859*** |
| | (0.089) | (0.110) |
| State fixed effects | | yes |
| Observations | 1961 | 1961 |
| Adj. $R^2$ | 0.09 | 0.10 |

*Notes:* Own calculations based on SOEP–IS, wave 2017. The dependent variable, self-control, is standardized to mean 0 and standard deviation 1. In column 1, we regress self-control on the variables stated on the left of the table using OLS and no further control variable. In column 2, we additionally control for state of residence in 2017 fixed effects. Standard errors clustered at household level in parentheses; $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

Table 3: Structural determinants of self-control: East versus West Germany

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Dependent variable: self-control | | | |
| | Specification | | | |
| | baseline | age–East interaction | current region | current state FE |
| Age (in years) | 0.017*** | 0.016*** | 0.017*** | 0.017*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Lived in 1989: East | 0.247*** | 0.043 | 0.257*** | 0.244*** |
| | (0.060) | (0.216) | (0.081) | (0.089) |
| Lived in 1989: outside of Germany | 0.198** | 0.188** | 0.199** | 0.200** |
| | (0.086) | (0.086) | (0.087) | (0.087) |
| Lived in 1989: not yet born | 0.091 | 0.060 | 0.093 | 0.088 |
| | (0.093) | (0.098) | (0.094) | (0.095) |
| Age × lived in East in 1989 | | 0.004 | | |
| | | (0.004) | | |
| Currently residing in East | | | −0.012 | |
| | | | (0.072) | |
| State fixed effects | | | | yes |
| Observations | 1953 | 1953 | 1953 | 1953 |
| Adj. $R^2$ | 0.10 | 0.10 | 0.10 | 0.10 |

*Notes:* Own calculations based on SOEP-IS, wave 2017. In column 1, we regress self-control on age and the three place of residence in 1989 indicators stated on the left using OLS. The reference category is "having lived in West Germany in 1989". Column 2 replicates the specification in column 1, but additionally controls for an age-in-years–East interaction. In column 3, we instead control for an indicator whether the respondent lives in an East German state in 2017 (otherwise 0). Column 4 controls for the specific state of residence in 2017. Compared to the results in Table 2, we lose eight out of a total of 1,961 observations because of missing information on the place of residence in 1989. Standard errors clustered at household level in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Relationship between self-control and government surveillance intensity in the GDR

| **Linear regression specification**<br>Dep. variable: std. self-control | (1)<br>Coefficient | (2)<br>$p$-value |
|---|---|---|
| 1 Informer density (number of informers per 1,000 inhabitants) | 0.159 | 0.033 |
| 2 Std. informer density | 0.188 | 0.033 |

*Notes:* based on SOEPremote information of SOEP-IS, wave 2017, and data on the number of Stasi informers taken from Lichter et al. (2021a) (based on Müller-Enbergs, 2008). 299 observations. The dependent variable is the mean 0, standard deviation 1 standardized Brief Self-Control Scale. The first row shows the relationship between self-control and the number of Stasi informers per 1,000 inhabitants in the assigned county of residence using a regression model with year of birth and federal state fixed effects. The bottom row states the coefficient of Stasi informer density when regressing standardized self-control on the standardized number of informers per 1,000 inhabitants (mean 0, standard deviation 1) in a model with year of birth and state fixed effects. $p$-values are calculated based on county-clustered standard errors.

Table 5: Introduction of the compulsory ninth grade for basic schools

| State | (1) First graduation year affected | (2) First birth cohort affected | (3) Share of students in basic schools (in %) | (4) All obser-vations | (5) Treated obser-vations |
|---|---|---|---|---|---|
| Baden-Württemberg | 1967 | 1953 | 77.3 | 67 | 32 |
| Bavaria | 1969 | 1955 | 81.1 | 100 | 50 |
| Bremen* | 1958 | 1943 | 73.4 | 5 | 4 |
| Hamburg* | 1949 | 1934 | 74.2 | 3 | 3 |
| Hesse | 1967 | 1953 | 72.4 | 32 | 17 |
| Lower Saxony | 1962 | 1947 | 78.0 | 58 | 42 |
| North Rhine-Westphalia | 1967 | 1953 | 76.9 | 117 | 53 |
| Rhineland-Palatinate | 1967 | 1953 | 82.4 | 41 | 27 |
| Saarland* | 1964 | 1949 | 83.1 | 7 | 7 |
| Schleswig-Holstein* | 1956 | 1941 | 71.4 | 12 | 12 |
| Summary[a] | | | 77.4 | 415 | 221 |

