Bombers and Bread: How Rearmament Accelerated Employment Recovery in Nazi Germany

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Abstract

Using novel archival data on the German Luftwaffe, I estimate the causal effect of military spending on unemployment in Nazi Germany between 1932 and 1936. Exploiting cross-district variation of supplier locations, I estimate the impact of military procurement on local labor market outcomes. I find that the average supplier location experienced a decline in unemployment per capita of approximately 6 percentage points. A back-of-the-envelope calculation suggests that this represents around 32% of the observed aggregate fall in unemployment. I also find evidence of significant spillovers, indicating that my results likely underestimate the true effect of military spending.

(*JEL* C21, E62, H56, J63, N14, N44)

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1 Introduction

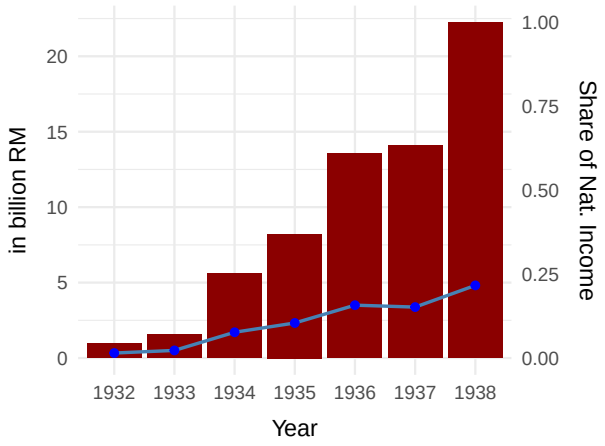
> *"The next five years must be devoted to the rearmament of the German people. Every public plan for the creation of jobs has to be judged from the point of view of whether it is necessary for the rearmament of the German people".*
>
> — Adolf Hitler, February 1933[1]

In 2022, Germany announced a ground-breaking EUR 100 bn military spending plan in response to Russia's invasion of Ukraine. After modifying her constitutional borrowing rules to ensure long-term spending capacities, military spending could reach levels not seen since the end of the Cold War.[2] In light of heightened geopolitical tensions in Europe and the world, the economic effects of defense spending are subject to increased scholarly attention, in Germany and beyond.

This paper asks whether large-scale defense buildups can reduce mass unemployment in a time of economic recession, analyzing the increase in defense spending in Nazi Germany. From 1933 to 1938, the National Socialist state embarked on an unprecedented buildup of its armed forces, increasing military spending as a share of national income by a factor of 22 in real terms (Oshima 1991).

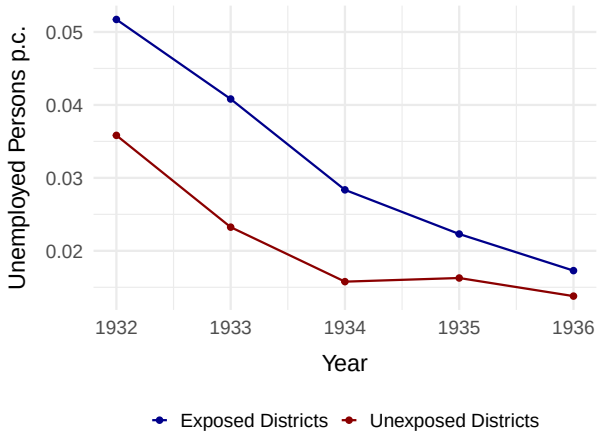
To assess how rearmament contributed to the rapid recovery of the German labor market, I combine existing historical and statistical sources with new archival data on the German air force, the *Luftwaffe*. I use a confidential list of Luftwaffe suppliers, which were recorded in a survey by the Reich Statistics Office (*Statistisches Reichsamt*, StRA) in 1933 and 1938. Exploiting their geographical distribution, I construct a regional military expenditure exposure variable, which I combine with district-level unemployment data from Ettmeier, Kriwoluzky, Papadia, et al. (2024). This allows me to identify the relative effect of military spending on district-level unemployment between 1932 and 1936. Figure 1 shows the evolution of total military spending as a share of national income (left-hand panel) and the number of unemployed persons by whether or not they were geographically exposed to these Luftwaffe suppliers.

[1] *Ministerialbesprechung vom 8. Februar 1933*, cited in Barkai (1990), p. 160.
[2] Deutscher Bundestag (2022), Bundesministerium der Verteidigung (2025).



(a) Military Spending & National Income

Notes: Military spending shown as red bars (lhs). Blue line expresses spending as share of national income (rhs). Sources: (Oshima 1991), *Statistisches Jahrbuch*



(b) Unemployed Persons Per Year

Notes: Unemployment per capita, aggregated to exposed (blue line) and unexposed (red line) districts. Source: Ettmeier, Kriwoluzky, Papadia, et al. (2024)

Figure 1: Military Spending and Unemployment, 1932–1938

The main contribution of this paper is the discovery of a new, archival data source, which allows for an identification of regional stimulus effects of defense spending, inspired by work from Nakamura and Steinsson (2014). Investigating economic effects of rearmament in Nazi Germany is notoriously difficult, as much archival material pertaining to its arms production has likely been intentionally destroyed during or by the end of the war. Detailed accounts of German military production are available only starting in about 1938–1939 (see Wagenführ 1954, Tooze 2006). I am, to the best of my knowledge, the first to estimate local defense spending effects on unemployment for the Nazi German case.

My baseline result suggests that average exposure to the Luftwaffe predicts a reduction in unemployment per capita of approximately 1.8 percentage points. When using the heterogeneity-robust difference-in-differences (DiD) estimator proposed by Chaisemartin and d'Haultfoeuille (2023), I find a weighted average treatment effect on the treated (WATT) of about 6 percentage points. Moreover, estimates obtained from an event-study regression indicate no presence of differential pre-trends. These estimates are robust to the inclusion of several control variables. Importantly, to address endogeneity concerns regarding supplier location choices, I demonstrate that my result holds when controlling for pre-existing industrial characteristics at the regional level.

Additionally, I find statistically significant spillover effects into untreated districts,

which suggest that these numbers are downward-biased, and should be considered as lower bounds. Moreover, I show that Luftwaffe exposure predicts lower population growth in both treated and neighboring areas. This is suggestive evidence that the employment effect was driven by locally absorbing the unemployed back into the labor market.

Finally, a back-of-the-envelope calculation suggests that military spending could account for 32% of the observed fall in aggregate unemployment between 1932 and 1936. While illustrative, this result hints at the important role of rearmament in the recovery of the German labor market, providing vital economic support to the Nazi regime.

Related literature. Overall, this paper contributes to three strands of existing research.

The first is the study of local fiscal multipliers using exogenous government spending shocks to estimate effects on GDP or unemployment. Chodorow-Reich et al. (2019) offers a summary of this literature. Nakamura and Steinsson (2014) use geographical variations in military procurement contracts to study regional fiscal multipliers. Auerbach, Gorodnichenko, and Murphy (2019) follow a similar approach and use defense contract data to find evidence for substantial fiscal spillovers across industries and regions. Hausman (2016) analyzes spending multipliers in the context of the 1936 Veteran's Bonus. More generally, Acconcia, Corsetti, and Simonelli (2014) investigate multipliers in the context of public spending contractions related to Italian anti-corruption legislation, while Pinardon-Touati (2026) leverages local government bank data to study the crowding-out effects of government debt. This paper adds to this literature by exploiting the locations of Luftwaffe suppliers to study employment effects, which I use to construct plausibly exogenous military spending shares (see Borusyak, Hull, and Jaravel (2025) for a review of the shift-share literature). The study of the Nazi German context is useful as the scale of both unemployment and rapid military expansion exceeded anything comparable in recent history. Additionally, the study of the Nazi German labor market recovery provides evidence on the importance of slack in determining the strength of such military spending multipliers. While Auerbach and Gorodnichenko (2012) argue that multipliers – including military spending multipliers – have a stronger impact if an economy is in recession, Ramey and Zubairy (2018) find no evidence thereof. This paper, utilizing cross-sectional evidence, shows that Nazi spending effects are strongest in the earlier rearmament years, and fade once full employment is reached.

This leads to the second strand of related research, which is specific about the role in

military spending in driving the Nazi German economic recovery. [3] Erbe (1958), Ritschl (2002), Buchheim (2001) and Buchheim (2008) argue that stimulus effects were limited, either driven by policy decisions that preceded the Nazis, or induced by a general rebound of the international business cycle. On the other hand, Abelshauser (1999) and, more recently Fremdling and Stäglin (2015), argue that Nazi deficit spending and Keynesian expansionary fiscal policy were indeed what paved the way for Germany's recovery. This paper argues for an active role of rearmament in creating jobs and boosting the labor market, and does so by introducing new data. I am, to the best of my knowledge, the first to study locally-identified multipliers for the Nazi German case.

Finally, this paper also contributes to the literature on recovery dynamics after the Great Depression. Eichengreen and Sachs (1985) have famously studied the importance of leaving the gold standard. Ellison, Lee, and O'Rourke (2024) study how this affected expectations formation and determined subsequent recovery. There is little doubt that the Nazis knew well how to exploit the rapid decrease in unemployment for propaganda purposes, as has been documented by Voigtländer and Voth (2014) and Ettmeier, Kriwoluzky, Papadia, et al. (2024). While Germany did not formally devalue, through the introduction of capital controls in 1931 and, the introduction of bilateral trading agreements in later years, Germany resorted to boosting public spending as a way to raise domestic demand instead. Mitchener and Wandschneider (2015) and – for the Nazi German case – Papadia and Schioppa (2024) investigate the role of capital controls during this period. While these tools were essential for the Reichsbank to defend the Reichsmark, Germany's lack of foreign reserves and reliance on imports were decisive limitation that heavily shaped Nazi military strategy (Tooze 2006). For lack of natural resources, Germany could not afford to extract herself from international markets completely. Bouscasse (2024) analyzes how currency devaluations contributed to raising output across trading partners during the 1930s – Germany instead imposed more or less favorable clearing agreements, especially onto economically weaker countries, if investors wanted to recover Reichsmark-denominated assets (Stucken 1964, Ritschl 1991, Tooze 2006). This paper offers new evidence on Germany's deficit-financed, domestic-driven economic recovery through the lens of rapid rearmament.

[3] Robinson (1972) famously said *"Hitler had already found how to cure unemployment before Keynes had finished explaining why it occurred."* Additionally, see Barkai (1990), Spoerer (2005), Spoerer and Streb (2013) for more detailed discussions.

2 Data and Historical Background

The objective of this paper is to identify regional effects of rearmament on unemployment, which requires various historical statistics and sources. This section introduces the data while providing relevant historical background necessary to understand the origin of these data and, most importantly, their limitations.

