SUPPLEMENTARY MATERIAL

THE PARTIAL EFFECTIVENESS OF INDOCTRINATION IN AUTOCRACIES
Evidence from the German Democratic Republic

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Online Appendix for "The partial effectiveness of indoctrination in autocracies: Evidence from the German Democratic Republic"

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Contents

A The organization and socializing effects of conscription in the GDR

Conscription took place in four main steps. First, all male citizens between 18 and 50 had to be registered. In January and February of each year, local police forces were supposed to register two birth cohorts. The registration process was publicly announced on billboards and in the press.

Second, all registered male citizens had to undergo a medical and psychological examination to assess their fitness to serve in the military. The NVA determined the cohorts and timing of the examination by decree (*Musterungsordnung*; see below). Examinations took place in one or more military facilities per district under the auspices of local examination committees including, among others, active officers of the NVA, representatives of district councils, and the Stasi. Like the registration process, examinations were publicly announced on billboards and in the press at least one month in advance. Members and candidates of the Politbüro; members of the State Council (*Staatsrat*); members of the Executive Committee of the Parliament (*Mitglieder des Präsidiums der Volkskammer*); members of the Council of Ministers (*Ministerrat*); the first secretaries of the SED district executives (*SED Bezirksleitungen*); the chairmen of the block parties; and active members of the NVA, the Stasi (*Staatssicherheit*) and the police (*Bereitschaftspolizei*) were exempt from examination (Wenzke 2013).

All citizens found fit for service could be enlisted between the time they turned 18 and December 31 of the year of their twenty-sixth birthday. The conscription law included the possibility to apply for postponement of military service—for example, due to ongoing education or training, specific employment conditions, or religious reasons. It did not allow any kind of legal refusal to serve. However, because of increasing pressure from churches, the regime passed a decree in 1964 that formally introduced the right to conscientious objection. Conscripts were allowed to serve in construction units (*Bautrupps*) instead. The units formally belonged to the NVA, were unarmed, and in charge of military construction works (e.g. airports, shooting ranges, etc.). Four to six weeks prior to enlistment, recruits were examined again in order to validate the results of medical examination and to fill any information gaps about them. Upon receipt of the draft notice, recruits had approximately two weeks before formal enlistment (Wenzke 2013).

The first round of registration, examination, and enlistment in April 1962 was marked by substantial problems that were mainly related to logistical challenges. In total, 754,528 individuals from the eligible birth cohorts from between 1937 and 1943 were registered. Approximately 10 percent were found unfit to serve in the following examination. Only around 8,000 individuals applied for postponement of their service (most of these applications were not successful), 178 conscripts had to be forced into the barracks by the local police, and 287 conscripts refused to serve in the military (253 for religious reasons).

Figure A.1 displays the number of individuals per birth cohort who were registered and examined in the year 1962 (based on archival data provided by Wenzke 2013, 320). The figure reveals two interesting patterns. First, we can see that barely any individuals were registered or examined at the age of 25 or above (i.e. birth cohorts before 1937). This

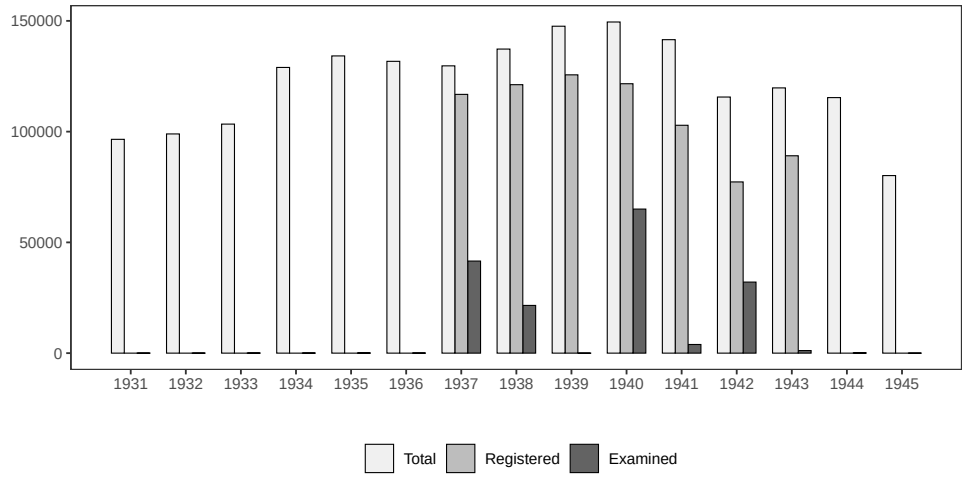


Figure A.1: Registration and Examination in 1962
Note: Bar height represent the size of the respective birth cohorts in the CCDB.

is a direct mechanical effect of the age threshold defined in the conscription law. Second, only a fraction of the individuals that became eligible after the introduction of the law in 1962 were actually registered and examined in the same year. The primary reason is that nine birth cohorts (all individuals aged 18 to 25 in the year 1962) became eligible in the same year—by far exceeding the yearly replenishment needs of the NVA. Eligible individuals from cohorts 1937 to 1945 were "distributed" across the following years to ensure constant replenishment without excess enlistments (Patzer 1998). This practice of postponement of enlistments increased the risk that some people would not have to complete service—for example, individuals who were eligible in 1962 but surpassed the maximum age threshold when actually selected for enlistment in a subsequent year.

