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Indoctrination and Coercion in Agent Motivation
Evidence from Nazi Germany

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Abstract

How do principals combine indoctrination and coercion to motivate their agents? Based on previous literature, we argue that indoctrination on the one hand and coercion on the other are *substitutes* in agent motivation– more of one requires less of the other. But measuring this substitution effect is hard since individuals often self–select into ideological organizations and have incentives to claim insincerely to be ideologically motivated. Using a novel dataset of wartime behavior contained in a large sample of WW2 German service records, we present a solution to these problems. We find convincing evidence to support our theory– the German army was able to induce similar effort levels from soldiers who had and had not been in the Hitler Youth, though Hitler Youth alumni required fewer punishments.

Word Count: 9,997 words

Keywords: principal–agent problems, motivation, socialization

1 Introduction

The concept of 'indoctrination' has until relatively recently aroused skepticism among social scientists. Many have, as Cederman et al (2013) note, a 'deep ontological belief in actors' selfish and unsentimental pursuit of their narrow material interests'. Although many conflict actors themselves describe their motivations in terms of ideas, this is often viewed as a cynical rationalization (Darden, 2013).

However, in recent years, many social scientists have argued that identity can explain variation in individuals' decisions in wartime (Darden, 2013) (Blattman et al., 2015) (Peisakhin and Pinto, 2009). Moreover, economists have begun to incorporate identity–based concepts such as indoctrination into formal models of principal–agent relationships (Akerlof and Kranton, 2000) (Frey and Gallus, 2016). Based on this literature, we argue that indoctrination (which alters an agent's intrinsic motivation) and externally given motivations such as punishments, are substitute strategies for principals to motivate their agents.

Testing theories of agent motivation based on ideology is, however, problematic on a number of levels. Actors' *post hoc* explanations of their own behavior are often unreliable due to preference falsification (Kuran, 1997). Moreover, if individuals join an ideological group voluntarily, it is hard to separate the effects of exposure to ideology within the group from the factors which caused them to join that group in the first place. Yet if all individuals in a society are compelled to join the group, there is no variation in the treatment at all.

This paper offers a solution to both of these problems. We test the effects of ideological indoctrination using a measure of *behavior* and combine this with the exploitation of an exogenous shock to individuals' exposure to indoctrination. The data source which we use is a novel dataset of WW2 German service records, containing information on individuals' behavior under fire (especially punishments), along with data on age and time spent in the Hitler Youth, along with extensive individual background information. Our analysis is composed of two parts. In the first part, we estimate a number of statistical models of the effects of Hitler Youth membership on punishment. We find Hitler Youth membership to have a significant and negative effect on the

probability and severity of punishment, an effect which is robust to socio–economic controls, birth year fixed effects and date of mobilization. In the second part, we probe the sensitivity of our causal conclusions by first using the degree of selection on observables to establish an upper bound on the extent of unobseved confound (Adena et al., 2015), (Altonji et al., 2005) and secondly by exploiting the opportunistic influx into the Hitler Youth after Hitler's rise to power to identify more closely the causal effect, using a regression kink design (Card et al., 2015), (Nielsen et al., 2008) . This method confirms the negative effect of Hitler Youth membership on the probability of punishment. Our findings represent a major empirical addition to the theoretical debate about the effects of different types of non–monetary motivation strategies (Akerlof and Kranton, 2000) (Frey and Gallus, 2016).

In this article, we will first outline the literature on socialization, identity and indoctrination, outline our theoretical model and then outline the relevant historical background to the Hitler Youth movement. We will then proceed to describe our research design before presenting our results along with implications and suggestions for future research.

2 Literature Review

2.1 Identifying Identity

The political socialization literature has demonstrated that political preferences are malleable and shaped by events in early life (Jennings, 2007), (Jennings et al., 2009), (Voigtländer and Voth, 2015), (Ghitza and Gelman, 2014). Yet this literature has made no link between these preferences and behavior in costly environments. Political scientists studying conventional military effectiveness have been open to accepting that non–material variables affect combat performance. Reiter (2007), for instance, pointed to the importance of nationalism and democratic values in combat motivation (Reiter and Stam, 2002). Castillo (2014) stressed the effects of ideology in motivating both the Vietcong and the Wehrmacht. However, in these cases, the evidence offered consists of fragmentary documents about a small number of soldiers. These soldiers could have

come to the attention of observers precisely because their behavior and motivation was unusual (Tversky and Kahneman, 1971).

Moreover, such studies suffer from the problem of 'preference falsification'. In authoritarian societies, soldiers have incentives to *claim* they were motivated by ideology (Neitzel and Welzer, 2012). Finally, cross–national time–series comparisons of the effects of ideology in motivating different armies would run into two problems– comparing very dissimilar units (Samii, 2016) (Clarke, 2005) and finding a good cross–national proxy for ideology or indoctrination.

At the micro–level, Costa and Kahn's analysis of service records from the US Civil War established that soldiers were less likely to desert if they came from counties with a higher pro-Lincoln vote (Costa and Kahn, 2010). Yet it is not clear from this whether it is because they themselves were intrinsically motivated by ideology, or whether they were more extrinsically motivated by the avoidance of social stigma on their return home.

In addition to this literature, a number of theorists have examined the conditions under which principals may choose between material and non–material, and among different types of non–material incentive schemes in order to motivate their agents. This literature provides the basis for our theoretical expectations.

2.2 Theory– Indoctrination and Coercion as Substitutes

Indoctrination is one type of political socialization. The concept drew attention in the early years of the Cold War due to the perceived success of totalitarian regimes in instilling ideology both in subject populations and in others. Interest in the concept has risen in response to popular concerns about religious cults and terrorist organizations (Winn, 2000). Indoctrination is also used by the US army to refer to the process by which recruits come to internalize 'army values' (Akerlof and Kranton, 2000).

In recent years an important strand of research has examined the economics of identity (Carvalho, 2016) (Akerlof and Kranton, 2000), with identity being used to explain a number

of phenomena such as cognitive performance (Hoff and Pandey, 2006), resistance to education (Carvalho et al., 2017) and the ability of organizations to overcome collective action problems (Carvalho, 2016). The revival of the concept of indoctrination in political science and economics began with Akerlof and Kranton (2000). Akerlof and Kranton noted that an exclusive reliance upon monetary means to motivate workers could be both 'costly and inefficient'. Therefore they argue that the 'ability of firms to place workers into jobs with which they identify and the creation of such identities are central to what makes organizations work'. Specifically, they develop a revised principal–agent model in which the agent's utility depends in part on the extent to which their effort level differs from the ideal expected of someone with their identity. The formal model is outlined below.

3 Akerlof and Kranton's Theoretical Model of Indoctrination

$U(y, e; c) = ln(y) - e + I_c - t_c \times |e^*(c) - e|$

where

- y is income, e is effort
- I_c is the agent's identity, $c \in (A, O)$ where A is an insider and O is an outsider
- $t_c \times |e^*(c) - e|$ is the loss in utility of diverging from the ideal effort level for the agent's category, denoted $e^*(c)$

(Akerlof and Kranton, 2000)

From this, it follows that if a worker's identity is that of an 'insider', their effort will be higher than that predicted on the basis of money alone. Consequently, organizations will be able to induce more work from their employees for the same external rewards, the more they can convince them to see themselves as 'insiders'. As Akerlof and Kranton note: "A worker who derives identity from her job is willing to work for lower overall pay" (Akerlof and Kranton, 2000). It is important to note that although individuals are usually indoctrinated by organizations external to themselves,

indoctrination itself creates a form of *intrinsic* motivation. Intrinsic motivation in this sense simply refers to an individual's desire to do a job for its own sake even if their contribution is neither noticed nor rewarded by anyone else (Akerlof and Kranton, 2000).

Applying this then to the military, let us add an additional term $p_c \times C$, which represents the probability with which the principal can punish an agent of type c, multiplied by the cost to the agent of being punished. Let us further assume that the agent has a desired level of effort $\bar{e}$. We assume that it is hard for the principal in a military context to use income to differentiate between insiders and outsiders. This is a reasonable assumption given that modern military pay scales tend to be relatively flat (Allen, 2011), (Akerlof and Kranton, 2000). The utility to an insider of giving effort level $\bar{e}$ is therefore

$\ln(y) - (p_i \times C) - t_i \times |e^*(i) - \bar{e}|$

And the corresponding utility calculation for an outsider is

$\ln(y) - (p_o \times C) - t_o \times |e^*(o) - \bar{e}|$

Since by definition the outsider's utility loss for diverging from the principal's desired effort level ($t_i \times |e^*(i) - \bar{e}|$) is greater than the corresponding utility loss for the insider ($t_i \times |e^*(i) - \bar{e}|$), the principal must ensure that the outsider faces a higher probability of punishment p_o than the insider p_o in order to induce effort level $\bar{e}$. The more an agent values doing a job for its own sake, the less he or she must be punished for failing to do it.