*Notes:* Own calculations. Information in columns 1 and 2 is taken from the working paper version of Pischke and von Wachter (2008) (Pischke and von Wachter, 2005, Table 1). Column 3 is calculated based on information from the German Federal Statistical Office (1967, p. 92). The first column gives the year in which the ninth grade was introduced. Column 2 gives the approximate corresponding birth cohort (=year of introduction−15 because of nine years of schooling with a school starting age of 6). As the compulsory schooling reform coincided with the one-off implementation of a short school year in some states (see Table 7), some states only have a difference of 14 years between column 1 and 2. Column 3 states the share of students in basic schools in 1964. We assign individuals as affected by the compulsory school law change if they currently live in the state on the left of this table and were born in the year stated in column 2 or later. Since our data only include the current state of residence, but no retrospective information on the state of birth or school graduation, we measure compulsory schooling exposure with measurement error. To safeguard against mis-assignment we exclude information on respondents that report to have lived outside West Germany in 1989. Columns 4 and 5 display the overall number of observations and the number of observations affected by the reform, respectively. Respondents living in the "city states" of Hamburg and Bremen are excluded in our analysis as those states consist of a single city only, making the reform assignment based on the state of residence in 2017 less reliable. Likewise we exclude the states of Saarland and Schleswig-Holstein in our analysis as we only observe individuals affected by the compulsory schooling reform in these less populated states. We further only consider individuals born in an interval around ±15 years of the first cohort affected who either visited a basic school (*Hauptschule*) or an intermediate school (*Realschule*). While only the former school type has been directly affected by the reforms, previous evidence suggests that some students may have decided to visit an intermediate school as a result of the reform (Cygan-Rehm, 2022). *For the reasons outlined above, these states are not included in our analysis. [a]For the share of students in basic school, this is the average; for the number of observations, this is the sum over all states (excluding Hamburg, Bremen, Saarland, and Schleswig-Holstein).

Table 6: Estimates for the effect of years of compulsory schooling on self-control

| | (1) | (2) | (3) |
|---|---|---|---|
| | Dependent variable | | |
| | Reduced form | IV estimation | |
| | Self-control | Years of schooling | Self-control |
| Reduced form | | | |
| Reform | −0.004 | | |
| | (0.381) | | |
| First stage | | | |
| Reform | | 0.689*** | |
| | | (0.165) | |
| Second stage | | | |
| Years of schooling | | | −0.006 |
| | | | (0.526) |
| Observations | 415 | 415 | 415 |
| First-stage $F$-statistic | | 17.318 | |

*Notes:* Own calculations based on SOEP-IS, wave 2017. The reduced form gives the direct effect of the introduction of a compulsory ninth grade for basic school students on self-control (standardized to mean 0 and standard deviation 1). The first stage gives the effect of the compulsory schooling reform on years spent in school. The second stage gives the effect of (instrumented) years of schooling on self-control. The sample is restricted to respondents who reside in a West German state at the time of interview and report to have lived in a West German state in 1989 as the reforms introducing the ninth grade took place in West Germany only. The small "city states" Berlin, Bremen, and Hamburg are excluded because their location and urban structure makes a mis-assignment of the reform status in the absence of precise state-of-schooling information more likely, e.g., students might have attended an out-of-state school. We drop respondents from Saarland and Schleswig-Holstein as we only observe individuals affected by the compulsory schooling reform in these less populated states. We further only consider individuals born in an interval around ±15 years of the first cohort affected who either visited a basic track school (*Hauptschule*) or an intermediate track school (*Realschule*). While only the former school type has been directly affected by the reforms, previous evidence suggests that some students may have decided to visit an intermediate school as a result of the reform (Cygan-Rehm, 2022). The first-stage $F$-statistic refers to the $F$-statistic of the instrument. All regressions in this table include full sets of gender, year of birth, and state fixed effects. State-of-birth, cohort-clustered standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Robustness checks: Estimates for the effect of years of compulsory schooling on self-control