B CCDB Data Quality

We only use the information from the 1989 file, since historians have not been able to reconstruct the reasons for differences in the population between the annual files. This means that we cannot track individuals exiting the cadre database either voluntarily or involuntarily—for example, through death or demotion. While this creates a potential selection bias, we believe that it should lead us to underestimate any effect of conscription on system engagement: if persons who where not conscripted, and thus were less exposed to indoctrination, left the cadre, this would remove individuals with low system engagement from our sample, making it harder to detect an effect.

Despite the high ambitions that led to the establishment of the central cadre database, it was never used to the extent originally intended. The project faced several challenges, including technical problems in the IT systems, a lack of qualified personnel, and a lack of paper (for printed lists when requested for planning purposes) (Ross 1997). Problems also resulted from incomplete or inaccurate data provision by collecting institutions. According to the data-hosting Ministry of Science and Technology, in the early 1980s up to 80 percent of all data entries were classified as more or less incorrect. Control routines

aimed to identify inconsistent or inaccurate entries, but the filters were rather coarse. More intense usage of the data and more comprehensive controls took place only from the mid-1980s on (Best and Hornbostel 2003). Issues related to data quality not only reduced the usability of the information for career-planning purposes in the GDR but also create challenges for the academic analysis of CCDB data today.

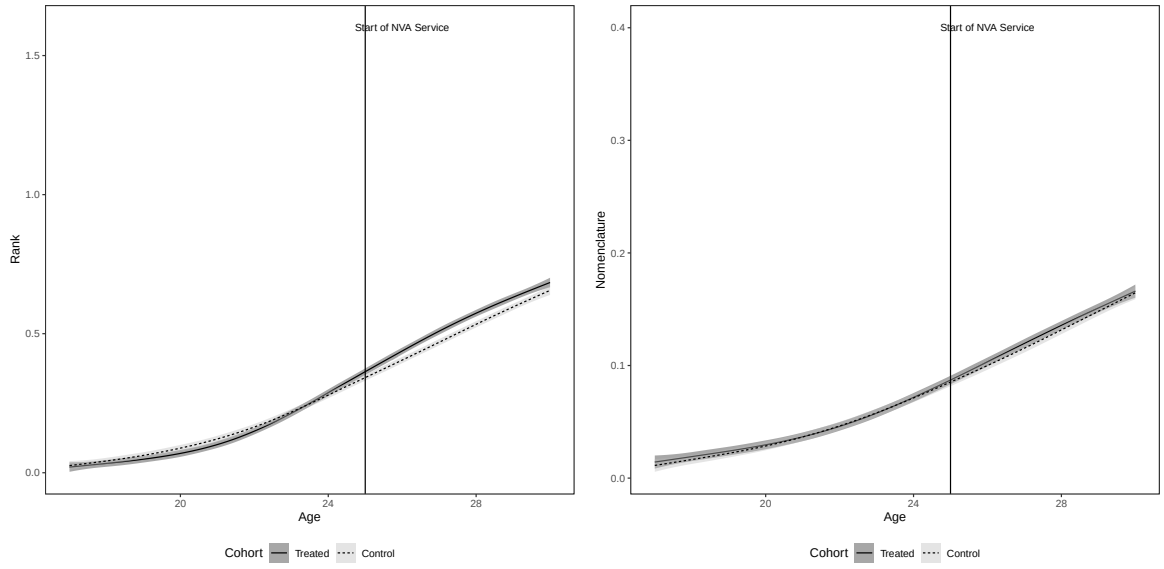
Unintentional misreporting resulted, for example, from changes to coding requirements that were not immediately implemented by data-collecting institutions. Moreover, while data entry was carried out by trained personnel, based on explicit coding rules, actual entries were not as coherent as intended. Third, programming mistakes resulted, for example, in double entries (Best and Hornbostel 2003). While these mostly unsystematic errors can compromise descriptive statistics, they are less likely to bias inferential analyses. A more pronounced challenge results from intentional misreporting. As data were collected in a centralized, repressive autocracy, there is a substantial risk that data were influenced by political considerations. This is likely to be particularly problematic for variables with a strong ideological component—for example, classifications of individuals' social origins (e.g. "working class"). First, individuals may have misreported to avert any negative consequences (e.g. on their political role before 1945). Second, data-collecting institutions may have misreported in order to make their institution's workforce seem more favorable in light of the regime's priorities (Best and Hornbostel 2003; Salheiser 2009). Due to the potential problems associated with this type of variable, we have omitted them from our analysis in our main fixed-effects models.