There is in fact evidence that Hitler himself held a similar theory of agent motivation. He claimed that 'voluntary heroes' needed no 'Articles of War' (that is, military law which prescribed punishments for disciplinary violations) but that 'cowardly egotists' could only be motivated to fight by the prospect of punishment (Hitler, 2002). Putting this together yields the following:–

- H_1 For the same level of effort, indoctrinated soldiers should require fewer punishments

4 The Hitler Youth Movement

Hitler intended the goal of the Hitler Youth to be ideological indoctrination. As he stated (Koch, 2000):

"When an opponent declares, 'I will not come over to your side', I calmly say – 'your child belongs to us already ... What are you? You will pass on. Your descendants, however, now stand in the new camp. In a short time they will know nothing else but this new community."

Similarly, the leader of the Hitler Youth, von Schirach, proclaimed that–

"Whoever marches in the Hitler Youth is not a number among millions but the soldier of an idea. The individual member's value to the whole is determined by the degree with which he is permeated by the idea. The best Hitler Youth, irrespective of rank and office, is he who completely surrenders himself to the National Socialist worldview." (Koch, 2000)

The aim of the HY was not only to imbue young boys with Nazi ideology but also to make them motivated soldiers. Nazi educational texts stressed that the HY should produce 'the eternally enthusiastic fighter' and 'the political soldier' (Koch, 2000) .

The social psychology literature on indoctrination developed in part in order to explain how the Nazis had been able to induce attitudinal change (Taylor, 2006). Specifically, post–war studies in this field have found two especially effective HY methods. First, the HY exploited individuals' desire to conform with group opinion (Asch, 1956). Second, the HY required members to sink costs by subjecting them to forced marches, dangerous 'courage tests', sleep and food deprivation and unofficial hazing (Koch, 2000) (Kater and Kater, 2009). Psychologists believe this cost sinking process encourages deep attitudinal change through dissonance reduction– individuals prefer to believe in the cause they sacrificed for rather than believe they suffered for nothing (Winn, 2000), (Taylor, 2006).

Historians and contemporary observers speak of the fanaticism of HY alumni in WW2. HW Koch, after describing a British tank commander whose unit had been attacked by Hitler Youth members 'like wolves' whom they 'were forced to kill against our will", notes that Normandy

represented the "Allies' first encounter with the Hitler Youth generation, a generation whose main development had taken place in Hitler's Germany". Nonetheless, Koch (2000) qualifies this by stating that "whether they were 'fanatical Nazis' is impossible to determine". Omer Bartov is less circumspect when discussing the impact on Nazi indoctrination on German combat motivation. He describes "young men who had spent the formative years of their youth not only in an increasingly Nazified school system, but much more important, in the Hitler Youth ...in whose militarized atmosphere they were exposed to relentless National Socialist indoctrination" (Bartov, 1991).

Although this evidence is suggestive, there are plausible objections. The canonical account of German motivation in World War II stressed that 'primary unit cohesion' was the main reason German soldiers fought (Shils and Janowitz, 1948). Moreover, recent work on secretly recorded conversations of German prisoners of war has noted that German soldiers often voiced Nazi ideology in public while expressing skepticism of the National Socialist world view in private (Neitzel, 2007) (Neitzel and Welzer, 2012).

The accounts of attacks by Hitler Youth members on Allied troops in the final stages of the war may simply mean that young adolescents were more reckless per se. Moreover, for individuals who joined the Hitler Youth voluntarily, self–selection complicates assessing the impact of indoctrination. Moreover, it could be that the majority of Hitler Youth alumni showed little fighting spirit but that the most lurid examples made it into the history books and so produced a distorted picture of the real relationship between indoctrination and subsequent wartime behavior. By systematically examining a large dataset of soldiers, our study can address these issues.

5 Data Analysis

5.1 Data

Data for the study are taken from WW2 German Army service records digitized by the University of Aachen under Professor Christoph Rass. The dataset is based on service records– *'Wehrstammbücher'* held at the German Federal Archives. Although many of these documents were destroyed

by bombing and combat during the war, 10 million remain, mostly for service personnel coming from the modern German Bundesland of Nordrhein-Westfallen (called *Wehrkreis IV* in WW2). This area formed a large part of Rass's sample partly because it was the largest *Wehrkreis* (defense district) in Nazi Germany and the most representative of the whole country in terms of support for the Nazis and the main social cleveages (such as urban/rural and Catholic/Protestant) and also because post–war Soviet occupation and wartime bomb damage either destroyed or made less accessible records from other parts of Germany (Rass and Rohrkamp, 2007). The latter two factors are not, however, plausibly correlated with a treatment (indoctrination in the Hitler Youth) which took place many years earlier. The service records contained background information including date and place of birth and prior membership in Nazi organizations (including the Hitler Youth) in addition to an individuals' service history– punishments, wounds, death and surrender (Rass and Rohrkamp, 2007).

Rass and his team digitized a total of 18,535 records. This dataset consists of five smaller samples. Two of these samples were based on random sampling of individuals who had served in the Waffen–SS and the Luftwaffe. An additional two regional samples consisted of individuals who enlisted at the recruiting stations in Aachen-Düren and Eupen–Malmedy. The fifth and largest sample was created by selecting 68 army companies recruited in *Wehrkreis IV* which were designed to be representative of the army as a whole in terms of the proportion of different service arms. For each of the companies, they chose individuals whose service records were complete and who had served at least one day in that company.

5.2 Data Analysis I– Logit with controls and fixed effects

5.2.1 Variables

We generated three outcome variables for punishments– a variable indicating whether an individual was punished, the number of punishments and worst punishment they received.[1]. In order to

[1] Costa and Kahn (2010) use desertion as their main outcome variable, but this was not possible in our work given that there were only 14 deserters in the dataset, consistent with the much lower desertion rates of the *Wehrmacht* in WW2 relative to the armies of the US Civil War (Messerschmidt and Wüllner, 1987)

create the ordinal variable for the worst punishment, we matched a verbal description of the each punishment imposed to the scales of severity (ranging from a simple warning to execution) outlined in the Wehrmacht's own penal code (see Appendix B). The Rass data also contains verbal descriptions of the reasons for which the punishments were imposed– among the ten most frequent terms were 'unauthorized', 'leave' and 'post', suggesting that temporary absences of leave from duty, not long enough to quality as desertion, constitute a large proportion of the cases [2]. Of the entire dataset, 88% of the soldiers received no punishment at all. To give an idea of the severity of the punishments imposed on the remaining 12%, we display a table below of each level of severity taken from the *Wehrmacht* penal code (Brandstetter and Hoffmann, 1936), with the corresponding percentage of soldiers who were punished and for whom this represented the most severe punishment they received. As the table below shows, the punishments were not simply 'slaps on the wrist' for minor infractions– the most common worst punishment for those soldiers who were punished was severe ('geschaẗrfter') arrest, which was one of the more severe punishments the *Wehrmacht* could impose.

[2]The wording is not standardized, which makes it impossible to assign precise numbers to each description.

Table 1: Most severe punishments

(Order of Severity) – Punishment	% of punished for whom worst punishment
(1) Warning	6.9%
(2) Punishment Drill	2.2%
(3) Fine	.9%
(4) Suspension of Leave	7.3%
(5) Confinement to barracks	.9%
(6) Arrest	6.4%
(7) Severe Arrest	59%
(8) Demotion	4.8%
(9) Prison	11.7%
(10) Dishonorable discharge	.04%

For effort levels, we used dummy variables indicating whether an individual was killed in action or surrendered and a count of the number of wounds received. While these measures are noisy, they match well with the Wehrmacht's own definition of acceptable effort. For instance, surrendering unwounded was considered by the Wehrmacht to be a clear sign of 'shirking' (Neitzel and Welzer, 2012).

For our main independent variable, we calculated indoctrination time as the number of months between the solider joining the HY (including the precursor organization the *Deutsches Jungvolk*, and individuals who remained in the HY as instructors) and their eighteenth birthday or their entry into the army, whichever came first. Figure 1 shows a histogram of the amount of time HY alumni in the dataset spent in the organization (excluding those who never joined).