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Reduced form | First stage | Second stage | | |
| | Dependent variable: | | | | |
| | Self-control | Years of schooling | Self-control | Number of observations | First-stage $F$-statistic |
| **a) Control for short school years** | | | | | |
| Coefficient | 0.003 | 0.613*** | 0.005 | 415 | 12.102 |
| | (0.379) | (0.176) | (0.587) | | |
| **b) Add state-specific linear trends** | | | | | |
| Coefficient | 0.025 | 0.696*** | 0.036 | 415 | 15.623 |
| | (0.384) | (0.176) | (0.523) | | |
| **c) Include additional birth cohorts (reform ±20 years)** | | | | | |
| Coefficient | 0.001 | 0.691*** | 0.001 | 508 | 18.620 |
| | (0.369) | (0.160) | (0.000) | | |

*Notes:* Own calculations based on SOEP-IS, wave 2017. Specifications and sample restrictions are as stated in the notes to Table 6 if not part of the robustness check given in the panel of this table. State-of-birth, cohort-clustered standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

# Figures

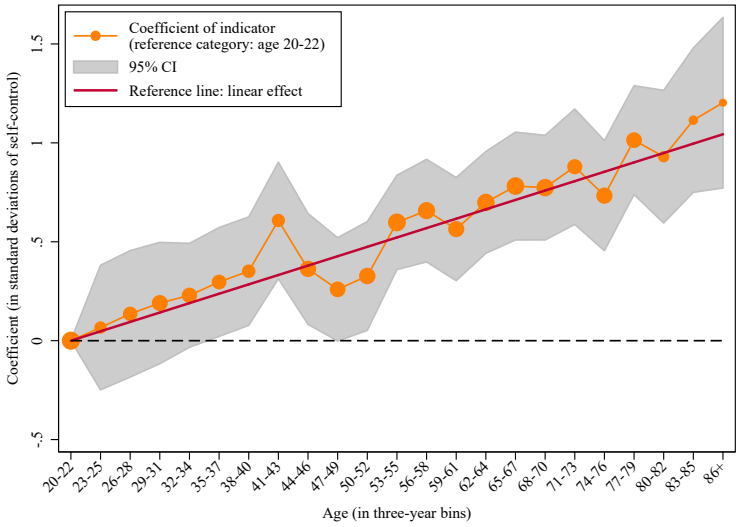


Figure 1: Linearity of self-control in age

*Notes:* Own illustration based on SOEP-IS, wave 2017. The plot shows the association between self-control and age for three-year age bins. The orange markers give coefficient estimates (in standard deviations of self-control) stated on the $y$-axis for each age bin on the $x$-axis. The coefficient is obtained by regressing self-control on the full set of three-year age-bin indicators and control variables (cf. column 2 in Table 2). The size of the markers indicates the number of observations in the bin, ranging from 19 observations for the 86+ bin to 119 observations for the 53–55 bin. The 95 percent confidence interval (for standard errors clustered on household level) is given in gray. The association between self-control and a linear term for age in years, similar to Table 2, column 2 is plotted in red. Unlike in Table 2, the indicator for people born after 1989 is not included in the specification, as this would change the intercept for markers indicating ages above 29 years. The linear age effect in this Figure is 0.016 as in Table 2.

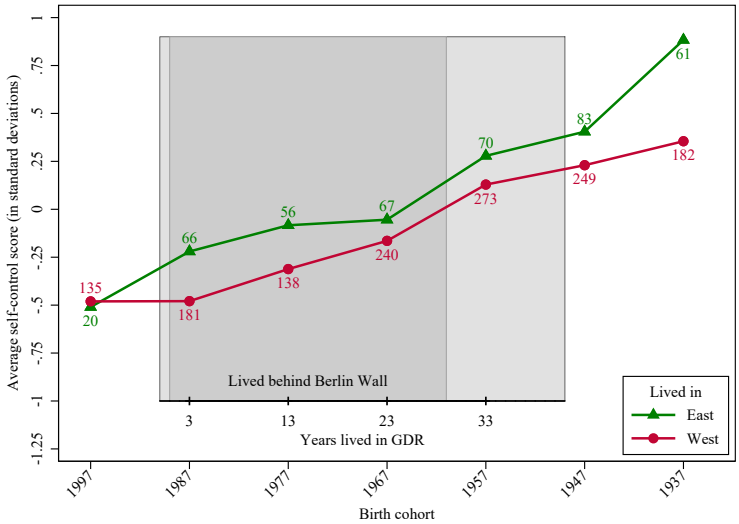


Figure 2: Trends in self-control of East and West Germans by birth cohort

*Notes:* Own illustration based on SOEP-IS, wave 2017. The plot presents the average self-control in standard deviations on the $y$-axis for ten-year birth-cohort bins on the $x$-axis, separately for East and West Germans. The year on the $x$-axis refers to the fifth birth cohort of the interval. The area shaded in light gray shows the time-span of Germany's division (1949–1990) when East Germans were born in the German Democratic Republic (GDR). Within this time-span, the area shaded in dark gray indicates the years the Berlin Wall was in place (1961–1989). The $x$-axis ranges from young birth cohorts on the left-hand side to older cohorts on the right-hand side. That is, the longer East German individuals lived in the GDR the closer to the right the marker is plotted. The number adjacent to each marker indicates the number of individuals born in this birth-cohort bin in East and West Germany with available information on self-control.