2.1 Local Unemployment Statistics

The severity of the recession Germany experienced between 1929 and 1933, which played a crucial role in creating the social and economic conditions that enabled the rise of the Nazi Party, cannot be understated. Between 1929 and 1932, real national income shrank by about 25% (Albers 1976). The twin crisis of 1931 led to the collapse of the financial sector: public borrowing came to a near standstill, and domestic credit markets froze (Schnabel 2004). A popular Nazi campaign slogan at the time was *"Arbeit und Brot"* – labor and bread – which underscores how the political success of the nascent Nazi regime hinged upon swiftly delivering on employment promises.

Data on local unemployment was kindly provided by Ettmeier, Kriwoluzky, Papadia, et al. (2024), who collect unemployment data on the *employment district*-level, or *Arbeitsamtbezirk*, from the Federal Archives in Berlin. These data were recorded by the Reich Labor Office and published in the *Reichsarbeitsblatt* from 1930 to 1936. Their data set covers unemployment statistics for all 358 districts, as well as district-level population, which allows for the calculation of district-level unemployment per capita.

Ettmeier, Kriwoluzky, Papadia, et al. (2024) record unemployment using three distinct categories: (1) individuals registered with unemployment insurance, (2) recipients of government crisis support transfers introduced during the Great Depression, and (3) recipients of general government welfare payments. This is because an official district-level unemployment *rate* has not been recorded. Figure 2 illustrates the annual evolution of unemployed persons aggregated from employment districts (in bars) against the total unemployment estimate from the Statistical Yearbook (StJB).

In this paper, I follow Ettmeier, Kriwoluzky, Papadia, et al. (2024) and focus on unemployment insurance and crisis relief. This ensures that the trend of both metrics follows the trend from the aggregate statistic.

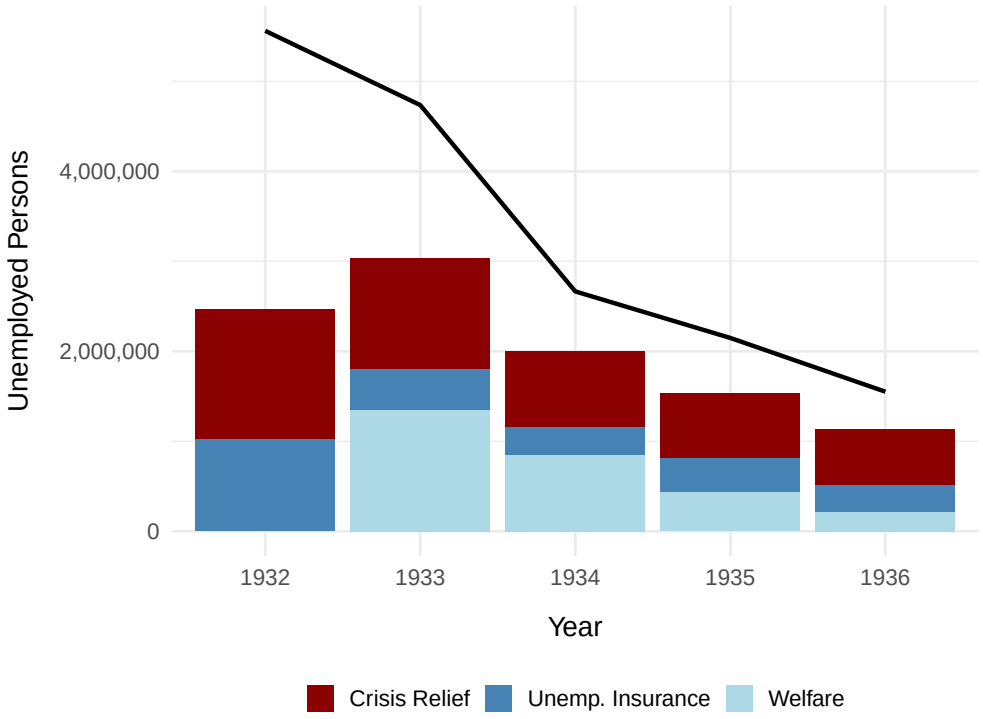


Figure 2: Unemployed Persons Per Year

Notes: Unemployment groups as recorded in Ettmeier, Kriwoluzky, Papadia, et al. (2024). The black line shows the Reich aggregate according to the *StJB*.

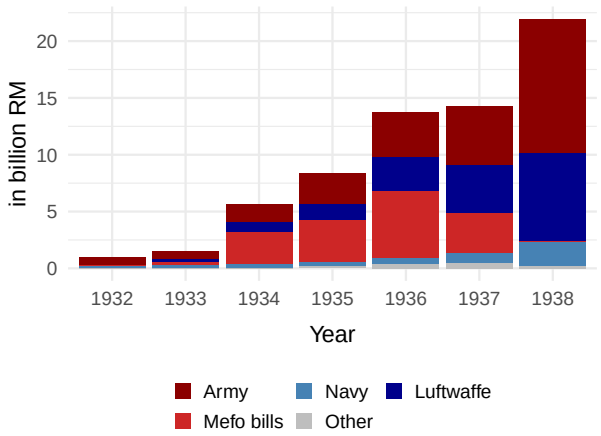
2.2 Aggregate Military Expenditure

Reconstructing aggregate military spending during the pre-war Nazi years is not straightforward. Official statistics were constructed specifically to conceal the true scale of military expenditure from the public. The following subsections will briefly outline how this was accomplished by the Nazi administration and how my data can take this into account.

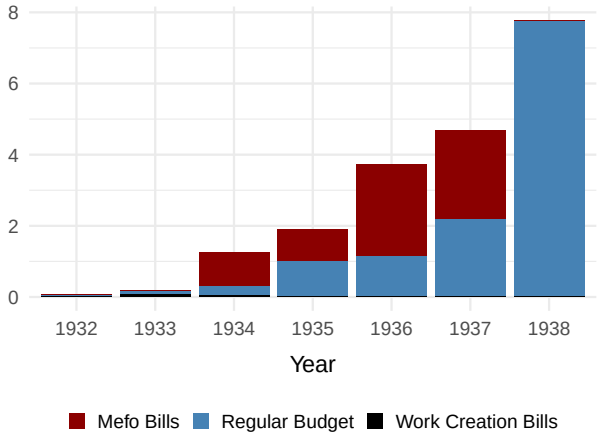
Put simply, a substantial part of military expenditure was paid for outside the regular public budget. Concretely, the Reich issued specific bills, known as *Mefo-Wechsel*, which were nothing more than a parallel currency backed by government debt. This government debt was held by the German public, who, given capital controls and related Nazi legislation, had little choice among assets in which to invest. This was a direct result of Nazi policies aimed at suppressing private demand to create excess savings, which

were absorbed by financial institutions. By design, these bills did not appear as military expenditure in government accounts, but were instead held by banks as fixed-income assets. This financing scheme was called *Geräuschlose Methode*, or *silent method*.[4].

To take this intentional misreporting into account, I use data from Oshima (1991), which include both official government accounts and broader estimates that incorporate shadow budgets resulting from *Wechsel* circulation. I hereafter refer to these as the *official* and *estimated* budgets, respectively.[5] Figure 6 shows, in 1929 terms, the decomposition of military expenditure as a whole (panel *a*), next to a decomposition of the Luftwaffe budget into its official and unofficial sources (panel *b*).



(a) Military Spending Per Year, incl. Shadow Budgets



(b) Luftwaffe Budget Sources

Notes: Decomposition of military spending into armed forces, Mefo bills represent shadow budgets that cannot be attributed to a specific category (Oshima 1991).

Notes: Mefo bills (red) and work creation bills (black) represent financing outside of official government accounts (Oshima 1991).

Figure 3: Aggregate Military Expenditure Statistics, 1932–1938

Between 1932 and 1936, Luftwaffe expenditure increased from 60 million RM to about 3 *billion* RM in 1936, which represents a 50-fold increase (Oshima 1991). Figure 6 also shows how important Mefo-bills were in turbocharging rearmament in the early 1930s.

Unfortunately, a large share of Mefo bills cannot be attributed to the specific armed forces, which is shown in light red in panel (a). This was done by design, as the vast bulk

[4]For a more detailed treatment of this topic, see Appendix B.

[5]Oshima (1991) provides a discussion of different sources used to construct military expenditure figures, which he argues were underestimated in the previous historical literature. I chose to use these estimates as they have been corroborated by later research (for instance Fremdling and Stäglin 2015) and provide clear archival documentation. For military expenditure: *Tabelle 10, Militärausgaben.*

of these bills was not intended to be repaid before the end of the 1930s.[6] To address this measurement issue, I use the subset of bills that were prematurely discounted – and can therefore be linked to specific branches of the military – to approximate the allocation of the unattributed share. In particular, I assign a portion of the "missing" Mefo bills to the Luftwaffe in proportion to its share among the discounted bills.

2.3 *Luftwaffe* Suppliers

Finally, I introduce the archival source that allows me to build a local military spending exposure variable.

As has just been demonstrated, military expenditure data for Nazi Germany is notoriously difficult to reconstruct, even more so considering that many sources were likely deliberately destroyed by the regime before the end of the war in 1945. Detailed arms production data is available only starting around 1938 (Wagenführ 1954, Tooze 2006), which is outside the period of interest of this paper. Moreover, these sources capture military output on an aggregate level, and do not allow for a regional analysis.

To construct my regional measure of exposure to the arms industry, I rely on an archival file of the Reich Statistics Office, the StRA.[7] In 1933 and 1938, the StRA sent surveys to firms that produced goods relevant to aircraft manufacturing, which contained questions on employment, production capacities, and raw material consumption.

The file I analyze in this paper contains the list of – presumably – all suppliers these questions were sent to in both of those years, including the addresses. Importantly, this covers *plant locations*, as some firms have multiple entries in different cities. In my analysis, I consider all individual plants. This yields a geographical distribution of plants for 1933 and 1938, which I then manually map to the district-level unemployment data described previously. In 1933, 30 plants were distributed across 18 out of 358 districts. In 1938, 88 plants were located in 42 individual districts.[8]

It is likely that these plant locations are endogenous to longer, economic trends in those respective area. Therefore, my main estimate is computed exploiting the location of suppliers in 1933. As will be further discussed below, I argue that these locations cannot

[6] Note that Mefo bills had a maturity of up to five years. Given that they started circulating at scale in 1934, it takes little imagination to grasp how the Nazis intended to repay this debt.