5.2.2 Results I– Logit with controls and fixed effects

- H_1: Strong support– indoctrinated soldiers are punished less, though there are no differences in effort levels

We first ran a set of statistical models regressing the probability of being punished on a set of control variables which could proxy for pre–existing sympathy for Nazi ideology and birth–year fixed effects. Previous research has suggested that support for the Nazis was higher amongst Protestants than Catholics, inhabitants of small towns rather than large cities and individuals from lower middle class backgrounds (King et al., 2008; Mühlberger, 1991). We therefore used the background information in the service records to create a set of demographic controls for religion, social class, education and whether the soldier came from an urban or non–urban location (Herder, 1932). We also used information from the service records to control for various measures of exposure to combat– membership of a combattant unit and the number of months a soldier's division spent on the Eastern Front (Rass and Rohrkamp, 2007), where the German army was engaged in its most intense combat (Overmans, 2004). Additionally, we included birth–year fixed effects to control for shared formative experiences besides HY membership and age when drafted.

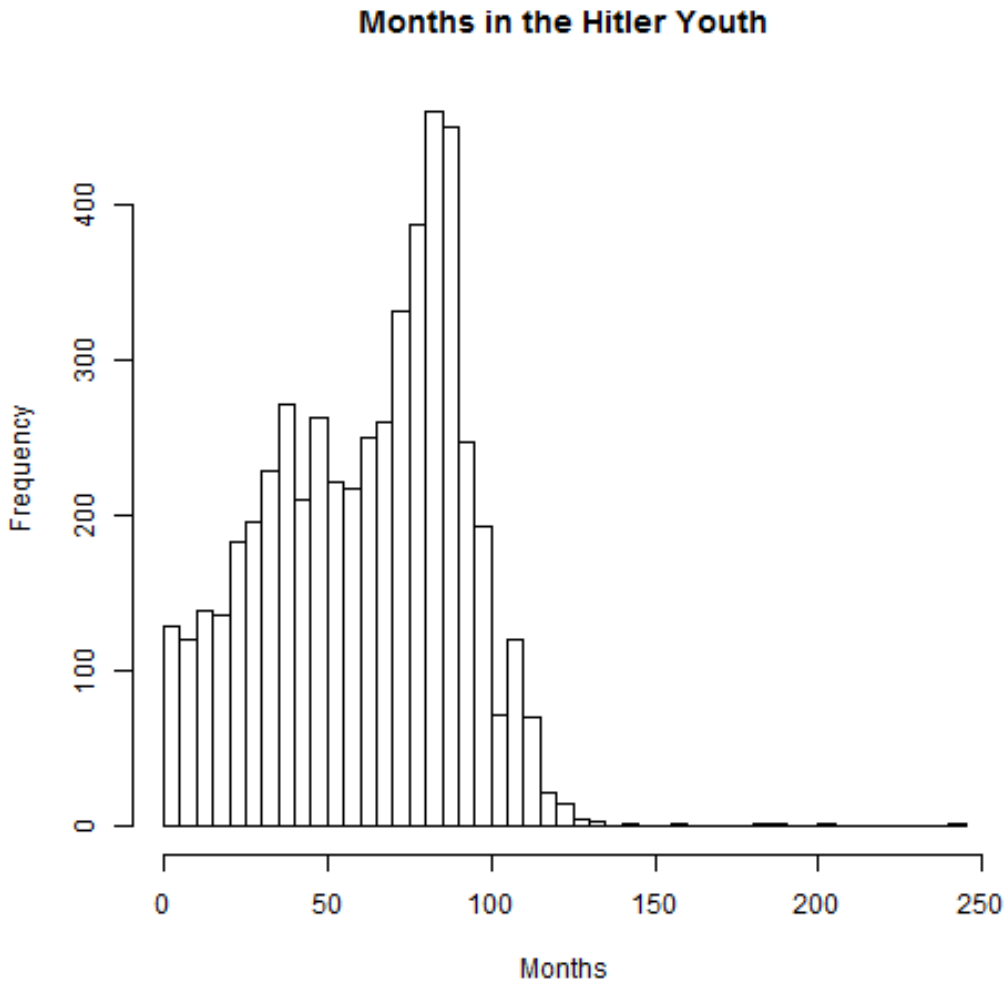

Figure 1: Time spent in the Hitler Youth.

Consistent with our expectations, we found that being a member of the HY significantly reduced the predicted probability of being punished. Controlling for demographics, combat exposure and birth year fixed effects, we found that one year in the HY was associated with a six percentage point reduction in the probability of being punished (see Figure 2). The ratio of the coefficient to its standard error was 2.4, making the effect statistically significant at the 5% level.

Table 2: Dependent Variable: Whether Punished

	Background	Background Plus Combat Experience
	(1)	(2)
Months in Hitler Youth	-.002	-.004
	(.0009)**	(.001)***
Catholic	-.119	-.062
	(.055)**	(.079)
Social Class	-.110	-.100
	(.045)**	(.079)
Education	-.103	-.085
	(.040)***	(.073)
Urban	.451	.533
	(.057)***	(.121)***
Combattant Unit		-.123
		(.075)
Months on Eastern Front		.011
		(.004)***
N	10828	3603
Log Likelihood	-3833.636	-1505.757
Birth Year Fixed Effects	Yes	Yes

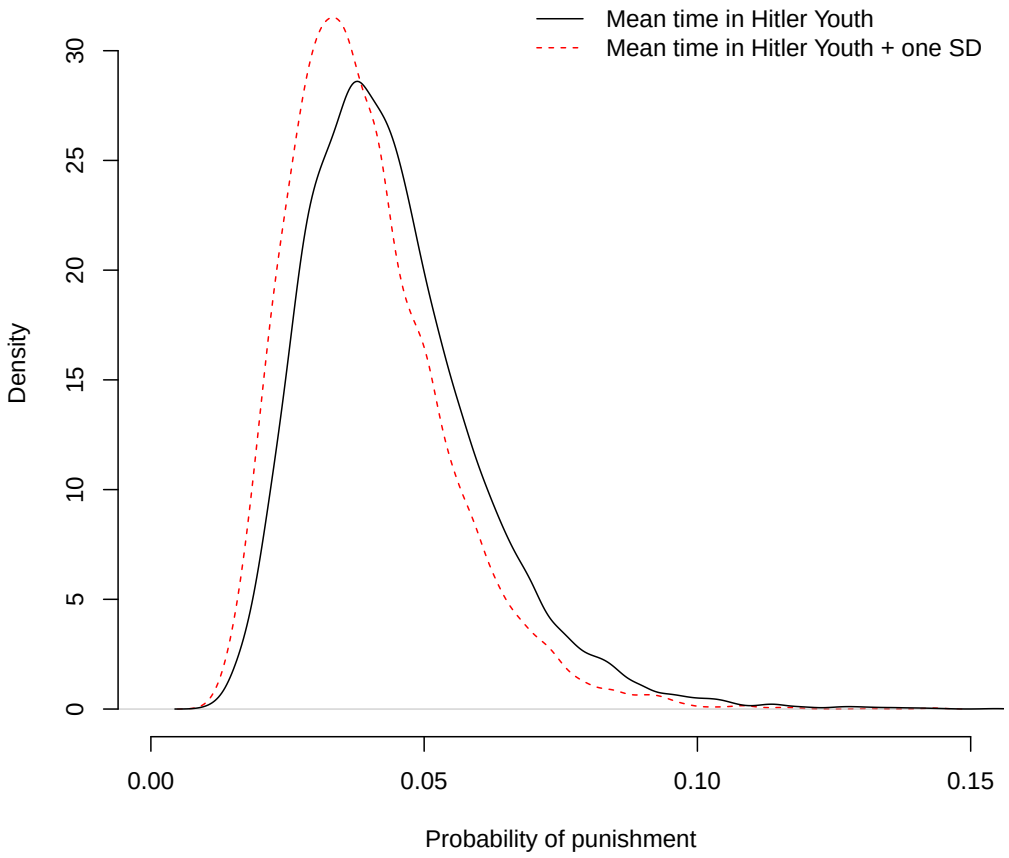

Figure 2: Marginal effects of Hitler Youth membership.