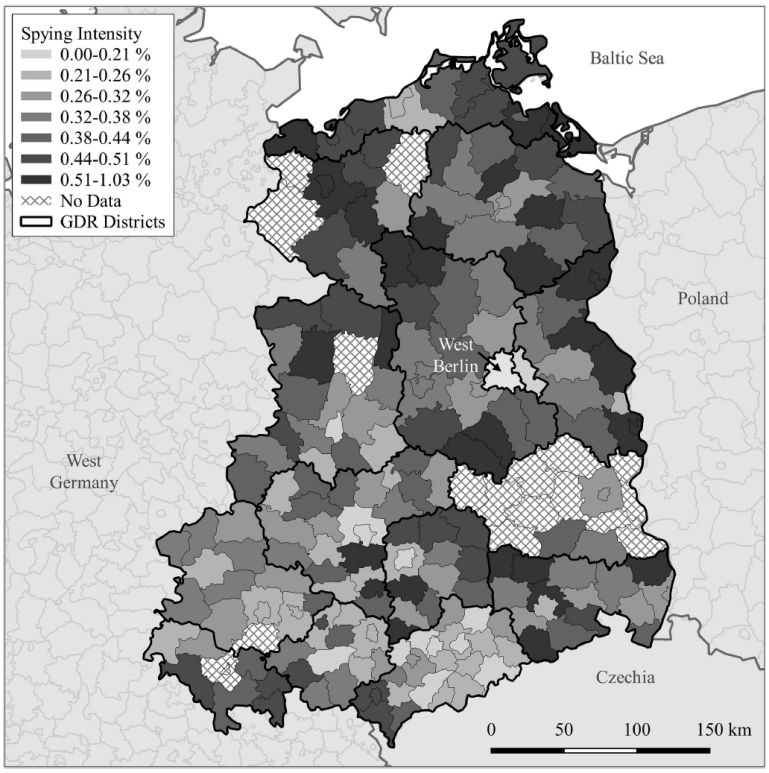


Figure 3: Map of the government surveillance through unofficial Stasi informers in the GDR

*Notes:* The figure is taken from Lichter et al. (2021b) who use information from Müller-Enbergs (2008) and include recent updates (a government agency, the Stasi Records Agency, has restored shredded Stasi files on informers and the people the information is about). This figure shows the county-level surveillance density measured by the average yearly share of operative unofficial informers relative to the population between 1980 and 1988.