C Parallel Trends

Our analysis implements a difference-in-difference approach and relies on the parallel trends assumption, i.e., that the progression in rank would have been the same for conscription-affected age cohorts as in their age-adjacent, unaffected cohorts. Conceptually, it is plausible that the men before and after 1961 are comparable and any differences in long-term outcomes likely stem from their differing exposure to the conscription policy. One way to empirically bolster the plausibility of the parallel trends assumption is to compare pre-treatment trends in the outcome variable for affected and unaffected cohorts. This is difficult in our setting because of the age of the affected males and the timing of treatment. E.g., for the affected cohort of 18-25 year-old men, the 18-year old individuals have just entered the sample and no additional pre-treatment periods are observable. E.g., Figure 2 in the paper is not ideal to assess parallel trends because for the treated cohort treatment is realized for different individuals at different points in time in the shaded area, muddying the comparison of pre-treatment trends.

The cleanest and longest comparison we can construct for an assessment of parallel trends in the pre-treatment period is the contrast of trajectories of 25- and 26-year old men. Panel (a) and (b) in Figure C.2 display that comparison. For these two immediately age-adjacent cohorts, there is no discernible difference in trends visible before treatment begins at age 25.

Beyond a comparison of trends in outcome variables, we can also compare differences in other covariates to get a sense of how similar these cohorts were. We focus on a series of pre-treatment covariates that are observable in our data: a dummy for the absence



(a) (b)

Figure C.2: The y-axis displays the different measures of career advancement: Panel (a) comparison of rank across 25-year old men (treated) and 26-year old men (control); Panel (b) comparison of Nomenklatura membership across 25-year old men (treated) and 26-year old men (control).

of relatives abroad, a dummy for the family not being a returnee from West Germany, dummies for KPD/SED membership of the father and mother, and dummies for the social background of the individual's family (worker/farmer, intelligentsia, economic elite).

Table C.1 shows a comparison of means for the treated 18–25 and control 26-33 cohorts, revealing a number of statistically significant differences across covariates. This difference in social profiles is not surprising since this comparison of age cohorts straddles pre-war and war-time births. As we tighten the cohort-window, those differences become smaller and largely statistically insignificant. E.g., Table C.2 shows the same differences in means for 23–25 and 26–28 year old men, showing nearly indistinguishable social profiles.

Since there are some differences in observable covariates that emerge across treated and control cohorts, we include individual-level fixed effects in our standard specification. In addition, we also assess the robustness of our standard difference-in-difference specification to possible unobserved trends. In results that were excluded due to space constraints, but that are available on request, we consider the inclusion of covariate-time-trend interactions to account for differential trends, an event study specification, interacting the treatment indicator with year effects, and models that include linear and flexible sector-specific time trends and a simplified difference-in-difference specification, all of which confirm our main results.

Variable	Treated Mean	Control Mean	Std Difference	T-test p-value	Fisher/Wilcox p-value
No Relatives Abroad	0.808	0.748	0.146	0.000	0.000
No Returnee	0.993	0.988	0.051	0.000	0.000
Father KPD/SED	0.023	0.039	-0.093	0.000	0.000
Mother KPD/SED	0.005	0.008	-0.028	0.000	0.000
Worker/Farmer	0.610	0.649	-0.081	0.000	0.000
Intelligentsia	0.055	0.033	0.111	0.000	0.000
Economic Elite	0.003	0.003	-0.002	0.750	0.759

Table C.1: Differences in Means for 18–25 vs. 26–34

Variable	Treated Mean	Control Mean	Std Difference	T-test p-value	Fisher/Wilcox p-value
No Relatives Abroad	0.796	0.765	0.075	0.000	0.000
No Returnee	0.991	0.990	0.012	0.251	0.272
Father KPD/SED	0.026	0.030	-0.025	0.019	0.019
Mother KPD/SED	0.006	0.006	0.006	0.574	0.622
Worker/Farmer	0.627	0.637	-0.020	0.061	0.062
Intelligentsia	0.045	0.037	0.037	0.001	0.001
Economic Elite	0.003	0.003	-0.001	0.926	0.925

Table C.2: Differences in Means for 23–25 vs. 26–28

D Alternative Specifications

D.1 Sector-specific Time Trends

Table D.3: Analysis of System Engagement

	Rank	NK	Rank	NK	Rank	NK	Rank	NK
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Affected Cohort * post-1961	0.18***	0.01	0.30***	0.03***	0.65***	0.18***	0.06***	−0.003
	(0.04)	(0.01)	(0.05)	(0.01)	(0.01)	(0.004)	(0.02)	(0.002)
Treated	M 18-25	M 18-25	M <26	M <26	M 18-25	M 18-25	M 23-25	M 23-25
Control	M 26-33	M 26-33	M >25	M >25	F 18-25	F 18-25	M 26-28	M 26-28
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual-Level FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,226,243	2,226,243	3,548,014	3,548,014	2,186,887	2,186,887	932,377	932,377
R^2	0.68	0.74	0.68	0.74	0.69	0.74	0.68	0.73
Adjusted R^2	0.66	0.73	0.66	0.73	0.68	0.73	0.67	0.72

Notes:

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Standard errors are clustered at the age-cohort level.