We ran alternative specifications with the highest punishment as the outcome variable and found our results unchanged. We further estimated statistical models including both birth year and division or company fixed effects so as to control for each soldier's unit's entire combat history including fronts other than the East. We found our results to be unchanged. We report these results in Appendix A 6. Similarly, months in the Hitler Youth had no effect on whether a soldier surrendered or was killed or wounded, suggesting that the *Wehrmacht*'s commanders were able to induce a similar level of effort both from those who had been in the Hitler Youth and those who had not.

Our results are open to the critique that former members of the Hitler Youth were simply better than other soldiers at explaining their behavior in terms of the language and symbolism of

the regime, and so received fewer punishments for similar behavior. To rule out this possibility, we re–ran the above statistical models using a control variable indicating whether the soldier was a member of the Nazi Party as an adult. If the *Wehrmacht*'s authorities systematically favored individuals who had a better command of the language and symbolism of Nazism, then actual members of the Nazi Party should, *ceteris paribus*, be less likely to be punished than other types of soldiers. As shown in Section A4 of the Appendix, the results do not support this interpretation– soldiers who were members of the Nazi Party were no less likely to be punished, and the effects of Hitler Youth membership on the probability and severity of punishment is also robust to controlling for Nazi Party membership. In addition, we used the historical source Erlam (1940)'s 'Ranks of the German Army, Navy and Air Force' along with the dataset to code how high each individual soldier's first recorded rank was, as a proxy for how favorably an individual soldier was viewed by the *Wehrmacht* on enlistment. We then included this as an additional control variable in our main statistical model and found our results to be unchanged. These results are displayed in Section A8 of the Appendix. Finally, it is plausible to assume that favoritism is more likely to be in operation for less serious offences– especially as more serious offences would be more likely to involve a formal process and investigation (Messerschmidt and Wüllner, 1987). Consequently, we created a new variable – sev_pun – indicating whether a soldier received ever received a punishment at least as severe as 'geschaŕfter Arrest'. We re–ran our main statistical models on this variable and still found the Hitler Youth variable to be negative and significant. These results are displayed in Section A7 of the Appendix.

This provides evidence to suggest that membership in the HY had a causal effect on soldiers' combat behavior. Yet of course it is possible that the Hitler Youth variables could simply be proxying for some unobserved omitted variable which could affect both Hitler Youth membership and subsequent punishment in wartime. To probe the sensitivity of our causal conclusions, we conduct two exercises.

Second, we follow Adena et al. (2015) in applying a test in the spirit of (Altonji et al., 2005). This test uses the observed controls to establish an upper bound on the extent of bias

due to an unobserved confounder. If the predicted value of the treatment based on the observed controls does not itself have a significant 'effect' on the outcome variable, then it is unlikely that the effect of our main treatment could be driven by an unobserved confounder either. As we shall detail below, this is exactly what we find, suggesting that the relationship between Hitler Youth membership and a reduced probability of punishment is indeed causal.

Second, we exploit the exogenous shock to the probability of joining the HY provided by Hitler's rise to power in January 1933. Hitler came to power in a backroom deal. While he did not immediately mandate membership in the HY for eligible boys, he did take a number of steps to discriminate against non–HY members in employment and education and to disband rival youth organizations. This, added to a bandwagon effect in which German parents wished to be on the winning side of German politics, induced a rapid inflow of recruits to the HY who would not otherwise have joined (Koch, 2000; Kater and Kater, 2009). A German boy's probability of being indoctrinated in the HY varied sharply based on his age when Hitler came to power.

5.3 Data Analysis II – Causal sensitivity analysis

5.3.1 Assessing the strength of potential unobserved confounders

While our results remain robust to the inclusion of the major demographic predictors of Hitler Youth membership, a causal interpretation of our findings rests on the assumption that they are not driven by some additional unobserved confounder. Of course it is not possible to assess directly the confounding effect of an unobserved variable. However, Altonji et al. (2005) provide some guidance for how observational studies may provide some checks against the possibllity that such an effect may be driving their results by using the degree of selection on the *observed* controls. They argue that if the observed controls were simply a random subset of all possible variables which may affect selection into the treatment, then the degree of selection on unobserved variables would be on average no greater than the degree of selection on the observed controls (Altonji et al., 2005). In practice, however, it is likely that the degree of selection on unobserved variables would be less than the degree of selection on observed controls since observed controls are usually

measured and controlled for specifically with the aim of reducing bias in causal estimation (Altonji et al., 2005). Using this insight, Adena et al. (2015) develops a test 'in the spirit of' Altonji et al. (2005), predicting the value of the treatment variable with the observed controls. If this predicted value does not itself have a significant effect on the outcome variable then it is not likely that the estimated effect of the treatment variable is really driven by an unobserved confounder, because this unobserved confounder would need to account for more variation in the treatment than all the observed controls combined. In the case of Adena et al. (2015), they had estimated the effect of radio signal strength on the Nazi vote in the 1932 German Election. They then tested the causal interpretation of this finding by predicting signal strength based on the other control variables in their original model and then regressing the Nazi vote in 1932 on this predicted value. As predicted signal strength had no significant effect on the Nazi vote, they judged their causal findings robust.

We replicate this method by using the observed controls in the the first part of the Data Analysis– Catholicism, social class, urban/rural, education and birth year fixed effects – to produce a predicted number of months in the Hitler Youth for each individual in the dataset. These observed controls accounted for almost half the variation in Hitler Youth membership. We then regressed the punished variable on this predicted value and found that there was no statistically significant relationship. We report these results in the Appendix A9. Following Adena et al. (2015), we therefore judge the causal status of our results to be robust.

5.3.2 The 1933 Influx

Hitler Youth membership was made legally mandatory in 1939 (Reich, 1939). However, by then, membership was already almost universal. The real discontiuity in Hitler Youth membership is earlier. When Hitler rose to power in 1933, he began to exert pressure on German families to enroll their children through a number of legal means discriminating against non–alumni and informal social pressure, for instance through the school system (Kater and Kater, 2009).

National level membership data show a steady increase prior to Hitler's rise to power in 1933, followed by even more rapid growth thereafter as sanctions against non–joiners were tightened

up. Michael Kater provides various measures of the growth of the Hitler Youth– in 1930, by his calculations, the HY comprised 18,000 members which had grown to 35,000 by 1931. By the time Hitler rose to power the HY boasted 100,000 members. However, his rise to power started a period of even more rapid growth– by the end of 1933, the Hitler Youth had 2.3 million members or 30.5% of eligible German Youth, by the end of 1936 it had 5.4 million members and by 1939 it comprised 98.1% of the eligible German population (Kater and Kater, 2009)

Hitler's rise to power therefore represented an exogenous shock to individual level Hitler Youth membership. Although the Nazis' rise in popularity is the result of other factors which may have influenced individuals' combat motivation, the fact that Hitler actually seized power and that he did so in January 1933 is plausibly exogenous to the individual characteristics of future German soldiers. Historical accounts make clear that Hitler was able to seize power in 1933 through a back room elite deal rather than through a direct electoral victory, at a time when the Nazi vote was waning and the party running out of money (Shirer, 1991) (Evans, 2005).

As will be seen below, the slope of the relationship between birth month and Hitler Youth membership likewise exhibits a sharp discontinuity. As can be expected based on the historical evidence, the relationship between birth month and probability of having been in the Hitler Youth is relatively flat for individuals who reached their 18th birthday prior to April 1933 (and who were no longer eligible for membership by the time Hitler came to power) but increases exponentially for those who were born later.

To show this graphically, we produced the following plot of the proportion of soldiers in the Aachen dataset who had been in the HY by their birth month. The oldest soldiers in the dataset were born in January 1900, which is therefore coded as birth month 1. Birth month as plotted on the x-axis of the graph therefore represents the number of months one was born after January 1900. The y–axis represents the proportion of soldiers in the Aachen sample born in that month who had been members of the Hitler Youth. We scaled the data points according to the number of soldiers in the sample born in that month and superimposed a lowess curve over the data to give an idea of the discontinuity in slope. The thick, black dashed vertical line represents those

individuals who turned 18 on Hitler's birthday in 1933, when the first post–*Machtergreifung* cohort of Hitler Youths were sworn in. As we can see, although there was a small upswing in membership for prior cohorts, the relationship between birth month and Hitler Youth membership increased rapidly for the cohorts which turned 18 after the Nazis came to power.