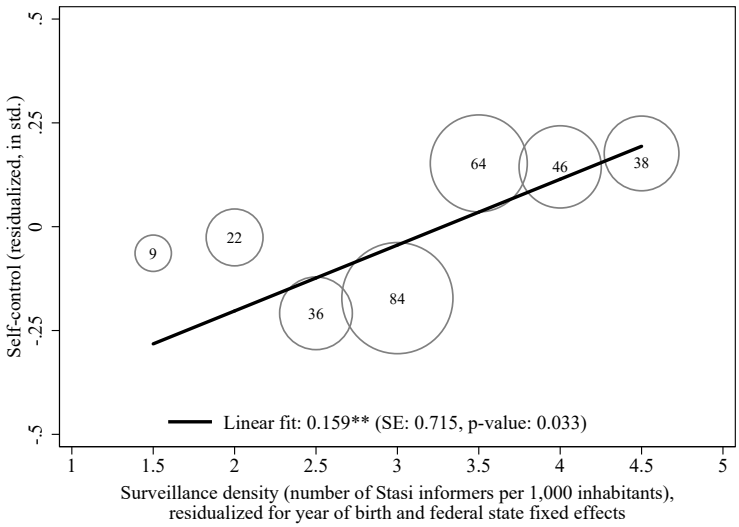


Figure 4: Government surveillance density and self-control in the GDR

*Notes:* Own illustration based on SOEPremote information of SOEP-IS, wave 2017, and data on the number of Stasi informers taken from Lichter et al. (2021a) (based on Müller-Enbergs, 2008). 299 observations. Self-control is measured in 2017, the number of informers is the average between 1980 and 1988. We assign the informer density based on the county of residence in 2017 for respondents who lived in the GDR in 1989 and still live in an East German federal state in 2017 since retrospective information on the county of residence during the existence of the GDR is not available. This figure shows the relationship between the number of unofficial informers of the Stasi (per 1,000 inhabitants). The black line gives the slope of the number of Stasi informers when standardized self-control is regressed on the number of Stasi informers, year of birth and federal state fixed effects. The axes are residualized. The $y$-axis gives the mean 0 and standard deviation 1 standardized residual when self-control is regressed on year of birth and federal state fixed effects. The $x$-axis gives the residual when the number of Stasi informers is regressed on year of birth and federal state fixed effects. We added the mean number of Stasi informers to this residual value to ensure a meaningful range of the $x$-axis. For visualization, the values on the $x$-axis are collapsed to 0.5 bins. The markers give the average self-control for all respondents in counties with an informer density within this bin. The size of the makers is proportional to the number of respondents (given by the number stated within the markers) in the informers' density bin. The slope is stated on the bottom of the plot. County-clustered standard errors for the Stasi informers coefficient in the regression model in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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# A Appendix

Table A1: Variable definitions

| Variable | Definition |
|---|---|
| Female | =1 if respondent is female, 0 else. |
| Age (in years) | Respondent's age in years in 2017. |
| Place of residence in 1989 | Respondent's answer to the question "Where did you live before German reunification, that is before 1989?" when entering the survey. We use the answer to this question to classify respondents into one out of five mutually exclusive groups: (i) lived in West Germany (usually our reference category), (ii) lived in East Germany, (iii) lived outside of Germany, (iv) born after 1989 ("does not apply"), and (v) unknown ("refused to answer"/"do not know", missing information). |
| Mom/Dad > basic school | =1 if the highest school degree of respondent's mother/father is more than basic schooling (*Hauptschule*), 0 else. |
| State of residence | Respondent's state of residence in 2017 (enters regression models through fixed effects). |
| Informer density | Number of informers (*Inoffizielle Mitarbeiter*) of the Ministry for State Security of the GDR per 1,000 inhabitants in the county, taken from Lichter et al. (2021a) who use data from Müller-Enbergs (2008) and include recent updates. |
| Years of schooling | Number of years of schooling based on the highest school degree, ranging from 8 years for having obtained a basic school (*Hauptschule*) degree before the compulsory schooling reform to 13 years for having an academic school (*Gymasium*) degree. |

*Notes:* Questions are taken from https://www.diw.de/sixcms/detail.php?id=diw_01.c.583496.de. Information on a respondent's gender, year of birth, place of residence in 1989, parents' education, and years of schooling comes from biography questionnaires usually answered when entering the sample for the first time (waves 2012 or 2013 for our respondents).

Table A2: Correlation of self-control and possible individual-level determinants

| | (1) Self-control (std.) | (2) Female | (3) Age (in years) | (4) Dad > basic school | (5) Mom > basic school | (6) Lived in 1989: East | (7) Lived in 1989: outside of Germany |
|---|---|---|---|---|---|---|---|
| Female | 0.009 | | | | | | |
| | (0.704) | | | | | | |
| Age (in years) | 0.300 | -0.034 | | | | | |
| | (0.000) | (0.131) | | | | | |
| Dad > basic school | -0.073 | -0.005 | -0.260 | | | | |
| | (0.001) | (0.828) | (0.000) | | | | |
| Mom > basic school | -0.124 | -0.000 | -0.391 | 0.559 | | | |
| | (0.000) | (0.990) | (0.000) | (0.000) | | | |
| Lived in 1989: East | 0.123 | -0.006 | 0.119 | 0.011 | 0.001 | | |
| | (0.000) | (0.790) | (0.000) | (0.641) | (0.948) | | |
| Lived in 1989: outside of Germany | 0.006 | 0.058 | -0.087 | 0.000 | 0.065 | -0.133 | |
| | (0.780) | (0.010) | (0.000) | (0.994) | (0.004) | (0.000) | |
| Lived in 1989: not yet born | -0.187 | 0.024 | -0.621 | 0.181 | 0.240 | -0.189 | -0.104 |
| | (0.000) | (0.289) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |

*Notes:* Own calculations based on SOEP-IS, wave 2017. Pearson correlation coefficients. $p$-values in parentheses.