[7] BA R3102/3666. *Statistisches Reichsamt. Reichsergebnisse der Produktionserhebungen. Band 2, Flugzeugindustrie.*

[8] Entries which are likely administrative headquarters are not considered. Plants in annexed Austria are also excluded.

have been influenced by Nazi economic policies after 1933. that However, for illustration, I also report results using the distribution of firms in 1938.

In total, *R3102/3666* records 121 individual entries, the first 10 of which are shown in Figure 4. The list includes companies that remain well-known names today, such as *Daimler-Benz*, *BMW*, *Junkers*, and *Bayerische Flugzeugwerke*, better known under their later name *Messerschmitt*. Others, such as Blohm & Voss or Henschel, grew into the ranks of Germany's top 100 firms. Dornier, Junkers, and Heinkel led the development of bombers, while Messerschmitt became well-known for fighter jets (Tooze 2006).

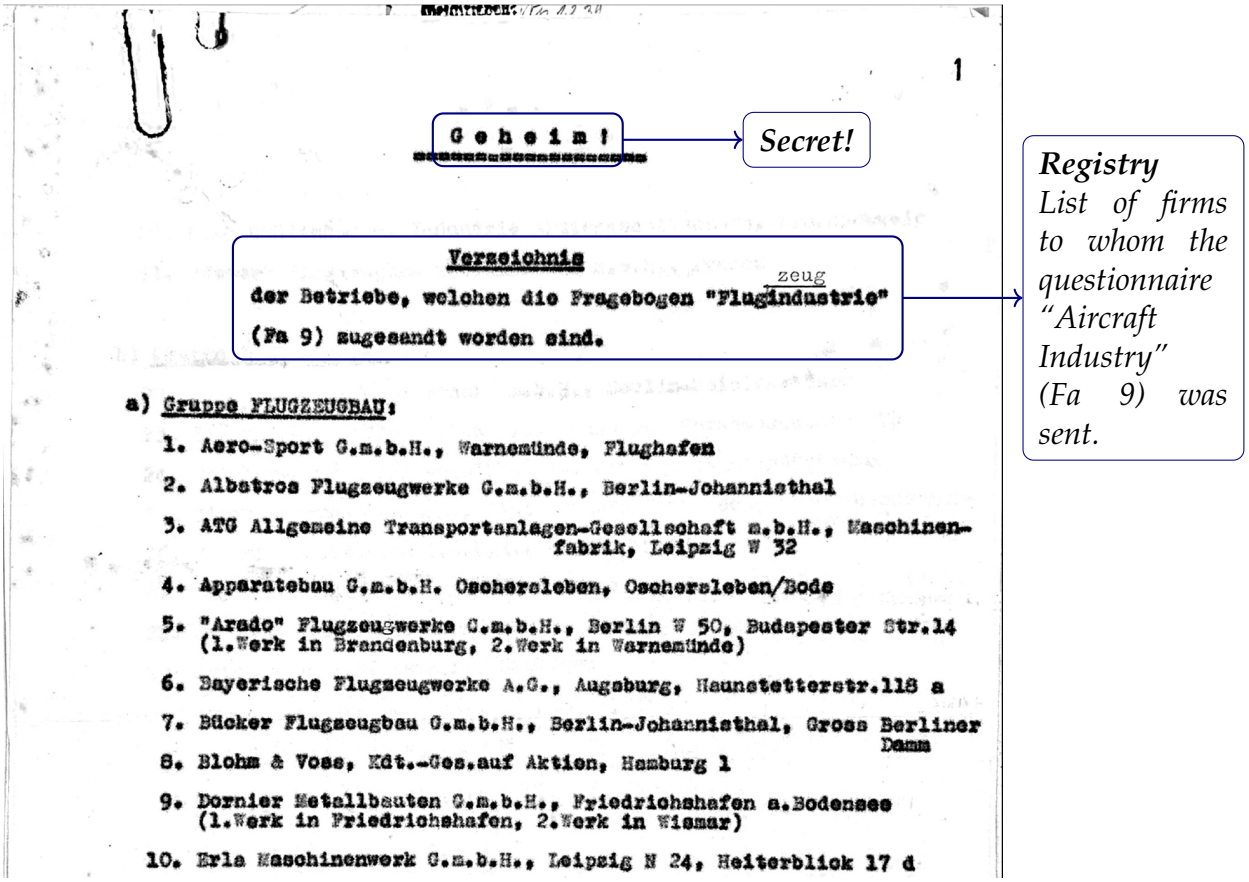


Figure 4: First Page of R3102/3666

Figure 5 shows the locations of plants for both years, illustrated as red dots.[9] In total, 30 plants are recorded in 1933, and 88 in 1938. Unfortunately, the data do not indicate when each individual plant started operating.

[9] 1936 Borders. Map according to IPUMS.

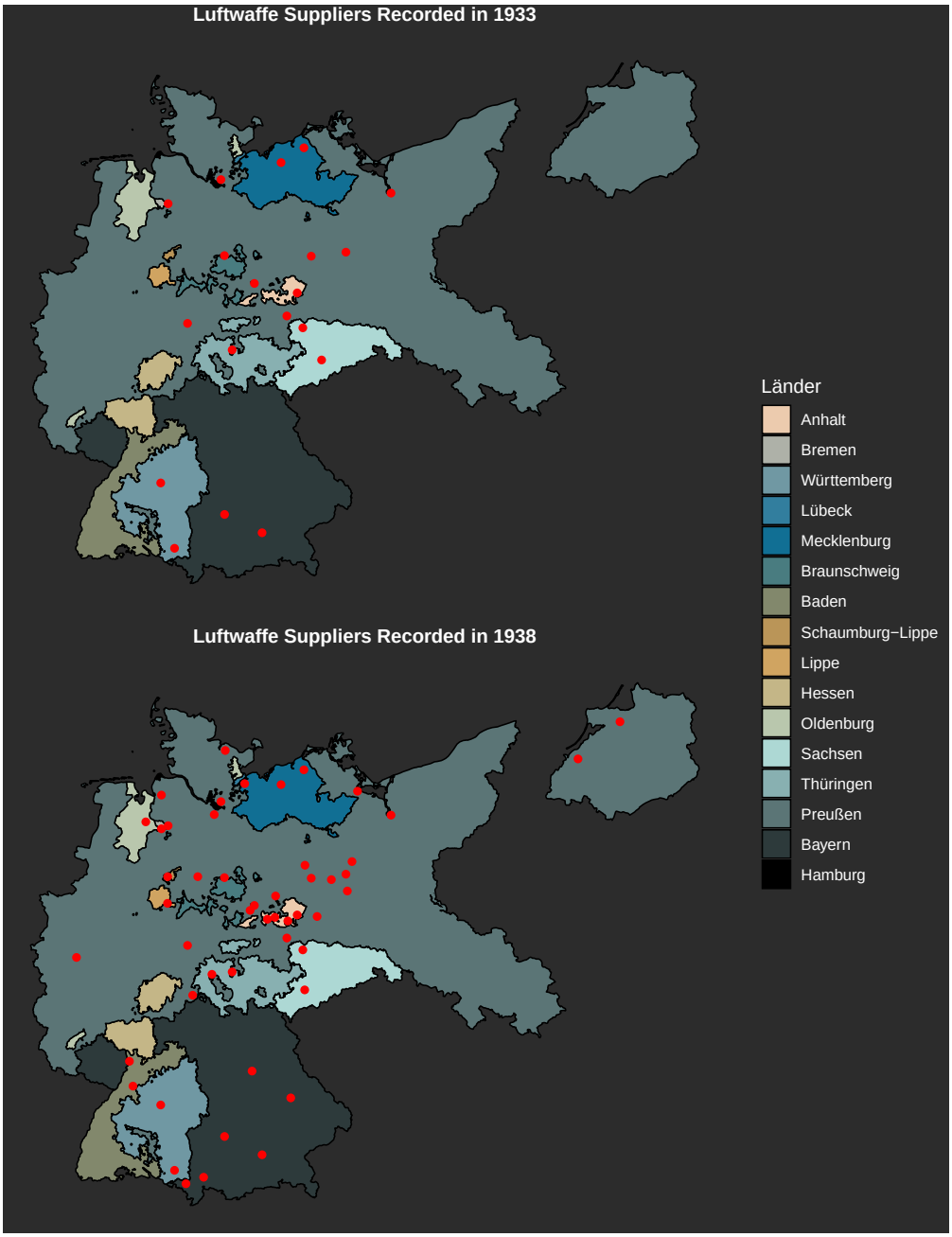
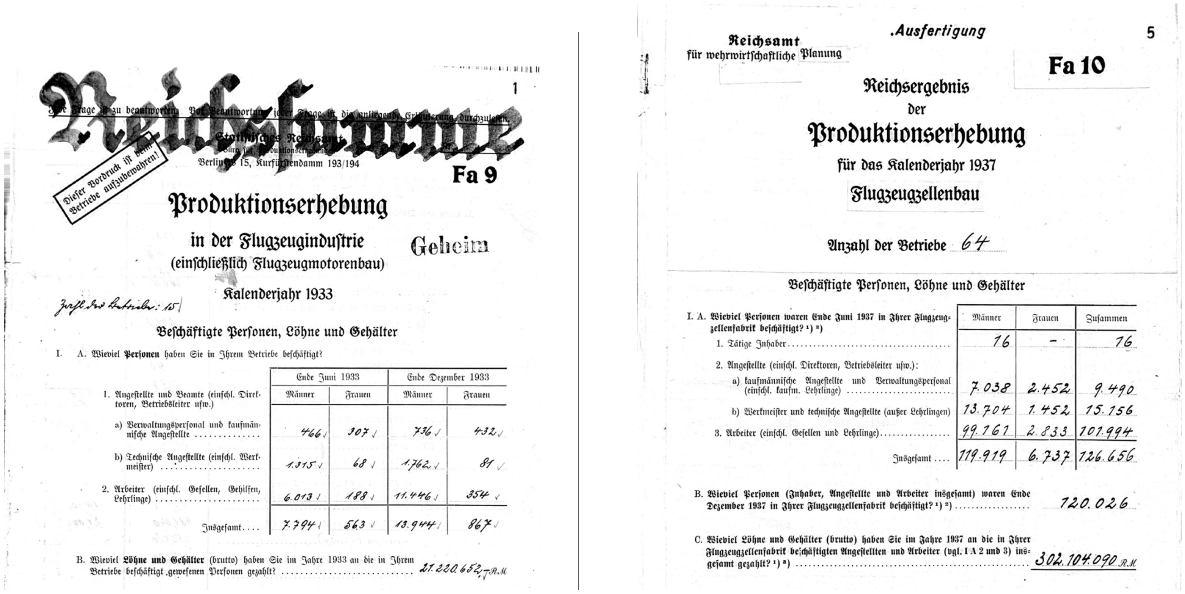


Figure 5: Distribution of Luftwaffe Suppliers

Notes: Luftwaffe supplier locations are shown as red dots. Sources: IPUMS and BA R3102/3666.