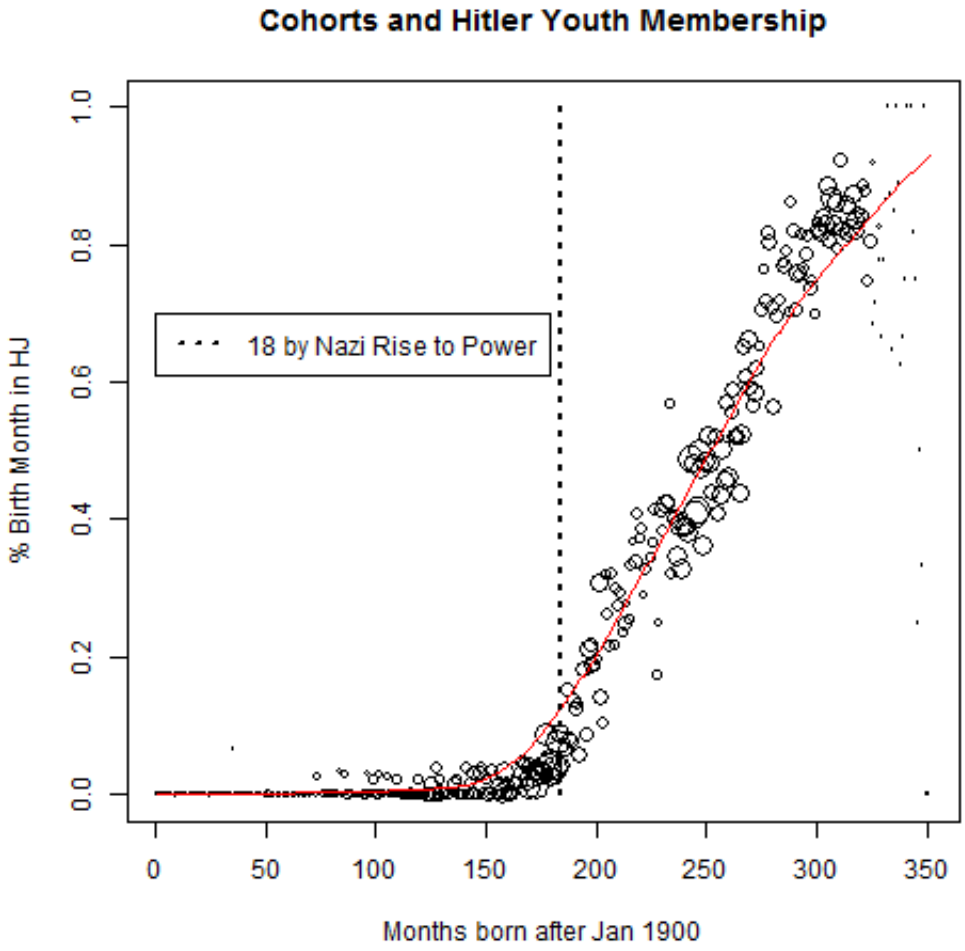

Figure 3: Birth Month and Hitler Youth Membership

Is the post–1933 membership trend though a genuine discontinuity or simply a continuation of the post–Crash trajectory? To examine this, we produced two plots. The first compares the actual growth rate of the Hitler Youth by cohort, to an extrapolation of the growth rate between 1929 and 1933 to the period after Hitler's rise to power. As can be seen below, on a continuation of the pre–1933 trend, Hitler Youth membership would have been far less prevalent amongst post–1933 cohorts than it turned out to be.

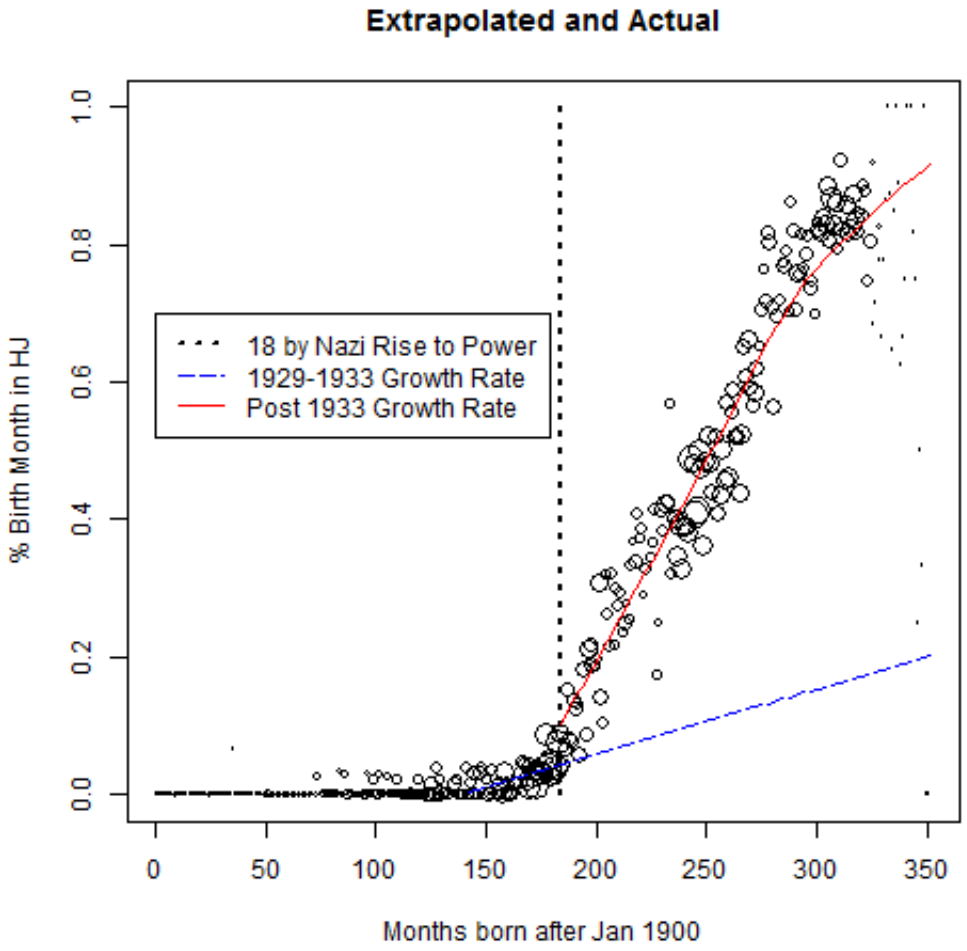

Figure 4: Birth Month and Hitler Youth Membership (Extrapolated)

Indeed, assuming that the growth of the Hitler Youth would have continued at the same rate post–1933 absent the *Machtergreifung*, far less sped up as it did, is implausible based on the trajectory of the Nazi vote share. Below, we outline this trajectory from the first post–Crash election in 1929 to the final free election in November 1932. Like Hitler Youth membership, the Nazi vote increased sharply during the depression, but it had begun to decline by the time Hitler came to power.

Table 3: Nazi Vote Share Growth

Election	Nazi Vote Share
September 1930	18.3%
July 1932	37.3%
November 1932	33.1%