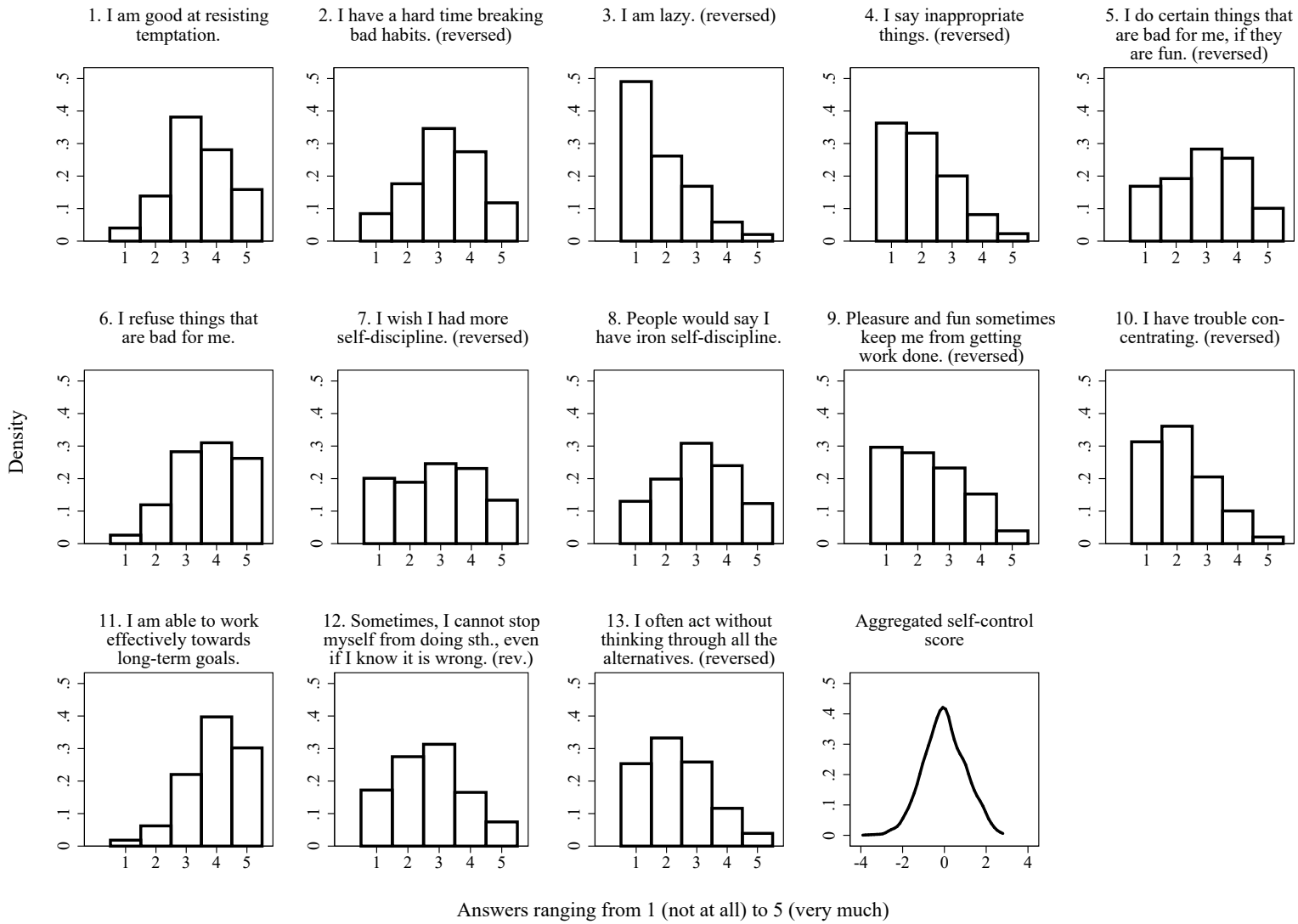


Figure A1: Brief Self-Control Scale questions and answers

*Notes:* Own illustration based on SOEP-IS. Questions marked as "reversed" enter the final self-control score as reversed items. The questions are asked in two blocks (block 1: questions 1–6 and 9–13; block 2: questions 7 and 8) separated by other questions. The average self-control distribution in the last panel refers to the average over the 13 single-item scores, i.e., the aggregated score. To account for possible different response behavior across items, we first standardize each item, take the average, and standardize the average again.

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