It could be of great interest to gather firm-level data for a richer analysis, but the returned individual questionnaires, if they still exist, are not in the StRA files. However, some aggregated data was collected, excerpts of which I show below in Figure 6. This document, which is likely a Reich Aggregate, tells us how many persons were employed

and how much the firms spent on wages in the *Flugzeugindustrie*, or *aircraft sector*. Unfortunately, I do not know which of the firms listed in Figure 4 are included in these totals, but they offer the following interesting observations.



Reichsamt für wehrwirtschaftliche Planung

Fa 9

Produktionserhebung

in der Flugzeugindustrie (einschließlich Flugzeugmotorenbau)

Geheim

Kalenderjahr 1933

Beschäftigte Personen, Löhne und Gehälter

	Ende Juni 1933		Ende Dezember 1933	
	Männer	Frauen	Männer	Frauen
1. Angestellte und Beamte (einschl. Direktoren, Betriebsleiter usw.)				
a) Verwaltungspersonal und kaufmännische Angestellte	466	307	736	432
b) Technische Angestellte (einschl. Werkmeister)	1.315	68	1.762	81
2. Arbeiter (einschl. Gesellen, Gehilfen, Lehrlinge)	6.013	188	11.446	354
Insgesamt	7.794	563	13.944	867

B. Wieviel Löhne und Gehälter (brutto) haben Sie im Jahre 1933 an die in Ihrem Betriebe beschäftigt gewesenen Personen gezahlt? 21.220.652 RM

(a) 1933

Reichsamt für wehrwirtschaftliche Planung

Ausfertigung 5

Fa 10

Reichsergebnis der Produktionserhebung für das Kalenderjahr 1937

Flugzeugzellenbau

Anzahl der Betriebe 64

Beschäftigte Personen, Löhne und Gehälter

	Männer	Frauen	Zusammen
1. Tätige Inhaber	76	-	76
2. Angestellte (einschl. Direktoren, Betriebsleiter usw.):			
a) kaufmännische Angestellte und Verwaltungspersonal (einschl. kaufm. Lehrlinge)	7.038	2.452	9.490
b) Werkmeister und technische Angestellte (außer Lehrlingen)	13.704	1.452	15.156
3. Arbeiter (einschl. Gesellen und Lehrlinge)	99.161	2.833	101.994
Insgesamt	119.919	6.737	126.656

B. Wieviel Personen (Inhaber, Angestellte und Arbeiter insgesamt) waren Ende Dezember 1937 in Ihrer Flugzeugzellenfabrik beschäftigt? 120.026

C. Wieviel Löhne und Gehälter (brutto) haben Sie im Jahre 1937 an die in Ihrer Flugzeugzellenfabrik beschäftigten Angestellten und Arbeiter (vgl. I A 2 und 3) insgesamt gezahlt? 302.104090 RM

(b) 1937

Figure 6: StRA Survey Aggregates

According to these files, 8.357 people were employed by the end of June in 1933. In contrast, in 1932, this number stood at 3200, according to Tooze (2006). By December of the same year, this number rose to 14.811. Since it is stated that total wage expenditure in 1933 totaled 21.220.652 RM, this yields a per-worker annual nominal wage of around 1430 RM in 1933. By the end of 1937, employment reached 126.656 workers across 64 plants, representing about 2% of all industrially employed workers in the Reich of that year.[10], with each worker earning around 2.390 RM annually.

These aggregate statistics can be compared with existing secondary sources. In terms of employment, Tooze 2006 estimates that in 1938, around 120.000 workers were employed in airframe manufacturing, 48.000 in engine production, and 70.000 in related equipment and maintenance. Considering that these numbers likely include suppliers of subcomponents[11] to the firms listed in Figure 4, this is further evidence that the StRA

[10]See *StJB* 1938, ch. 10, p. 369.

[11]For instance, important specialized parts were produced by Siemens, AEG, or Bosch, just to name a few, and are not listed in Figure 4.

source offers a comprehensive overview of the Luftwaffe sector.

To put these numbers into perspective, per-capita GDP was 713 RM in 1933, and 1046 RM in 1937. This implies that the average per-worker wage rose by 67% in this period, while per capita GDP rose by 47%. Notice also that the share of the female workforce decreased from about 10% in 1933 to 5% by the end of 1937. Table 1 summarizes descriptive statistics this file allows me to compute.

Table 1: *Flugzeugindustrie* Summary Statistics

Year	Firms	Employed Persons	Avg. Annual Wage	Per capita GDP
1933	15	14 811	1 430 RM	713 RM
1934	23	48 738	1 637 RM	804 RM
1937	64	126 656	2 390 RM	1 046 RM

Notes: BA R 3102/4151 & 5866. *Statistisches Reichsamt, Reichsergebnisse der Produktionserhebungen. Band 2, Flugzeugindustrie.* Wages and GDP in nominal terms reported in StJB 1938, ibid.

One concern regarding these data may be that some of these firms, such as Daimler, did not restrict their production to military goods. Hence, part of the observed economic activity may stem from civilian rather than military demand. However, a outlined by Tooze (2006), this is likely a limited cause for concern. Even if Nazi propaganda tried to convey otherwise, cars were a luxury good unaffordable to the average worker. In 1933, there was one car for 37 households on German roads (Tooze 2006). Additionally, civilian aviation was no where near developed enough to present a large-scale source of employment. The aviation sector was an entirely state-driven enterprise, organized under the notorious Reich Air Ministry, the *Reichsluftfahrtministerium* (RLM), headed by Hermann Göring. The RLM oversaw the entire aircraft sector, it was a market completely driven and controlled by the military needs laid out by the Nazi leadership. Overall, Tooze (2006) assesses that the scope for non-military use of goods produced by the aircraft industry was non-existent.

By focusing on a select group of firms with a clearly identifiable military purpose, this source offers an advantage over relying on aggregated arms data or indices previously used in the literature for the Nazi German case.

2.4 Building the *Luftwaffe* Exposure Variable

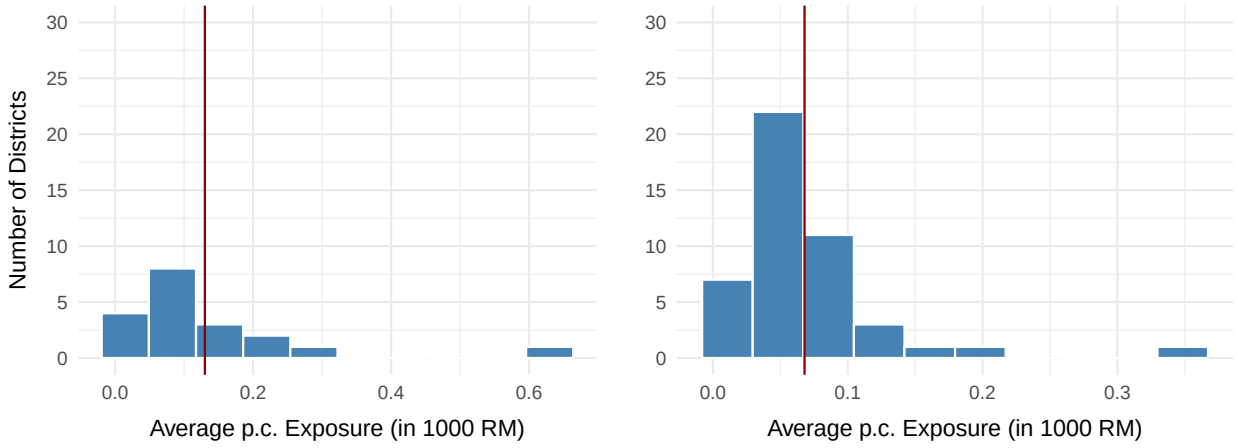
The ingredients hitherto mentioned can now be combined to calculate a local military spending proxy.

To map military spending to each district, I propose the following measure of *"Luftwaffe exposure"* for district d at time t, which I call LW_{dt}:

$$LW_{dt} = \underbrace{LW_{Reich,t}}_{\text{aggregate shift}} \times \underbrace{\frac{Plants_d}{Plants_{Reich}}}_{\text{geographical share}}$$

I compute district-level exposure by weighting the total Luftwaffe budget $LW_{Reich,t}$, which varies across years t, according to the number of plants in each district as a share of all plants in the Reich, which varies across district d. This supposes that the total Luftwaffe budget is distributed evenly across firms.

In total, this approach yields four theoretical exposure estimates, as I compute LW_{dt} using plant locations from both 1933 and 1938, combined with either the official or the estimated (i.e., including shadow budgets) aggregate Luftwaffe expenditure. Alas, only the former are of interest.



(a) Plants 1933 (b) Plants 1938

Figure 7: Luftwaffe Exposure by Districts, Average 1933–1936

Notes: Blue bars show the density of Luftwaffe exposure per capita across exposed districts. Mean exposure per capita is given by the red line.

Figure 7 plots average per capita Luftwaffe exposure in 1000 RM from 1933 to 1936

across all non-zero districts and using the full estimated budgets. The mean across all districts is given by the red line. Districts with plants recorded in 1933 experienced an average annual exposure of about 130 RM per capita, with most plants being located in Berlin, Stuttgart, Rostock, and Dessau. For plants recorded in 1938, the average exposure per year is about 60 RM per capita, and the most exposed districts are Berlin, Brandenburg, Leipzig, and München. This district-level exposure measure is next combined with unemployment and population data from Ettmeier, Kriwoluzky, Papadia, et al. (2024).

Figure 8 shows employment districts scattered according to unemployment and log Luftwaffe exposure for each year in the sample, both in per capita terms. It shows that it takes until 1934 for Luftwaffe exposure to negatively correlate with unemployment, consistent with what was previously shown in Figure 1.