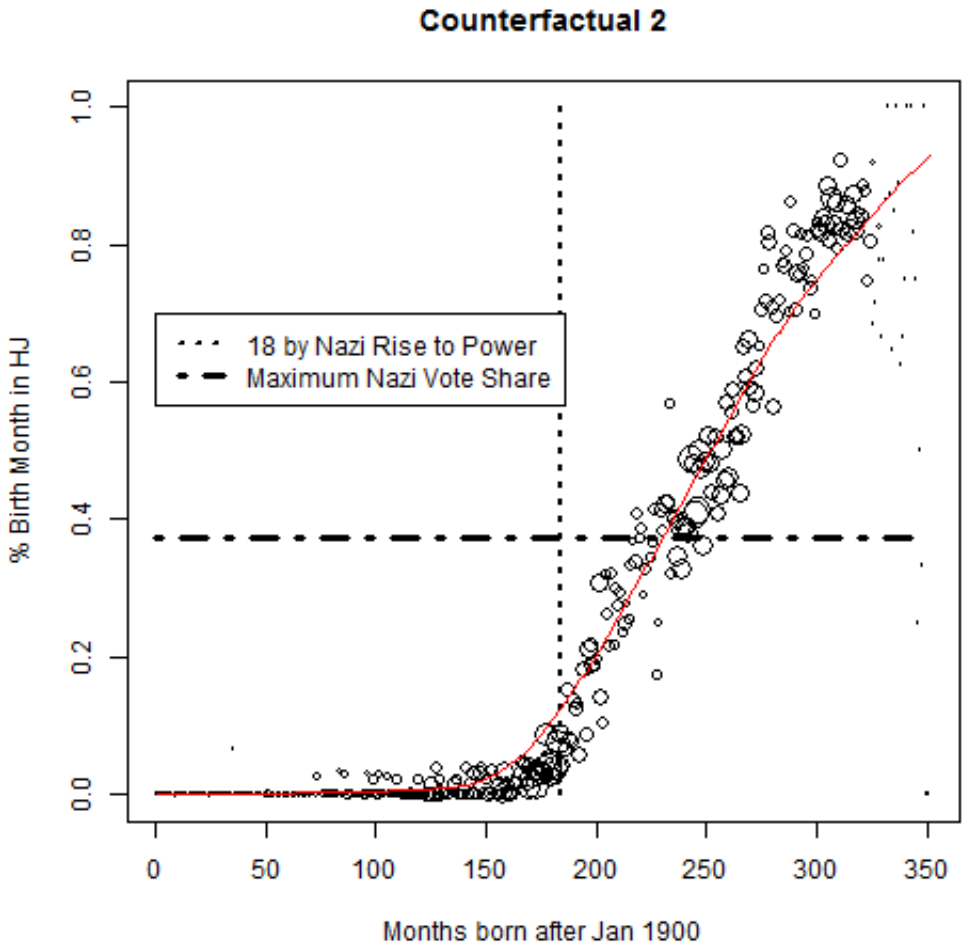

Figure 5: Birth Month and Hitler Youth Membership Compared to the Nazi Vote Share

On the basis of the figures above, a stagnating Hitler Youth membership would have been a more plausible forecast had Hitler not come to power, unless one were to assume that families would have been more willing to send their children to join a Nazi Youth Movement than to vote for the Nazi Party. To show this graphically, we plot below the trajectory of Hitler Youth membership again, compared this time to the highest proportion of German adults who voted for the Nazi Party in a free election, which can be thought of as representing a 'ceiling' on the proportion of the German population who would have freely chosen to allow their children to join the Hitler Youth. As can be seen, the proportion of pre–1933 cohorts who actually did join the Hitler Youth significantly undershoots this ceiling, whereas the post–1933 generation rapidly surpasses it.

Hitler's rise to power therefore resulted in an upward tick in Hitler Youth membership amongst cohorts who were still eligible for membership in April 1933. This uptick is unlikely to have happened had Hitler not risen to power when he did. As this shock produced a change in the growth rate of the Hitler Youth across birth cohorts rather than a sharp change in levels, exploiting this shock requires the application of a regression kink design.

5.3.3 Regression Kink Designs

The regression kink design is an extension of the regression discontinuity design. First introduced by (Thistlethwaite and Campbell, 1960), regression discontinuity designs seek to exploit exogenous discontinuities in the assignment variable in order to gauge the causal effect of the treatment on an outcome of interest. Examining differences in the outcome variable in the locality of the cut–point therefore allows the researcher to isolate any causal effect. A regression kink design exploits exogenous discontinuities in the *slope* of the line relating the assignment variable to the treatment ('kinks').

A regression kink design aims to estimate the causal effect τ of treatment B on outcome Y where both B and Y are also functions of some assignment variable V and there exists an exogenous 'kink' in the relationship between B and V at the kink point. The RKD estimand is therefore simply the change in the slope of the conditional expectation function at the kink point divided by the

change in the slope of the assignment function (Card et al., 2015). The RKD exploits an exogenous shock to the relationship between the assignment variable and the treatment. The intuition is that if the treatment has a causal effect on the outcome then we should observe a corresponding shock in the outcome variable too. The two identifying assumptions of the RKD are first that the relationship between the assignment variable and the outcome is smooth in the vicinity of the kink point and that there are no discontinuous changes in any other potential confounders at the kink point either (Card et al., 2015). In the context of this research the former assumption means that the effect of birth month on the probability of punishment cannot itself change at the kink point, while the latter point implies that there cannot be a discontinuous change in some other variable which also affects the probability of punishment at the same kink point.

The relationship between the assignment variable and the treatment may be non–linear in the full sample. In order to ensure therefore that a regression kink design is picking up a genuine discontinuity in this relationship, two strategies are possible. One can estimate the RKD only within a specific bandwidth around the kink point, or one can use the entire sample for estimation, which implies estimating a potentially high degree polynomial specification for the relationship between the assignment and treatment variables. For this paper we used both strategies. On the full sample, we used a Bayesian version of RKD estimation (Rau, 2011) using Markov Chain Monte Carlo simulation [3] with our own R package `BayesRKD` using a fifth degree polynomial chosen through stochastic variable selection (George and McCulloch, 1993) and adaptive shrinkage (Zou, 2006) to represent the relationship between the assignment variable and the treatment variable (Hans, 2009) (Park and Casella, 2008). The exogenous shock or kink in the relationship between birth month and Hitler Youth membership was not the result of a specific, formal rule and therefore had to be estimated through fuzzy RKD methods (Card et al., 2015). We also report results based on Calonico et al's (2014)'s frequentist method for regression kink designs. This procedure used only a local subset of the data within a specified bandwidth around the kink point with the

[3] Markov Chain Monte Carlo simulation is a common estimation method for Bayesian statistical models. It relies repeated draws of parameter values from the corresponding conditional distribution which are then compared with one another based on posterior probabilities until the algorithm converges on the set of values which are most likely to have generated the data we observe. A more detailed description of the MCMC procedure can be found in most standard textbooks on Bayesian methods such as Gelman et al. (2014).

bandwidth chosen so as to minimize the mean squared error. Both procedures give very similar results.

We used the number of punishments as our outcome variable [4]. The treatment was the number of months spent in the Hitler Youth [5] with birth month being the assignment variable.

5.3.4 Results II– Regression Kink Design

- H_1: Strong support– indoctrinated soldiers are punished less, though there are no differences in effort levels

As in the analysis with controls and fixed effects above, we found no statistically significant kink effect with respect to effort levels defined as the number of wounds an individual soldier suffered, suggesting no significant difference in effort levels between indoctrinated and non–indoctrinated soldiers.

However, application of this technique yielded an estimate of the treatment effect of Hitler Youth indoctrination on punishments of -.05 – that is, that an individual should expect to receive one fewer punishment in their career for every 20 months spent in the HY. This effect is very precisely estimated since the standard error is one fifth the size of the absolute value of the mean (.01).

To guard against the possiblity that this treatment effect is simply picking up the effects of younger soldiers experiencing less combat, we re–ran the regression kink design analysis including control variables for the date each soldier first joined the military in World War Two, and for the number of wounds they suffered in their career (proxying for the intensity of combat they experienced). Including these controls reduced the point estimate somewhat, although the 95% credible interval for the treatment effect still excludes zero, as shown graphically below in Figure

[4] While in principle there is no reason why a regression kink design analysis could not be performed using a binary outcome variable such as *punished* or *killed*, there are to date no good software packages for performing the computation. All existing RKD software assumes a continuous or approximately continuous outcome variable

[5] Where the Aachen data did not provide information on the date an individual left the Hitler Youth, this date was assumed to be their eighteenth birthday, consistent with HY regulations, or the date on which they were first drafted into the military, whichever was earliest.

6. The point estimate for the treatment effect remained -.05 while the standard errors were very small by comparison (.016 for the RKD model controlling for draft date and .01 for the RKD model controlling for wounds.).