Table D.4: Analysis of System Engagement

	Rank	NK	Rank	NK	Rank	NK	Rank	NK
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Affected Cohort * post-1961	0.15***	0.01	0.25***	0.02***	0.67***	0.19***	0.05***	−0.002
	(0.04)	(0.01)	(0.04)	(0.01)	(0.01)	(0.005)	(0.02)	(0.002)
Treated	M 18-25	M 18-25	M <26	M <26	M 18-25	M 18-25	M 23-25	M 23-25
Control	M 26-33	M 26-33	M >25	M >25	F 18-25	F 18-25	M 26-28	M 26-28
Sector FE:Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual-Level FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,226,243	2,226,243	3,548,014	3,548,014	2,186,887	2,186,887	932,377	932,377
R^2	0.68	0.74	0.68	0.75	0.70	0.75	0.69	0.74
Adjusted R^2	0.67	0.73	0.67	0.74	0.69	0.74	0.68	0.73

Notes:

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Standard errors are clustered at the age-cohort level.

D.2 Controls Variables X Time Trend

Table D.5: Analysis of System Engagement: Controls X Time Trend

	Rank	NK	Rank	NK	Rank	NK	Rank	NK
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Affected Cohort*post-1961	0.19***	0.01**	0.28***	0.03***	0.70***	0.20***	0.07***	0.0005
	(0.04)	(0.01)	(0.04)	(0.01)	(0.02)	(0.01)	(0.02)	(0.001)
Treated	M 18-25	M 18-25	M <26	M <26	M 18-25	M 18-25	M <26	M 23-25
Control	M 26-33	M 26-33	M >25	M >25	F 18-25	F 18-25	M >25,F	M 26-28
Controls X Time Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual-Level FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,201,556	2,201,556	3,503,215	3,503,215	2,169,196	2,169,196	920,970	920,970
R^2	0.67	0.73	0.68	0.74	0.69	0.73	0.68	0.73
Adjusted R^2	0.66	0.72	0.66	0.73	0.68	0.72	0.67	0.72

Notes:

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Standard errors are clustered at the age-cohort level.

E Regression Discontinuity

To complement our difference-in-difference models, we also implement a regression discontinuity design at the age cutoff generated by the conscription law. We prefer our main difference-in-difference specification for several reasons. First, the difference-in-difference approach requires weaker assumptions than a regression discontinuity design, notably parallel trends (Bertrand, Duflo, and Mullainathan 2004; McKenzie 2012). Second, the law introducing conscription for men technically included individuals aged 26 in 1962, but in practice hardly anyone from that specific year-cohort was called up for the physical examination (see above). With the practical cutoff being > 25, there were many individuals close to the age cutoff who used personal connections to avoid being drafted. This suggests sorting around the program cutoff. Third, we can only measure the running variable, age, in years, which means we have a "lumpy" running variable at the cutoff. Given these conditions, we have a fuzzy regression discontinuity in age in 1962 and opt for a "donut-hole" approach, excluding age 25 and a 1-year window around the cutoff by using randomization inference (Cattaneo, Idrobo, and Titiunik 2019). Below we show differences at the cutoff for a number of observable characteristics as a balance-check (Table **??**). For most variables the differences are miniscule, but there are some differences in educational attainment and language proficiency.

Figure E.3 displays the probability of doing military service for men as a function of the running variable age in 1962, excluding individuals aged 25. While imperfectly implemented, there is a clear jump in the probability of doing military service for men below the age cutoff in 1962.

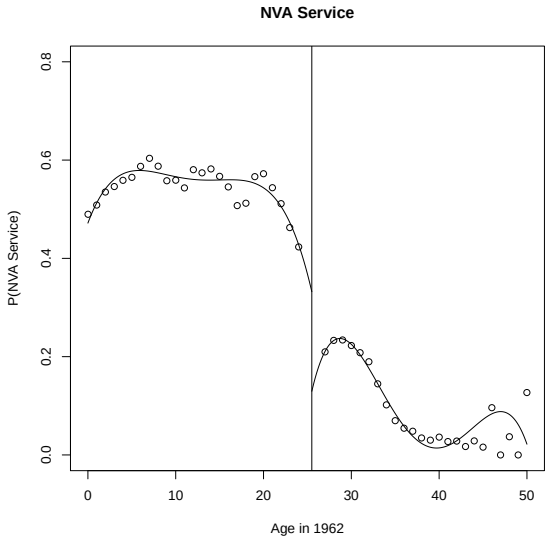


Figure E.3: Probability of doing military service at the age cutoff in 1962.