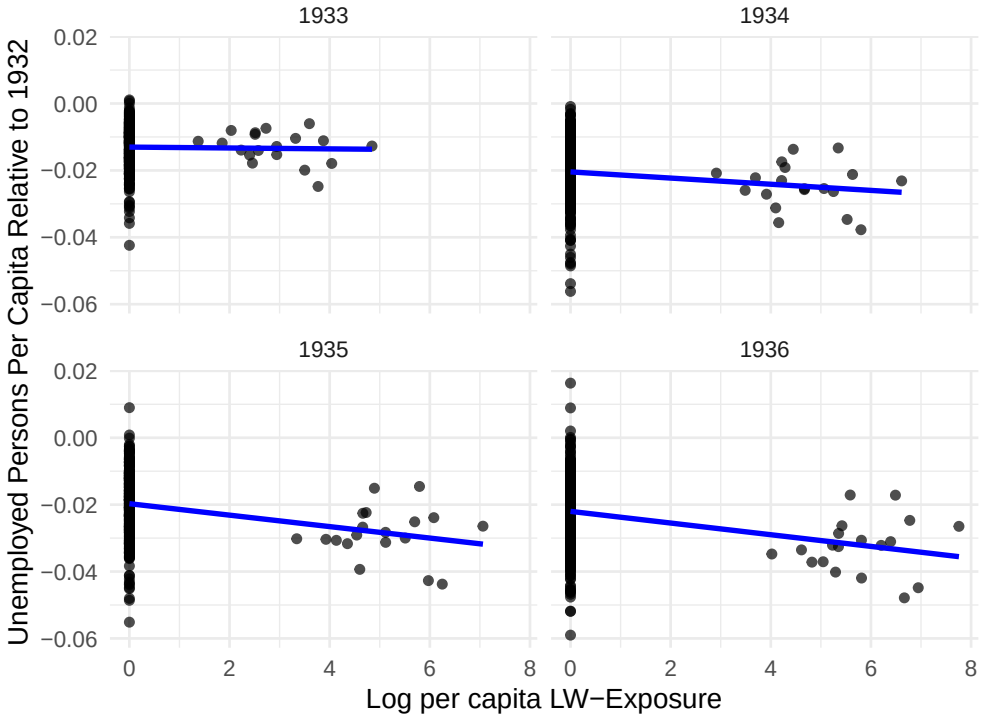


Figure 8: Per capita Unemployment and log Luftwaffe Exposure

Notes: This figure plots unemployment per capita relative to 1932 (y-axis) and log Luftwaffe exposure (x-axis) on a district level.

I view this as evidence that the labor market took until 1934 to react to the military spending shock. Therefore, my main analysis will estimate the effect of Luftwaffe exposure on unemployment *relative* to 1932 as a base year to allow for a cumulative effect over

time. [12].

3 The Employment Effects of Luftwaffe Exposure

3.1 Empirical Strategy and Identification

To estimate the effect of Luftwaffe exposure on unemployment, I make the following identifying assumption, which has previously been made by Nakamura and Steinsson (2014) in the context of military spending in US states. I assume that Nazi Germany did not undertake its military buildup in response to relative economic conditions between German regions. Therefore, regional – or, in my case district-level – variations in Luftwaffe exposure allow me to identify the causal effect of military spending on unemployment. In other words, my identification rests on the exogeneity of my shares (Borusyak, Hull, and Jaravel (2025)), that is, that the distribution of shares is orthogonal to the spending shift.

To ensure that this is the case, my main result is computed using the shares as recorded in 1933 – other variations are shown for illustration. Since 1933 is the year the Nazis came to power, I argue that the shares could not have yet responded to upcoming Nazi policies and spending decisions.

I then estimate the following equation:

$$\Delta U_{dt} = \gamma_t + \beta \Delta LW_{dt} + \epsilon_{dt} \tag{1}$$

where

$$\Delta U_{dt} = \frac{\text{Unemployment}_{dt} - \text{Unemployment}_{d,1932}}{\text{Pop}_{d,1932}}$$

and

$$\Delta LW_{dt} = \frac{\text{Exposure}_{dt} - \text{Exposure}_{d,1932}}{\text{Pop}_{d,1932}}$$

γ_t represents time fixed effects, and LW_{dt} is expressed in 1000 RM. Both variables are divided by district-level population. ϵ_{dt} is an error term.

[12] I will show that my main result holds when estimating the baseline equation in levels as opposed to differences.

I estimate the equation using weighted least squares (WLS), with 1932 population levels serving as observation weights. By estimating the change in unemployed persons per capita relative to 1932, I allow for a cumulative effect of military spending over time.[13] By keeping population constant to 1932 levels in the denominator on the right-hand side, I control for potential population changes that might be driven by Luftwaffe exposure.

3.2 Baseline Results

Table 2 reports estimates for β from Equation 1 using the three different unemployment metrics from Ettmeier, Kriwoluzky, Papadia, et al. (2024). *Panel A* uses the exposure variable constructed from the plant distribution in 1933, while *Panel B* does so using the plant locations in 1938. As outlined previously, I include estimated shadow budgets in my independent variable. Standard errors are clustered at the district level.

Table 2: Unemployment Relative to 1932

	(I) Insured	(II) Crisis	(III) Ins. + Crisis
Panel A: 1933 Plant Locations			
LW_{dt}	-0.0054* (0.0032)	-0.0130** (0.0062)	-0.0184** (0.0092)
Num. Obs.	1755	1755	1755
R^2	0.604	0.494	0.596
R^2 Within	0.015	0.040	0.038
Std. Errors	District	District	District
Panel B: 1938 Plant Locations			
LW_{dt}	-0.0090*** (0.0022)	-0.0288*** (0.0058)	-0.0378*** (0.0071)
Num. Obs.	1755	1755	1755
R^2	0.605	0.517	0.609
R^2 Within	0.018	0.083	0.069
Std. Errors	District	District	District
Fixed Effects	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

[13] I also estimated Equation 1 in levels, but including district-level fixed-effects. See Subsection C.1.

I find statistically significant effects of Luftwaffe exposure on unemployment across all possible specifications. Reassuringly, these effects are present when looking at firm locations in 1933. The effect is larger when looking at the distribution of suppliers in 1938. However, as areas that benefited from the Nazi economic boom could have attracted more suppliers, I show these results for illustration only. Hence, the remainder of the paper will reference the exposure measure obtained using 1933 plant locations and full Luftwaffe expenditure numbers unless otherwise stated.

Thus, my main result, the coefficient on my unemployment variable $Ins. + Crisis$, implies that a 1,000 RM per capita increase in Luftwaffe expenditure is associated with an approximate 2 percentage point decline in unemployment – or, equivalently, a reduction of 18 unemployed persons per 1,000 inhabitants – relative to unexposed districts. The coefficient is statistically significant at the 5 % level. This evidence points to a substantial effect of rearmament in reducing unemployment, even during the early years of Nazi rule, lending support to the results by Fremdling and Stäglin (2015).

To further reinforce this claim, I estimate a standard event-study regression to show pre-trends. This serves as a first exercise to test whether this result is driven by predetermined, longer-term dynamics. I estimate the following equation.

$$\frac{U_{dt}}{Pop_{d,1932}} = \sum_{k \neq 1932} \beta_k \mathbb{1}_{\{t=k\}} \frac{LW_{d,1936-1932}}{\text{Pop}_{d,1932}} + \alpha_d + \gamma_t + \epsilon_{dt} \tag{2}$$

The resulting coefficients are illustrated in Figure 9.

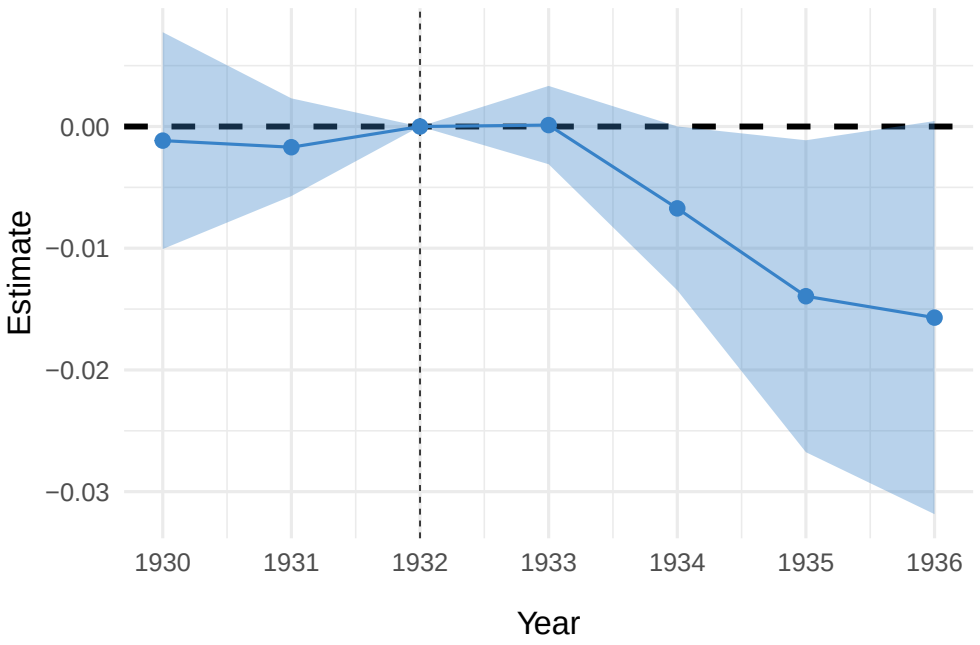


Figure 9: Event-Study Coefficients, 95% Confidence Band

The pre-treatment coefficients are small and statistically indistinguishable from zero, providing no evidence of differential pre-trends prior to the treatment year. Consistent with the hypothesis of this paper, estimates turn negative and grow in magnitude starting in 1934. In 1935, the coefficient is statistically significant at the 5%-level, while the coefficients for 1934 and 1936 are so at the 10%-level.

To further demonstrate the validity of my empirical strategy, I will next extend the analysis by introducing a heterogeneity-robust difference-in-differences (DiD) estimator and testing for spillover effects.

3.3 Heterogeneity-robust Difference-in-differences Estimation

An important shortcoming of my baseline analysis is that it fails to accurately account for heterogeneity in both treatment timing and intensity, as my exposure metric is a continuous variable. By pooling across all districts and years and averaging across treatment intensities, the OLS result cannot be interpreted cleanly as a treatment effect.

Following the recent difference-in-differences (DiD) literature, I address this by repeating my analysis using the heterogeneity-robust estimator proposed by Chaisemartin and d'Haultfoeuille (2023). This estimator only compares units that experience a change

in treatment dose to those that do not, which allows for the computation of a weighted average treatment effect of the treated (WATT).

Due to the large number of untreated units in my setting, Chaisemartin and d'Haultfoeuille (2023) show that the WATT can be obtained through a 2SLS regression, in which the continuous treatment variable is regressed on a binary instrument in the first stage. Applying this framework on my Luftwaffe exposure and across all my specifications, I obtain the results shown in Table 3.

These results indicate a substantial treatment effect on treated units, considerably larger than the average treatment effect obtained previously using OLS: the estimated employment effect increases from around 2 percentage points on average to almost 6 percentage points for treated units. The DiD-coefficient is statistically significant at the 1% level.

What is more, this results suggests that the identifying variation indeed stems from the comparison of exposed and unexposed districts, less so from differences between the exposed ones. This is further evidence in support of my identification strategy.