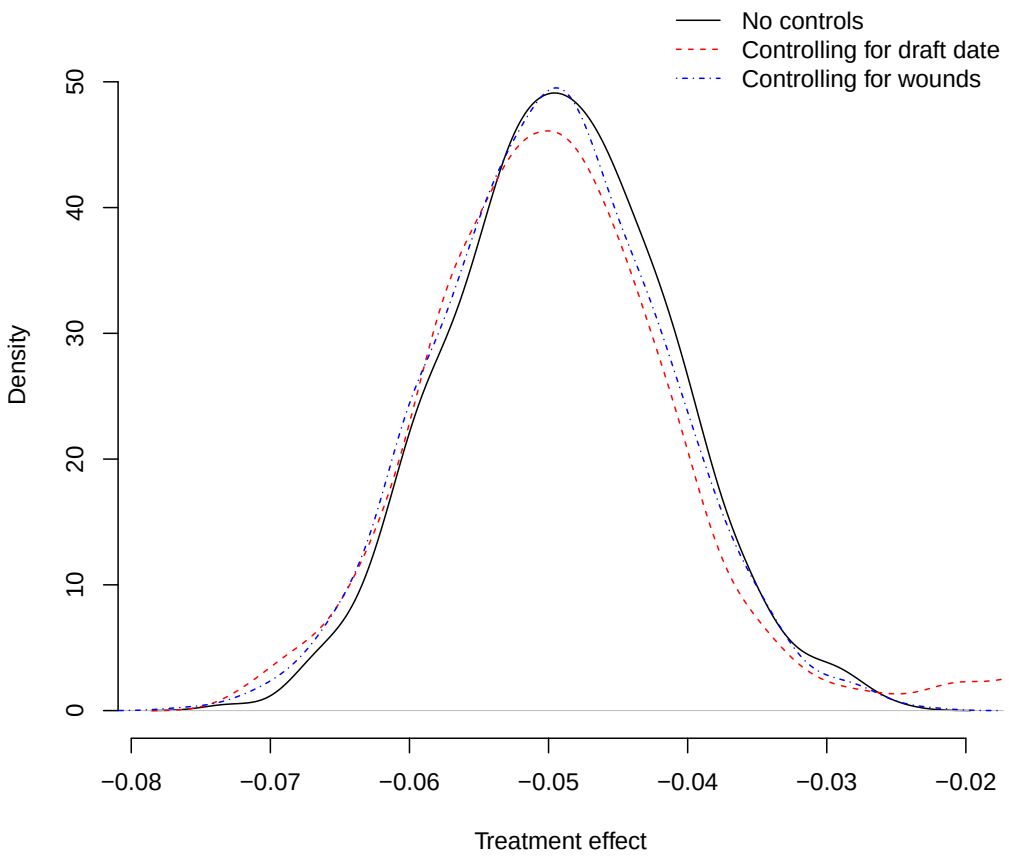

Figure 6: Effect of Hitler Youth membership on number of punishments.

To guard against the possibility that this effect merely represents favoritism, we included the Nazi Party membership variable as a control. As shown graphically below, even including this variable, the 95% credible interval for the treatment effect still excludes zero, and Nazi Party membership itself has no effect on the severity of punishment.

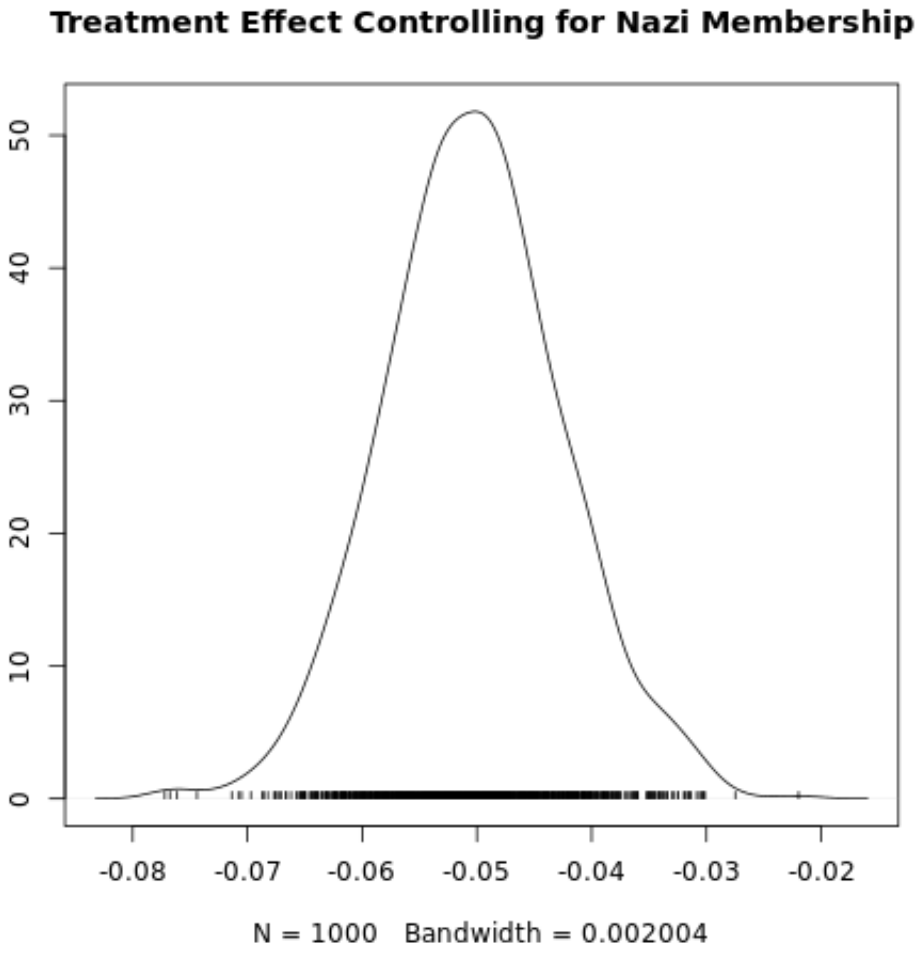

Figure 7: Controlling for Nazi Party membership

Finally, we examine the validity of the RKD's identifying assumptions in three ways. First we chose a number of alternative 'placebo' kink points. We found that a number of other kink points also yielded a statistically significant treatment effect. However, to say that these alternatives are truly placebos would be quite incorrect since the popularity of the Hitler Youth experienced a number of other exogenous shocks either side of the kink point we chose– the troubles of the Weimar economy before Hitler's rise to power and the formal and informal measures the Nazis took to expand the size of the Hitler Youth after it. We therefore also used two alternative means to probe the validity of our assumptios of an exogenous kink in 1933. First, we simply regressed the probability of punishment on the birth month dummies. We found there to be little difference between the 'effects' of birth month on the probability of punishment at the kink point and in the months before and after. Second, while we cannot of course rule out the possiblity of a discontinuity in some other unobserved variable in the vicinity of the kink point, we did test for the presence of a discontinuity in the observed controls – urban status and social class – at the kink point and again found nothing.

6 Analysis

Our results suggest that the 'Hitler Youth' generation had internalized Nazism such that their military commanders needed fewer punishments to motivate them to fight.

Is this analysis picking up other inter–cohort differences such as the effects of indoctrination through the school system and the mass media, for instance, or having experienced economic recovery and foreign policy success under the Nazis at a more impressionable age (Ghitza and Gelman, 2014)? If so, however, this would still imply that individuals' preferences can be molded by early political socialization so as to impact their behavior, even though it would be harder to say how important the Hitler Youth alone was to this political socialization process. However, the fact that the results of the statistical models in Data Analysis I are robust to the inclusion of birth year fixed effects suggests that HY membership is important even when accounting for other shared formative experiences.

Conversely, it is true that the Hitler Youth provided more than just indoctrination. The HY also provided their members with military training, such as instruction in the use of firearms (Koch, 2000) (Kater and Kater, 2009). Yet all soldiers in the sample went through basic training in which men who had not been in the Hitler Youth would have made good whatever deficiency they may have had in technical military knowledge (Rass, 2003).

It could also be that the Hitler Youth simply inculcated individual soldiers with habits of obedience. However, it is not clear that the difference between this phenomenon and indoctrination is more than a matter of semantics. As Akerlof and Kranton (2000) noted, the US military uses the term 'indoctrination' to describe precisely the process whereby recruits acquire these habits of obedience. Moreover, as Voigtländer and Voth (2015)'s work showed, the 'Hitler Youth Generation' continued to show traces of Nazi ideology even after the war when such views were discouraged or actively suppressed by the West German state. Similarly, Neitzel (2007) notes that German PoWs who were most acceptant of Nazi ideology were also *least* likely to cooperate with the Allied authorities.

As our study covers only World War Two Germany, this leaves open the question of generalizability. A theory of individual–level behavior is hard to test on cross–national data. However, our findings are a proof of concept, which undermine the skepticism many social scientists have expressed (Akerlof and Kranton, 2000) that indoctrination can *ever* have an effect on agent behavior in a costly environment, by showing that there exists a time and place in which it has [6]. The question of whether this extends to other times and places can be a subject of future research. Evidence from military history suggests, however, that it extends to many other very different times and places. The Viet Cong, for instance, used few physical punishments. Wrongdoers were punished through 'self–criticism' sessions. For the worst offenses, miscreants would be relieved of all duties and returned to their village in disgrace, not beaten or executed (Donnell et al., 1965). Looking at modern day child soldiering, Beber and Blattman (2013) theorize that rebel groups

[6]Something which is also true if one makes the restrictive assumption that the results hold only for *Wehrkreis IV*

often prefer child soldiers precisely because they can be more easily indoctrinated and so require fewer material rewards.