Tables E.6 and E.7 present RD estimates for the effect of military service on attained rank at different ages and randomization inference p-values. In line with our difference-in-difference approach, we find a positive and statistically significant difference between

men aged 24 and 26 in 1962 in terms of their rank as individuals age. A similar pattern emerges for our second outcome variable, moving into the Nomenklatura.

Outcome	Treated	Control	Difference	P-Value
Rank at 25	0.372	0.364	0.008	0.594
Rank at 30	0.703	0.683	0.020	0.306
Rank at 35	1.013	0.958	0.055	0.017
Rank at 40	1.271	1.223	0.048	0.050
Rank at 45	1.527	1.457	0.070	0.008
Rank at 50	1.741	1.646	0.095	0.000

Table E.6: RDD Estimates

Outcome	Treated	Control	Difference	P-Value
Nomenclature at 25	0.095	0.082	0.013	0.009
Nomenclature at 30	0.176	0.165	0.011	0.118
Nomenclature at 35	0.262	0.245	0.017	0.020
Nomenclature at 40	0.340	0.319	0.020	0.024
Nomenclature at 45	0.417	0.390	0.027	0.003
Nomenclature at 50	0.491	0.454	0.037	0.000

Table E.7: RDD Estimates

When we repeat the same analysis for women, we find little evidence of differences in career trajectories (see below).

Outcome	Treated	Control	Difference	P-Value
Rank at 25	0.146	0.142	0.004	0.709
Rank at 30	0.242	0.232	0.010	0.420
Rank at 35	0.363	0.351	0.012	0.420
Rank at 40	0.477	0.464	0.013	0.495
Rank at 45	0.569	0.532	0.037	0.045
Rank at 50	1.055	1.085	-0.030	0.086

Table E.8: RDD Estimates, Women, Only Excluding Age 25

Outcome	Treated	Control	Difference	P-Value
Nomenclature at 25	0.034	0.034	0.001	0.840
Nomenclature at 30	0.052	0.055	-0.003	0.424
Nomenclature at 35	0.078	0.081	-0.004	0.484
Nomenclature at 40	0.102	0.104	-0.002	0.758
Nomenclature at 45	0.126	0.125	0.001	0.835
Nomenclature at 50	1.055	1.085	-0.030	0.086

Table E.9: RDD Estimates, Women, Only Excluding Age 25

F Other Robustness Checks

F.1 Exclusion of Sector Fixed Effects

Table F.10: Analysis of System Engagement

	Rank	NK	Rank	NK	Rank	NK	Rank	NK
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Affected Cohort*Post-1961	0.18***	0.01*	0.29***	0.03***	0.74***	0.21***	0.07***	−0.001
	(0.04)	(0.01)	(0.05)	(0.01)	(0.01)	(0.005)	(0.02)	(0.002)
Treated	M 18-25	M 18-25	M <26	M <26	M 18-25	M 18-25	M <26	M 23-25
Control	M 26-33	M 26-33	M >25	M >25	F 18-25	F 18-25	M >25,F	M 26-28
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	No	No	No	No	No	No	No	No
Individual-Level FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,226,243	2,226,243	3,548,014	3,548,014	2,186,887	2,186,887	932,377	932,377
R^2	0.67	0.73	0.67	0.74	0.68	0.73	0.68	0.73
Adjusted R^2	0.66	0.72	0.66	0.72	0.67	0.72	0.67	0.72

Notes:
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Standard errors are clustered at the age-cohort level.

F.2 Simple Difference-in-Difference Specification

Table F.11: Analysis of System Engagement

	Rank	NK	Rank	NK	Rank	NK	Rank	NK
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	−0.58***	−0.12***	−0.75***	−0.16***	0.09***	0.04***	−0.58***	−0.12***
	(0.01)	(0.003)	(0.01)	(0.003)	(0.01)	(0.002)	(0.01)	(0.003)
Affected Cohort	1.02***	0.32***	0.82***	0.28***	0.52***	0.11***	1.02***	0.32***
	(0.01)	(0.003)	(0.01)	(0.002)	(0.01)	(0.002)	(0.01)	(0.003)
Post-1961	0.60***	0.12***	0.42***	0.04***	1.10***	0.32***	0.60***	0.12***
	(0.01)	(0.004)	(0.01)	(0.003)	(0.01)	(0.003)	(0.01)	(0.004)
Affected Cohort*Post-1961	0.78***	0.19***	0.95***	0.23***	0.12***	0.03***	0.78***	0.19***
	(0.01)	(0.002)	(0.01)	(0.002)	(0.003)	(0.001)	(0.01)	(0.002)
Treated	M 18-25	M 18-25	M <26	M <26	M 18-25	M 18-25	M 23-25	M 23-25
Control	M 26-33	M 26-33	M >25	M >25	F 18-25	F 18-25	M 26-28	M 26-28
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual-Level FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	124,245	124,245	212,209	212,209	126,777	126,777	124,245	124,245
R^2	0.22	0.14	0.10	0.07	0.32	0.22	0.22	0.14
Adjusted R^2	0.22	0.14	0.10	0.07	0.32	0.22	0.22	0.14

Notes:
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Standard errors are clustered at the individual level.