Table 3: Difference-in-differences Estimates

	(I) Insured	(II) Crisis	(III) Ins. + Crisis
Panel A: 1933 Plant Locations			
LW_{dt}	-0.0280**	-0.0300***	-0.0580***
	(0.0121)	(0.0086)	(0.0159)
Num. Obs.	1755	1755	1755
R^2	1.000	1.000	1.000
R^2 Within	1.000	1.000	1.000
Std. Errors	District	District	District
Panel B: 1938 Plant Locations			
LW_{dt}	-0.0328**	-0.0556***	-0.0883***
	(0.0145)	(0.0117)	(0.0215)
Num. Obs.	1755	1755	1755
R^2	1.000	1.000	1.000
R^2 Within	1.000	1.000	1.000
Std. Errors	District	District	District
Fixed Effects	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

3.4 Spillover Effects into Neighboring Districts

As a final empirical exercise, I test for potential spillover effects. Given the significant local effects I find, it is natural to suppose that proximity to a Luftwaffe supplier could affect local unemployment beyond an exposed district's borders.

To do so, I estimate an analogous version of Equation 1. Here, I define treatment not as a district hosting a Luftwaffe plant. Instead, I consider treated units as districts that share a border with a host district, and omit districts that are both hosts and neighbors simultaneously. Hence, I construct my local share not as the ratio of host districts, but of neighboring ones:

$$\frac{\Delta U_{dt}}{\text{Pop}_{d,1932}} = \gamma_t + \beta \frac{\Delta \text{Neighbor Exposure}_{dt}}{\text{Pop}_{d,1932}} + \epsilon_{dt} \tag{3}$$

where, instead of using

$$LW_{dt} = LW_{Reich,t} \times \frac{Plants_d}{Plants_{Reich}}$$

I define:

$$\text{Neighbor Exposure}_{dt} = LW_{Reich,t} \times \frac{Neighbors_d}{Neighbors_{Reich}}$$

I do this for both my baseline OLS framework, but also using the same DiD-approach as outlined previously for Table 3.

Table 4: Coefficients in Neighboring Districts

	(I) Insured	(II) Crisis	(III) Ins. + Crisis
Panel A: OLS			
Neighbor Exposure$_{dt}$	-0.0012*	-0.0040***	-0.0052***
	(0.0007)	(0.0011)	(0.0017)
Num. Obs.	1623	1623	1623
R^2	0.610	0.447	0.580
R^2 Within	0.002	0.012	0.010
Std. Errors	District	District	District
Panel B: DiD			
Neighbor Exposure$_{dt}$	-0.0021	-0.0082***	-0.0103**
	(0.0017)	(0.0029)	(0.0042)
Num. Obs.	1590	1590	1590
R^2	1.000	1.000	1.000
R^2 Within	1.000	1.000	1.000
Std. Errors	District	District	District
Fixed Effects	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The results are shown in Table 4 and suggest modest, albeit statistically significant unemployment effects if a district is neighboring a district that hosts a Luftwaffe supplier. The OLS estimate suggests that being a neighbor is associated with an approximate 0.5 percentage point reduction in unemployment per capita. As before, the estimated WATT is larger, indicating a 1 percentage point reduction of unemployment per capita among

neighboring districts. This is a reassuring result, as it provides evidence that my baseline results underestimate the true employment effects.

3.5 Discussion of Measurement Error and Estimator Bias

Before continuing with further analysis, this section addresses important limitations and resulting sources of estimator bias.

The primary drawback of relying on Luftwaffe-related suppliers is that they do not cover all branches of the military. Therefore, it is possible that the relative effect I identify is in fact picking up on employment effects of other sources of arms spending, which may be present near Luftwaffe suppliers.

To address this, I repeat the baseline estimation using the total budget of the Wehrmacht in my exposure metric. While this still relies on the geographical distribution implied by the Luftwaffe, using this budget yields a more conservative measure of broader military spending.

Specifically, let:

$$LW_{dt}^{\text{Wehr}} = \underbrace{\text{Wehrmacht}_{Reich,t}}_{\text{aggregate shift}} \times \underbrace{\frac{Plants_d}{Plants_{Reich}}}_{\text{geographical share}}$$

When using this measure as the independent variable, coefficients remain statistically significant, but smaller in magnitude, as the same employment effect is now attributed to a much larger aggregate shift. The OLS coefficient decreases from 1.8 percentage points to 0.5 – the corresponding DiD coefficient falls from 5.8 percentage points to 1.2. The full set of results obtained when using this metric are reported in Subsection C.2. This underscores that while my local identification strategy remains valid, precisely determining the magnitude of the employment effect remains challenging given data limitations.

Additionally, further measurement error is introduced as I impose a proportional distribution of the aggregate Luftwaffe budget onto districts given their respective shares of supplier locations. While this mapping is accurate on average, deviations in some districts are possible.

However, I find it unlikely that this source of mismeasurement be systematically correlated with military spending, as building local shares based on observed firm locations should reasonably approximate the respective weights of exposed districts. Moreover, the results shown so far suggest that the identifying variation originates from the comparison

of unexposed districts to exposed ones, not from within the exposed locations themselves. What is more, if the error introduced by proportional shares is indeed uncorrelated, my estimates will suffer attenuation bias (Wooldridge (2012), ch. 9).

A final source of measurement error lies in the district-level unemployment statistics. Figure 2 shows that the district-level data underestimate unemployment in 1932 and 1933 relative to the Statistical Yearbook. Even the numbers reported in the Statistical Yearbook may suffer from bias. As has been documented by Spoerer and Streb 2013, the Nazis resorted to statistical trickery to mechanically reduce the number of unemployed. They achieved this by, for instance, changing legal definitions or access rights to unemployment insurance. This excluded or discouraged workers from officially registering as unemployed. Overall, Spoerer and Streb (2013) estimate a broader unemployment peak of up to eight million persons.

However, I believe that this measurement error is unlikely to be correlated with my measure of Luftwaffe exposure, as such mis- or underreporting will have likely affected all districts, not just the exposed ones in my sample. In fact, my identification rests on relative differences between exposed and unexposed districts, not absolute levels. Under this no-correlation assumption, my estimates are unlikely to be biased (Wooldridge 2012).

4 Robustness Analysis & Aggregate Quantification

4.1 Controlling for State-Level Trends

The main threat to my identification strategy is that supplier locations, may have been influenced by pre-existing regional characteristics, such as historical industrial structures or patterns of agglomeration. By focusing on locations in 1933, I aim to mitigate a contamination of my results by recovery dynamics that occurred after the Nazi regime came to power.

To further take pre-existing economic dynamics into account, I undertake two additional empirical exercises. First, I repeat the baseline estimation on a district level, but including state-time fixed effects. Then, I estimate unemployment effects entirely on a state level to compare with the district-level results.

The states, the *Länder*, are the second-highest administrative unit in Germany. Including state-time fixed effects allows me to test whether Luftwaffe suppliers may have already been located in better-recovering states, thus potentially biasing the coefficients. It

is plausible that any underlying endogeneity due to historical trends should be detectable beyond the district level, as those likely caused spillover effects over time.

Therefore, when including state-time fixed effects, Equation 1 becomes:

$$\Delta U_{dt} = \alpha_{lt} + \beta \Delta LW_{dt} + \epsilon_{dt} \tag{4}$$

where α_{lt} represents the Land-level state-time fixed effect.

The results are shown in Table 5. Compared to the baseline specification, the coefficients increase slightly. The fact that the effect is stronger when restricting comparison to districts within the same state $\times$ year cell shows that the effect of Luftwaffe exposure is persistent within states, and that the overall result is not driven by Luftwaffe suppliers inherently being located in better performing ones. This is a reassuring result: even in states with traditionally different recovery dynamics, Luftwaffe exposure still seems to have a significant effect on unemployment.

Table 5: Including State-Time Fixed Effects

	(I) Insured	(II) Crisis	(III) Ins. + Crisis
Panel A: 1933 Plant Locations			
LW_{dt}	-0.0063* (0.0033)	-0.0131** (0.0052)	-0.0194** (0.0083)
Num. Obs.	1735	1735	1735
R^2	0.663	0.526	0.639
R^2 Within	0.019	0.035	0.038
Std. Errors	District	District	District
Panel B: 1938 Plant Locations			
LW_{dt}	-0.0097*** (0.0028)	-0.0262*** (0.0063)	-0.0359*** (0.0087)
Num. Obs.	1735	1735	1735
R^2	0.664	0.541	0.648
R^2 Within	0.022	0.066	0.062
Std. Errors	District	District	District
Fixed Effects	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This result motivates a deeper analysis on the Länder-level. Länder-level data is, by nature, much less detailed than district-level data. However, moving to this higher unit of analysis allows me to introduce further data to control for state-level industrial trends.

Combining *Länder*-level data by Hohls and Kaelble (1989) and national industrial production numbers from the League of Nations (1939), I construct an *industrial exposure* proxy analogous to my measure LW_{dt}. This measure interacts state-level industrial employment shares with national industrial output.

$$\text{Ind. Exposure}_{lt} = \text{Production Index}_{Reich,t} \times \frac{\text{Ind. Employment}_{l,1933}}{\text{Pop}_{l,1933}}$$

The availability of the industrial production index by the League of Nations allows me to compute this metric for 1933. Additionally, the data by Hohls and Kaelble (1989) include industrial employment shares for 1925 and 1907, which I include as additional control variables.

Moreover, using state-level data has the advantage of extending the period of observation up to 1938. This allows me to test how the unemployment effect changes beyond 1936, which is when full employment was reached.

To do so, I run the identical event-study regression as in Equation 2, including the additional controls described above, that is, industrial exposure and employment based on Hohls and Kaelble (1989). I also control for the unemployment lag. The coefficients are shown in Figure 10 As the number of states is much lower than the number of districts, confidence intervals are computed using bootstrapped standard errors.

On a state level, I find effects of Luftwaffe exposure on unemployment statistically different from zero at the 5% level as early as 1933. What is more, this finding suggests that the strength of the labor market recovery was linked to the unused economic capacity, that is, the large number of unemployed persons in 1932. The significant effect of Luftwaffe exposure is detectable on the state level only until 1936.