7 Conclusion

Our study has shown that indoctrination can be an important factor in agent behavior. However, it has extended the existing literature by clarifying the relationship between indoctrination and coercion. Using both standard binary choice models and a regression kink design, we have shown that indoctrination reduces the necessity to use coercion to induce a given level of effort in combat.

Although extrinsic incentives can substitute for intrinsic motivation, they are less efficient from the principal's point of view in a number of ways. First, the more costly it is for principals to monitor their agents, the higher must the absolute value of punishments be to induce the same level of effort, *ceteris paribus*. In some cases monitoring may be so costly that there is no level of external incentive which a principal could provide to induce an agent to supply the desired levels of effort. Second, the very act of monitoring itself may crowd out intrinsic motivation by revealing information to the agent about the trust which the principal places in her (Bénabou and Tirole, 2005). Finally, reliance on extrinsic motivation may be inefficient in the sense that it can cause agents to focus only on easily measurable outputs and ignore other tasks (Bénabou and Tirole, 2013)(Bénabou and Tirole, 2013).

The next important question, therefore, is to establish the conditions under which indoctrination is more likely to have an effect on individuals, what types of indoctrination are most effective and how indoctrination can be undone. If indoctrination is so effective in reducing the level of punishments required to induce effort, then all actors in conflicts ought to have strong incentives to indoctrinate on two counts. Yet some actors are better at 'indoctrination' than others– the Germans and Japanese in World War II were superior 'indoctrinators' than the French or the Italians for instance. Why is this? Answers have ranged from the level of control which the state or non–state actor enjoys over their home society (Castillo, 2014) to the individual level costs of

exit from the indoctrinating organization (Akerlof and Kranton, 2000). Future research into this area could prove fruitful.

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8 Descriptive and Inferential Statistics

8.1 Summary Statistics

Table 4: Summary Statistics of Key Variables

Statistic	N	Mean	St. Dev.	Min	Max
Number of Punishments	18,536	0.250	0.900	0	21
Months in Hitler Youth	18,421	17.466	31.775	0	242
First Draft Date in World War Two	15,117	118.675	118.370	0	279
Combattant	6,565	0.780	0.414	0	1
Months on Eastern Front	6,565	31.887	11.649	0	47
Catholic	18,536	0.517	0.500	0	1
Urban	18,536	0.172	0.378	0	1
Human Capital	12,446	1.501	1.039	1	6
Class	16,515	2.182	0.772	1	4
Punished	18,536	0.133	0.339	0	1

8.2 Worst Punishment

Table 5: Dependent Variable: Worst Punishment in Career

	Ordered Logit	OLS
	(1)	(2)
Months in Hitler Youth	-.005	-.004
	(.001)***	(.001)***
Combattant	-.148	-.082
	(.077)*	(.073)
Months on Eastern Front	.013	.008
	(.004)***	(.003)***
Catholic	-.103	-.112
	(.091)	(.079)
Urban	.501	.445
	(.130)***	(.130)***
Education	-.087	-.063
	(.075)	(.052)
Class	-.114	-.091
	(.075)	(.056)
N	3626	3626
Log Likelihood	-2138.855	-8300.377
Birth Year Fixed Effects	Yes	Yes

8.3 Effects on Proxy Variables for Effort

Table 6: Dependent Variable: Killed, Wounded, Surrendered (PoW)

	DV: Killed	DV: Wounded	DV: PoW
	(1)	(2)	(3)
Months in Hitler Youth	.002	-.0006	-.0002
	(.002)	(.001)	(.003)
Catholic	-.031	-.066	-.274
	(.112)	(.066)	(.176)
Class	-.111	-.070	.199
	(.066)*	(.058)	(.154)
Human Capital	-.057	.015	-.013
	(.055)	(.045)	(.106)
Urban	.308	.187	.518
	(.157)**	(.110)*	(.264)**
Combattant	.243	.210	-.342
	(.137)*	(.078)***	(.188)*
Months on Eastern Front	.021	.016	-.006
	(.006)***	(.004)***	(.007)
N	3547	3603	3538
Log Likelihood	-1551.154	-2260.688	-501.225
Birth Year Fixed Effects	Yes	Yes	

8.4 Controlling for adult Nazi Party membership

Table 7: Dependent Variables: Whether punished and worst punishment

	Whether Punished	Worst Punishment
	(1)	(2)
Months in Hitler Youth	-.004	-.005
	(.001)***	(.001)***
Nazi Party Member	.399	.323
	(.341)	(.325)
Catholic	-.059	-.101
	(.079)	(.091)
Social Class	-.103	-.116
	(.079)	(.075)
Education	-.087	-.088
	(.073)	(.074)
Urban	.536	.503
	(.122)***	(.130)***
Combattant	-.121	-.147
	(.075)	(.077)*
Months on Eastern Front	.011	.013
	(.004)***	(.004)***
N	3603	3626
Log Likelihood	-1505.243	-2138.543
Birth Year Fixed Effects	Yes	Yes

8.5 Regression Kink Design Results: Robust Non–Parametric Method

Table 8: Estimated Treatment Effect of Months in Hitler Youth on Punishments

	Coef	Std. Err.	z	P value	CI Lower	CI Upper
Conventional	-0.012	0.002	-8.258	0	-0.015	-0.009
Bias-Corrected	-0.044	0.002	-29.098	0	-0.047	-0.041
Robust	-0.044	0.003	-13.047	0	-0.050	-0.037

8.6 Including Division and Company Fixed Effects

Table 9: Dependent Variable: Punishment

	DV: Punished	DV: Punished
	(1)	(2)
Months in Hitler Youth	-.005	-.005
	(.002)***	(.002)**
Catholic	-.066	-.057
	(.104)	(.124)
Class	-.107	-.106
	(.058)*	(.083)
Urban	.527	.522
	(.155)***	(.132)***
Human Capital	-.076	-.082
	(.056)	(.055)
N	3598	3598
Log Likelihood	-1489.779	-1480.586
Birth Year Fixed Effect	Yes	Yes
Division Fixed Effects	Yes	No
Company Fixed Effects	No	Yes

8.7 Effects on Severe Punishment

Table 10: Dependent Variable: Severe Punishment

	DV: Severe Punishment	DV: Severe Punishment
	(1)	(2)
Months in Hitler Youth	-.003	-.005
	(.0009)***	(.002)**
Catholic	-.230	-.173
	(.066)***	(.116)
Class	-.182	-.126
	(.049)***	(.107)
Urban	.479	.439
	(.084)***	(.165)***
Human Capital	-.134	-.061
	(.046)***	(.067)
N	10828	3583
Log Likelihood	-3117.649	-1247.106
Birth Year Fixed Effects	Yes	Yes
Division Fixed Effects	No	Yes

8.8 Controlling for first rank

Table 11: Dependent Variable: Punishment

	DV: Punished	DV: Punished
	(1)	(2)
Months in Hitler Youth	-.003	-.005
	(.0008)***	(.002)***
Catholic	-.055	-.107
	(.077)	(.119)
Class	-.109	-.134
	(.044)**	(.062)**
Urban	.452	.451
	(.068)***	(.217)**
Human Capital	-.092	-.059
	(.044)**	(.054)
First Rank	.025	-.055
	(.017)	(.015)***
N	7621	2766
Log Likelihood	-3045.213	-1214.789
Birth Year Fixed Effects	Yes	Yes
Division Fixed Effects	No	Yes

8.9 Altonji–Elder–Taber Test

Table 12: Dependent Variable: Punishment

	DV: Punished	DV: Punished
	(1)	(2)
Index of Observables	-.002	.001
	(.004)	(.003)
N	10959	3642
Log Likelihood	-4133.716	-1548.854
Division Fixed Effects	No	Yes

9 Punishments

- 1 *Verweis*– warning
- 2 *Strafexerzieren*– punishment drill
- 3 *Geldstrafe*– fine
- 4 *Ausgangsbeschraẅkung*– suspension of leave privileges
- 5 *Kasernenarrest*– confinement to barracks
- 6 *gelinder Arrest*– arrest
- 7 *geschaẅrfter Arrest*– severe arrest
- 8 *Dienstgradherabsetzung*– demotion
- 9 *Gefaẅngnis*– prison
- 10 *Dienstentlassung*– dishonorable discharge
- 11 *Todesstrafe*– death

  Source:– Brandstetter and Hoffman, *Gesetzbuch der deutschen Wehrmacht*