F.3 Difference-in-Difference with Age instead of Calendar Year

Table F.12: Analysis of System Engagement

	Rank	NK
	(1)	(2)
Affected Cohort*Post-1961	0.13***	0.01*
	(0.03)	(0.01)
Treated	M 18-25	M 18-25
Control	M 26-33	M 26-33
Year FE	Yes	Yes
Sector FE	Yes	Yes
Individual-Level FE	Yes	Yes
N	2,226,243	2,226,243
R^2	0.68	0.73
Adjusted R^2	0.66	0.72

Notes:
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Standard errors are clustered at the age-cohort level.

F.4 Placebo Difference-in-Difference

Table F.13: Analysis of System Engagement: Placebo Test

	Rank	NK	Rank	NK	Rank	NK
	(1)	(2)	(3)	(4)	(5)	(6)
Affected Cohort*Post-1961	0.05***	0.003	0.06***	0.005**	0.03***	−0.001
	(0.01)	(0.002)	(0.01)	(0.002)	(0.004)	(0.002)
Treated	F 18-25	F 18-25	F <26	F <26	F 23-25	F 23-25
Control	F 26-33	F 26-33	F >25	F >25	F 26-28	F 26-28
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual-Level FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1,920,624	1,920,624	3,512,659	3,512,659	844,735	844,735
R^2	0.65	0.75	0.65	0.75	0.65	0.75
Adjusted R^2	0.64	0.74	0.63	0.74	0.64	0.74

Notes:
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Standard errors are clustered at the age-cohort level.

G Alternative Mechanisms–Pathways and Tests

G.1 Education

A large body of research has investigated the effects of military service on career prospects. Several studies have found evidence that military service can qualify individuals for the job market—for example, by directly providing certain relevant skills and qualities or by providing access to advanced education and training (Torun and Tumen 2016; Asali 2017). The GDR granted various benefits to former conscripts, such as training to prepare individuals for the civilian job market or the granting of privileged access to higher education institutions (Wenzke 2013). The latter, in particular, may have boosted the careers of former conscripts. Previous research on the GDR demonstrates that from the 1970s on, (higher) education gained in importance in determining individual careers. Thus, military service may have fostered careers through qualification.

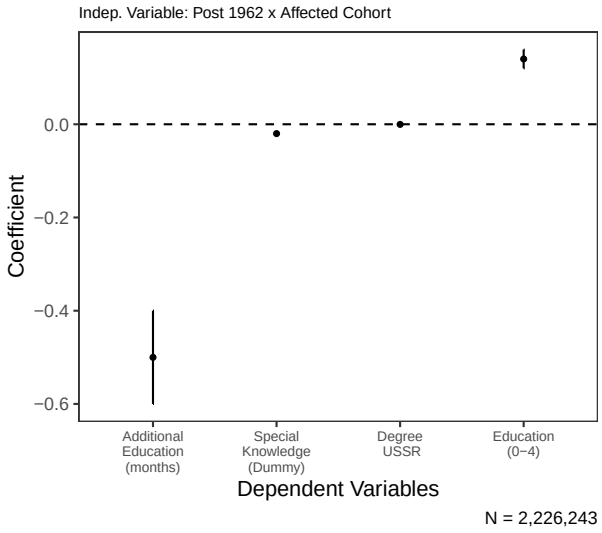


Figure G.4: Education

To explore the possible effects of military service on competence, we compare educational attainment of recruits to that of non-recruits in Figure G.4. The models in Figure G.4 use the same difference-in-difference specification as before but now employ the outcome variables overall educational attainment[1], a binary variable for the acquisition of special knowledge[2], the cumulative duration in months of completed additional

[1] An ordinal measure of educational attainment: 0= no degree, 1= non-academic degree, 2= academic degree, 4= Ph.D.

[2] Special knowledge refers to a wide range of subjects acquired during professional or university education, such as language, industrial, or technical skills. Whereas the education variable captures educational attainment in general, this variable captures the acquisition of specific skills within that education.

adult education, and the acquisition of a degree in the USSR, which was considered prestigious.

The results are at least partially consistent with an education mechanism. However, we also find that affected cohorts completed approximately half a month less additional training than non-affected cohorts. We also do not find any evidence that affected cohorts were more likely to have a degree from the USSR, as the coefficient is almost exactly zero. We also report a substantively miniscule (-0.02) and negative effect of conscripted cohorts on specialized knowledge.