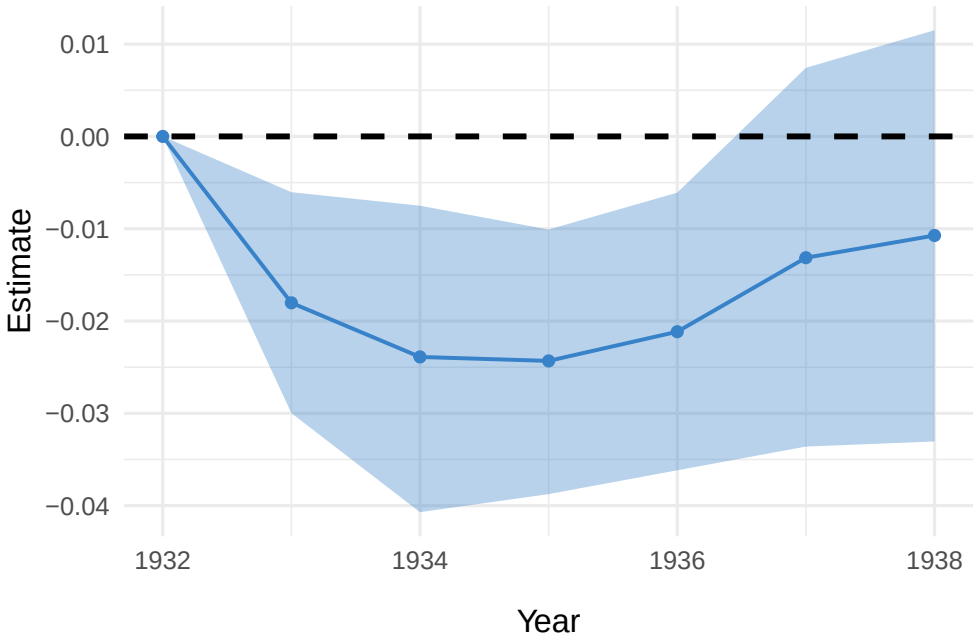


Figure 10: State-level Event-Study Coefficients, 95% Confidence Band

Notes: Event-study coefficients from a regression run on the state level. Due to data limitations in the Statistical Yearbook, no data pre-1932 is available. Standard errors are computed using a cluster bootstrap procedure in which employment districts are resampled with replacement and the model is re-estimated in each replication; reported standard errors correspond to the standard deviation of the resulting coefficient distribution.

This hypothesis has previously been examined by Ramey and Zubairy (2018), who find that the amount of slack in an economy had a limited effect on defense spending multipliers. While a large part of this literature analyzes time-series data, I provide cross-sectional evidence that, for the case of the Nazi German recovery, excess labor supply seems important in determining the strength of employment effects.

Detailed regression results are presented in Table 12 in the appendix. They also show that, when restricting the period of observation to 1936, the coefficient becomes larger and more statistically significant, which further underscores the diminishing effects once the labor market had fully recovered.

Overall, these results support the hypothesis that the observed employment boom cannot merely be explained by aggregate trends or pre-existing economic conditions. As a final exercise, I check whether changes in unemployment per capita could have been driven by compositional effects in the population.

4.2 Population Growth Effects

The evidence of spillover effects documented previously naturally raises questions about the relationship between military spending and population dynamics. It could be that more exposed areas attracted larger inflows of workers from outside their own district, or that migration outflows mechanically reduced the number of unemployed in treated areas by reducing the size of the labor force. To test this, I estimate an equation similar to the baseline, but now using district-level population as the dependent variable. This yields:

$$\Delta Pop_{dt} = \gamma_t + \beta \Delta LW_{dt} + \epsilon_{dt} \tag{5}$$

I equivalently repeat this exercise for neighboring districts and using the same DiD estimator as I did for the spillover effect estimation. The results are shown in Table 6.

Table 6: Population Relative to 1932

	Treated Units	Neighbors
OLS		
Exposure 1933	-0.1502***	-0.0482**
	(0.0219)	(0.0210)
Num. Obs.	1303	1303
R^2	0.094	0.097
R^2 Within	0.001	0.004
Std. Errors	District	District
DiD		
Exposure 1933	-0.1054***	-0.0823*
	(0.0276)	(0.0427)
Num. Obs.	1269	1269
R^2	1.000	1.000
R^2 Within	1.000	1.000
Std. Errors	District	District
Fixed Effects	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

I find a statistically significant and negative relationship between the change in Luftwaffe exposure and population growth. The coefficient remains negative even for neighboring districts. Several factors may explain this interesting result.

First, Nazi policies restricted (labor) mobility as early as 1934, for instance, by limiting relocation into larger cities and agglomerations (Petzina 1970). Second, it is possible that workers migrated away from treated units within the scope of work creation projects. Work creation projects were undertaken between 1932 and 1935 as a series of public investment and employment schemes, most notably in the construction and infrastructure sectors (Grebler 1937).[14] It is for those projects that the Nazi administration intervened actively into the labor market and labor allocation immediately after their seizure of power, as they allowed for a swift, state-driven reduction in unemployment numbers by reclassifying workers as employed within these programs (Spoerer and Streb 2013). Treated districts may have been less likely to benefit from such a reallocation as the growing arms sector could only absorb so many unemployed workers once arms production increased (Petzina 1970).[15]

While my estimating equation expresses unemployment and Luftwaffe exposure in 1932 per capita terms, if unemployed workers move from treated to untreated areas, this would imply an overstatement of the rearmament effect in treated units. Conversely, if untreated units were more likely to experience a labor market recovery due to the reorganization of labor in the scope of work creation, this would weaken the treatment effect between the treatment and control districts. Hence, the net effect is, at least, ambiguous.

However, it is unclear how labor migration linked to work creation is reflected in the district-level unemployment data. Lower-skilled labor, which was more likely to be affected by these programs, was less likely to register in unemployment insurance in the first place (Spoerer and Streb 2013). These workers would then be excluded in the numerator, but appear in the population statistics and drive the results in Table 6. Moreover, the results in Subsection C.4 find no evidence of a statistically significant effect of the population level in driving the overall results.

Overall, the evidence presented in Figure 4 and Tooze (2006) clearly points to an undeniable surge of employment in the Luftwaffe sector. Therefore, while I cannot rule out compositional effects in the labor force due to obvious data limitations, I believe that this result does not invalidate the evidence that Luftwaffe employment increased sharply as a result of the sharp drive in armaments production.

[14] It ought to be noted that the funds spent on work creation were dwarfed by military spending as early as 1934. In that year, no new such funds were made available as the focus had already fully shifted to rearmament (see Tooze (2006), ch. 2.)

[15] It is for this reason that the Nazi administration intervened to allocate labor into rearmament-related sectors only after full employment had been reached (Petzina 1970).

4.3 Aggregate Quantification Exercise

This paper has, so far, made no claim about the aggregate effects of military spending in the Nazi economy. While the previous sections establish the robustness of the local identification strategy, mapping these estimates into aggregate outcomes is not straightforward.

To address the natural question of aggregate magnitudes, I perform a back-of-the-envelope extrapolation of the estimated effects. Specifically, I scale the estimated local effects of Luftwaffe exposure to the aggregate level of military and public spending.[16] This exercise is conducted in the absence of a structural model and should be interpreted as a partial-equilibrium approximation rather than as an estimate of aggregate causal effects. A structural analysis of the Nazi economy would require further work on the politically enforced interventions into market mechanisms and warrant a paper on its own.

Extrapolating local treatment effects to the aggregate level raises well-known challenges. In particular, scaling by aggregate shifts suffers from the missing intercept problem highlighted by Moll and Hanney (2025). Moreover, this approach implicitly assumes that the marginal effects identified in the cross-section remain invariant when applied to large-scale policy changes. The resulting magnitudes should therefore be interpreted with caution.

Concretely, I construct a counterfactual change in unemployment per capita by combining the estimated treatment effects with the observed aggregate exposure. To approximate the total marginal effect, I sum the baseline and spillover coefficients and multiply this combined coefficient by cumulative exposure per capita between 1933 and 1936. Formally, I compute:

$$\text{Average Effect} \approx \beta \cdot \frac{\sum_{d,t=1933}^{1936} \text{Exposure}_{dt}}{\sum_{d} \text{Pop}_{d,1933}}$$

This corresponds to a linear projection that holds the estimated coefficient fixed and abstracts from general equilibrium adjustments. In particular, it assumes that untreated districts are not affected by aggregate spillovers, an assumption that is unlikely to hold in practice but useful for gauging magnitudes.

[16] Appendix D discusses an alternative Bayesian estimation framework to identify aggregate effects by combining local cross-sectional and aggregate time-series information, following Matthes, Nagasaka, and Schwartzman (2025).

Table 7: Predicted Reductions in Unemployment per capita

Spending Type	**Per Capita (RM)**	β_{OLS} **(in pp)**	β_{DiD} **(in pp)**
Luftwaffe	83	–0.2	–0.6
Military Total	339	–0.8	–2.3
Government Spending	1385	–3.3	–9.5

Notes: These predicted effects are obtained by multiplying the sum of the baseline and spillover coefficients with the respective aggregate per capita expenditure. That is, coefficients taken from Table 2, Table 3 & Table 4. The respective aggregated coefficient is multiplied by the number in column 1, and converted into percentage points.

The results are reported in Table 7. Column 1 displays spending per capita by category in Reichsmark. Columns 2 and 3 report the implied change in unemployment per capita (in percentage points) based on the OLS and DiD estimates, respectively. The DiD coefficients imply an aggregate effect of up to 2.3 percentage points.

For comparison, data from the *Statistisches Jahrbuch* indicate a peak unemployment rate of 9.4% in February 1932, declining to 2.2% by the end of 1936.[17] Relative to this observed decline, the implied magnitude suggests that military spending could account for a non-negligible share of the reduction in unemployment. A simple ratio of the implied effect to the total decline yields a figure of approximately 32%, though this comparison should be interpreted as an order-of-magnitude benchmark rather than as a structural decomposition.

Repeating the exercise for total government spending yields implied effects that exceed 100% of the observed decline in unemployment. This highlights that the linear extrapolation is highly sensitive to the magnitude and composition of spending.

Overall, this exercise provides a consistency check on the magnitudes implied by the local estimates, but does not constitute evidence on aggregate causal effects. They show that military spending may have played an economically meaningful role at the aggregate level.

5 Concluding Remarks

This paper provides new empirical evidence on the effects of rearmament on unemployment in Nazi Germany. Using a newly assembled archival dataset that pinpoints the geo-

[17] *Statistisches Jahrbuch*. 1934: pp. 5 and 292. 1937: pp. 5 and 350.

graphic distribution of firms supplying the German *Luftwaffe*, I build a regional measure of exposure to military expenditure.

I show that districts hosting Luftwaffe-related production experienced a statistically significant decline in unemployment of 1.8 percentage points relative to districts that do not. This result is robust to various specifications including different control variables. Estimation using an appropriate DiD-estimator reveals large treatment effects on treatment units of around 6 percentage points.