G.2 Networks

Previous research has argued that military service may contribute to the formation of networks "that extend beyond family and local neighborhoods" (Hisnanick 2003). After the end of military service, these ties may have boosted career advancement: as individuals within these networks achieved career success within the state administration, they may have lifted network members upwards in the hierarchy. Thus, if military service supported career advancement through access to career-boosting networks we should see the highest service premium among those former conscripts who had access to high-quality networks during their military service.

It is difficult to test whether conscription generated career-boosting networks for conscripts. We cannot observe informal networks directly. Also, our data do not provide any information on the specific military units in which conscripts served. We therefore cannot reconstruct likely network constellations. Consequently, we have to rely on an indirect test of this alternative explanation.

For each individual, the career-fostering value of a network depends on the career prospects of all other network members. The higher the likelihood that at least one member will attain an influential position within the state administration, the better the chances that other members will profit from this connection. Thus, our test aims to exploit exogenous variation in the likelihood that conscripts ended-up in service cohorts that had a higher or lower chance of including future high-level cadres. Specifically, we focus on variation in the average education levels of service cohorts. Previous research has demonstrated that after the 1950s formal education advanced to a key requirement for high-level cadre positions in the GDR—second only to political loyalty (Best 2005). Thus, having served in more-educated rather than less-educated cohorts should have—all else being equal—increased the potential network value and relative career prospects of former conscripts.

In our empirical test, we make use of the fact that the different timing of the conscription dates (April and November) in a year led to a concentration of different education levels within the same group of conscripts: Most children in the GDR completed their schooling after eight to ten years between the ages of 14 and 17 to pursue vocational training or after 12 years at the age of 18 or 19 with a qualification for higher education. Members of the latter group were old enough to be enlisted right after their graduation and before the beginning of their university studies. Regular school years ended in early July, leaving ample time for the administration to complete the recruitment process (see above) before the beginning of the November enlistment rounds. The former group, on the other hand, was too young to be enlisted in the year they graduated. Instead, they

Table G.14: Analysis of System Engagement

	Rank	NK	Rank	NK
	(1)	(2)	(3)	(4)
Affected Cohort				
	(0.00)	(0.00)	(0.00)	(0.00)
Post-1961				
	(0.00)	(0.00)	(0.00)	(0.00)
Abi in POS-Cohort				
	(0.00)	(0.00)		
POS in Abi-Cohort				
			(0.00)	(0.00)
Affected Cohort*Post-1961	0.23***	0.02*	0.23***	0.02**
	(0.06)	(0.01)	(0.07)	(0.01)
Affected Cohort*Abi in POS-Cohort				
	(0.00)	(0.00)		
Post-1961*Abi in POS-Cohort	−0.08	−0.08		
	(0.08)	(0.07)		
Affected Cohort*Abi in POS-Cohort*Post-1961	0.03	0.03		
	(0.12)	(0.07)		
Affected Cohort*POS in Abi-Cohort				
			(0.00)	(0.00)
Post-1961*POS in Abi-Cohort			0.01	0.02
			(0.09)	(0.02)
Affected Cohort*POS in Abi-Cohort*Post-1961			0.02	−0.01
			(0.10)	(0.03)
Treated	M 18-25	M 18-25	M 18-25	M 18-25
Control	M 26-33	M 26-33	M 26-33	M 26-33
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
Individual-Level FE	Yes	Yes	Yes	Yes
N	316,457	316,457	316,459	316,459
R^2	0.70	0.76	0.70	0.76
Adjusted R^2	0.68	0.74	0.68	0.74

Notes:
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Standard errors are clustered at the age-cohortl level.

first entered mandatory professional training. According to the conscription law, enlistment had to be suspended until the end of this training. Training programs started in September of each year and lasted for 2 to 2.5 years. Consequently, most members of the first group became eligible between the months of September and February, leading to a stronger concentration of less-educated individuals in the April enlistment rounds.

Sometimes, however, individuals with the qualification for higher education ("*Abitur*") ended up in the April enlistment round, due to unforeseen circumstances. Compared to their peers, these individuals were at a disadvantage due to the lower average education level and, in turn, the lower potential network value of their service cohort.

To test the resulting career effects, we construct a dummy variable for each individual

that captures both the cadre's educational level and whether he served in the April or November enlistment round (based on the duration of their service: three calendar years for November rounds and two calendar years for April rounds, since the service lasted 18 months). We then estimate the triple interaction between the affected cohort, post-1961, and the "*Abitur* in POS draft group"-dummy. If networks play any role, we should see a negative effect of this triple interaction (and conversely, a positive effect for those individuals who had a lower education background and ended up in the enlistment round where "*Abitur*" conscripts were concentrated). While we don't expect strong interaction effects, the sheer size our data set allows us to pick up even rather small differences at conventional levels of confidence.

In Table G.14 we report the results of this test. The coefficient for the triple interaction is statistically insignificant across models. This mirrors qualitative evidence that the NVA actively discouraged networks among conscripts. In the "*Grenztruppen*," for instance, patrol partners would typically rotate, with the intention of preventing close bonding between officers.[3] This is only an indirect and rather specific test. It captures specific networks formed within narrowly defined conscription cohorts. We are not able to rule-out that military service fostered the creation of alternative types of networks. However, as conscripts interacted mainly with individuals of the same cohort in the barracks and day-to-day training, we assume that cohort-specific networks constituted a particularly relevant and frequent type of network formed during military service. Thus, our null-findings presented above cast doubt on the assumption that networks constituted the primary channel of career advancement for former conscripts.

G.3 Signaling

The career advancement of former conscripts may be the result of top-down selection effects rather than candidates' normative predispositions. As argued above, the GDR elite was convinced that conscription would work as a "school for society." The sheer amount of effort invested in (political) training of recruits made it seem plausible that the military service did indeed improve the competence change the mindset of former conscripts—instilling values such as loyalty, acceptance of authority, and diligence. Thus, military service itself may have functioned as a label that signaled certain values to administrative elites, independently of the actual behaviour (in terms of displays of effort and loyalty of former recruits).

It is difficult to directly test this potential channel, but we use an indirect approach to ascertain its relevance. As discussed in Appendix B, the CCDB did not record information perfectly. One dimension where we know that the CCDB under-reports is a cadre's military service: given the known aggregate numbers of draftees in the cohorts after 1961, the CCDB micro-data features too few individuals who officially reported that they served in the military. Thus, a substantial number of individuals underwent military service without their service being recorded in the CCDB.

If CCDB served career planning purposes and if elites selected individuals based on CCDB-information on their military service, then we should find that the difference-in-difference effect (which is only based on the timing variable and the cohort definition,

[3]Interview with former NVA Grenztruppen conscript; see also (Pergande 2014)

Table G.15: Analysis of System Engagement

	Rank	NK
	(1)	(2)
Affected Cohort		
	(0.00)	(0.00)
Post-1961		
	(0.00)	(0.00)
NVA Ever		
	(0.00)	(0.00)
Affected Cohort*Post-1961	0.19***	0.02**
	(0.05)	(0.01)
NVA Ever*Post-1961	−0.11***	−0.05***
	(0.03)	(0.01)
Affected Cohort*NVA Ever		
	(0.00)	(0.00)
Affected Cohort*NVA Ever*Post-1961	0.07**	0.03***
	(0.03)	(0.01)
Treated	M 18-25	M 18-25
Control	M 26-33	M 26-33
Year FE	Yes	Yes
Sector FE	Yes	Yes
Individual-Level FE	Yes	Yes
N	2,226,243	2,226,243
R^2	0.67	0.73
Adjusted R^2	0.66	0.72

Notes: ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Standard errors are clustered at the age-cohort level.

not information on military service recorded in the CCDB) should be larger for individuals who did officially record military service in the CCDB, as captured by the dummy variable *NVA ever*.

Table G.15 reports estimates for this triple interaction model. We still find evidence that the introduction of conscription improved the careers of the affected cohort (the standard difference-in-difference term is positive and statistically significant below the 0.1% level), but this effect is even bigger for individuals who did officially record military service in the CCDB (the triple interaction is positive and significant at the 0.1% level). We interpret this as evidence of possible (additional) top-down informational capacity effects.

H Survey data

Table H.16: Survey description

Survey year	1983	1985	1989	1990
GESIS survey ID	ZA6129	ZA6082	ZA6008	ZA2644
Questions used to construct socialist preferences index	• "I feel very close the GDR as my socialist fatherland." • "Only the Marxist-Leninist world view helps me to understand our times." • " I am proud to be a citizen of our socialist state." • "It is important in my life to... apply every skill I have to advance socialism."	• "I feel very close the GDR as my socialist fatherland." • "The transition from capitalism to socialism is a natural law." • "Imperialism is the greatest danger to world peace."	• "In the GDR, power is exercised according to my preferences." • "I am convinced by the Marxist-Leninist worldview." • "I feel closely connected to the GDR." • "Socialism will prevail globally."	• "Income should not be determined according to a person's performance. Rather, everyone should have what they need for themselves and their family." • "Only if the differences in income and social standing are large enough will there be an incentive for personal performance." • "Socialism was basically a good idea that was poorly executed." • "The rents should be fixed by the state." • "In free-market systems there should be a right to work."
Enumeration period	Jan 1983	Jan 1985	End 1988 to Start of 1989	Aug 1991 to Sept 1992
Number of male respondents	1793	1105	1396	557
Notes				We restrict the sample to respondents younger than 44 years since the pre-1989 surveys do not include age 44+ cohorts.

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