Additionally, I find evidence for statistically significant spillover effects, indicating that my results likely underestimate the true effect of military spending. My results suggest an active role of Nazi economic (or, perhaps more accurately, rearmament) policy in contributing to the German upswing of the early 1930s. This likely reinforced popular support for the regime and facilitated its transition to a war economy.

These results, and particularly this new data source, opens up numerous avenues for future research. It could be of interest to examine individual suppliers (or at least those with accessible archival records) to investigate the firm-level effects of the Nazi rearmament shock further. Another possible next step could be to extend the spatial aspects of this analysis by refining the estimation of spillovers. Moreover, the Nazi system of closed credit cycles and shadow budgets remains vastly understudied in the existing literature. A next paper could try to model these aspects more formally: this could not only allow for an improved discussion of aggregate effects, but invite the literature to reflect on how structural assumptions may change when operating under a politically controlled, and profoundly illiberal, economic framework. Such research could therefore shed light on how autocratic states maintain economic resilience in the context of military conflict — an area of growing relevance as the economic consequences of geopolitical tensions and war unfortunately demand renewed scholarly attention.

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Appendix

A Discussion of Data Sources and Archival Material

With the exception of district-level data, which has been provided by Ettmeier, Kriwoluzky, Papadia, et al. (2024) and for which I am incredibly grateful, all data used in this research project has been hand collected from primary and secondary sources. I have done so at the *Bundesarchiv Berlin-Lichterfelde*, the library of the *Deutsches Institut für Wirtschaftsforschung* (DIW Berlin) and the *Bibliothèque nationale de France* in Paris, where I was able to discover various primary and secondary sources at various stages when researching this paper. I am indebted to the staff at all three of those institutions for their helpful suggestions and support, particularly Diana Fuenmayor, Katja Buro and Katharina Zschuppe.

A statistical source frequently cited in this project is the Statistical Yearbook of the German Empire (*Statistisches Jahrbuch für das Deutsche Reich*), which I accessed online using this link. At the DIW, I was able to look at the *Wochenberichte* and the *Konjunkturstatistische Handbücher*. I also consulted the editions of *Vierteljahreshefte zur Statistik des Deutschen Reichs*, the *Lageberichte der Deutschen Kreditgesellschaft AG* and *Wirtschaft und Statistik*, two recurring publications of the StRA, to construct time-series of additional outcome variables that I did not analyze further in this paper.

While in Berlin, my main focus were the archives of the Reichsbank, the StRA and the Reich Ministry of Finance. Archival files cited in this project are:

- **R 2501/1760.** *Berichte zur allgemeinen Konjunktur, Band 27.*
- **R 3102/2700.** *Wirtschaftsfinanzierung.*
- **R 3102/7132.** *Deutsche Kriegsfinanzierung und Wirtschaftspolitik.*
- **R 3102/2482.** *Arbeitsbeschaffung: Auswirkung und Finanzierung.*
- **R 3102/3666, 4151 & 5866.** *Reichsergebnisse der Produktionserhebungen, Band 2: Flugzeugindustrie.*

B Supplementary Background

This appendix section provides additional background on elements of Nazi economic policy.

Overall, Nazism did not elaborate a distinct economic ideology or theory. The key element was that the economy was subordinate to the objectives of the National Socialist state and the *Führer*. Many economic policymakers from the Weimar era continued to serve under the Nazi administration, including Hjalmar Schacht (Barkai 1990). The regime relied on stimulating public demand while simultaneously maintaining a strict control over economic processes to limit inflation risks (Albers 1976, Boelcke 1992).

The contradictory nature of these two objectives was balanced by large-scale government control and interventionism. Thus, Nazi economic policy can be characterized by three pillars.

B.1 I. Fiscal Expansion

The first pillar is a vast expansion of *debt-financed* government spending: Figure A1 shows the evolution of nominal Reich government debt by year.[18] Debt was oriented away from foreign creditors into medium- and long-term obligations held domestically (Stucken 1964, Banken 2020).

[18] *Statistisches Handbuch von Deutschland*, p. 555, *Reichshaushalt, Reichschsschuld und Notenumlauf*.

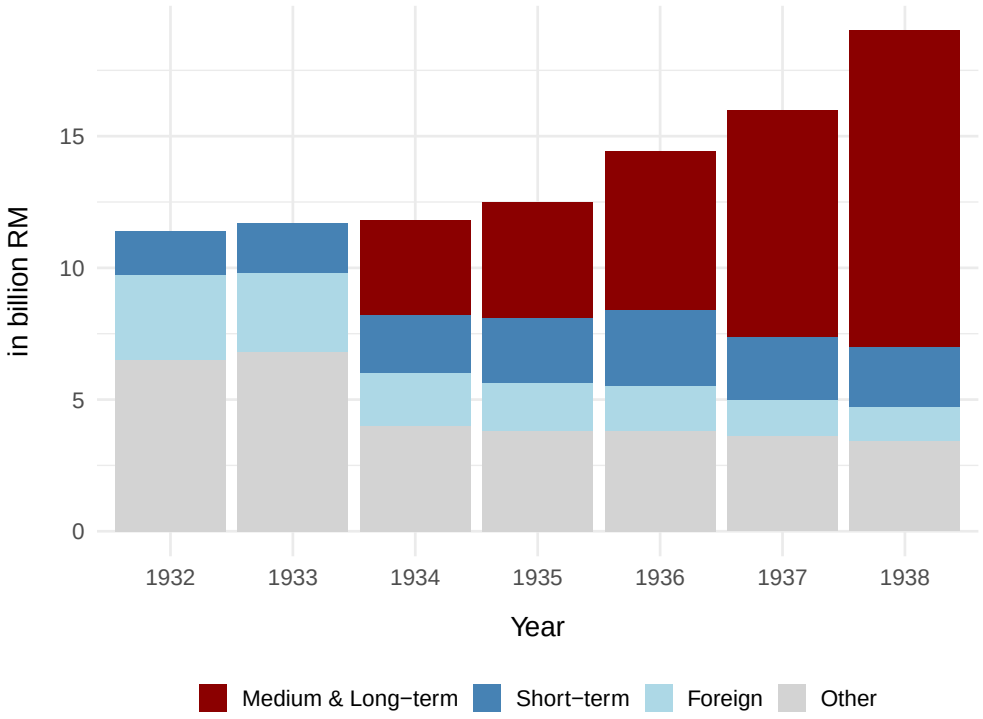
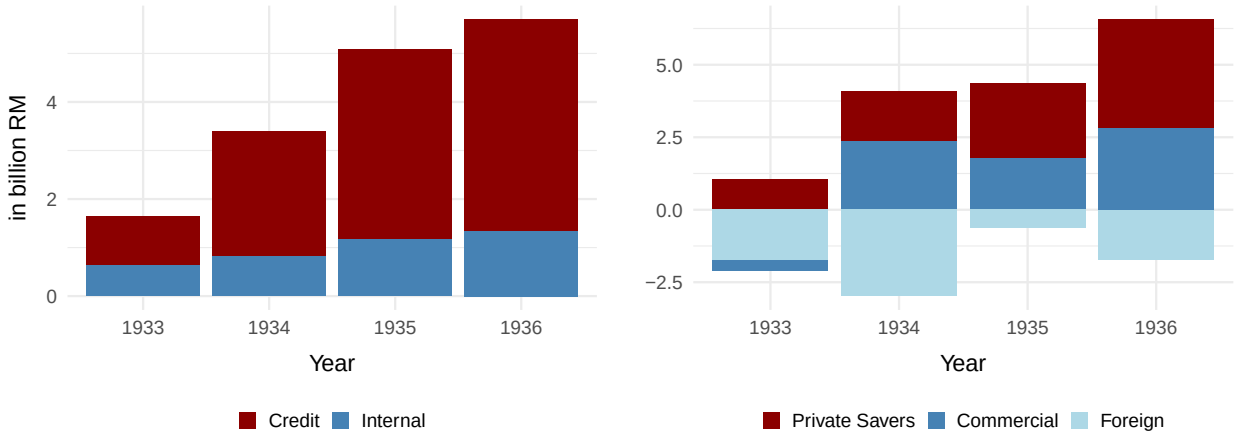


Figure A1: Components of Reich Public Debt

This is also illustrated in Figure A2, which decomposes the credit sources of nominal public investment in the Nazi economy from 1933–1936, according to a confidential document by the Reich Statistics Office (StRA).[19] Not only was public investment predominantly financed by an expansion of credit (panel *a*), it was also directly linked to credit supplied by private domestic savers, that is, deposits and insurances (in red, panel *b*), while foreign credit experienced a sharp net outflow:

[19] *BA R 3102/2700*



(a) Sources of Public Investment

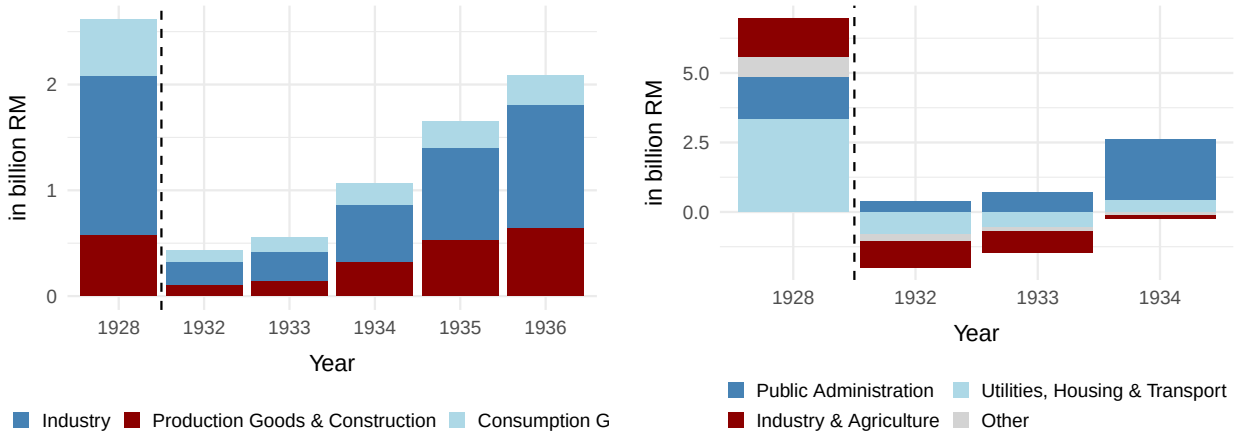
(b) Annual Changes in Credit Flows

Figure A2: The Financing of Economic Activity, 1933–1936

This was a direct consequence of the second pillar of Nazi economic policy, which was the gradual dismantling of market-based economic mechanisms to absorb purchasing power from the private sector and place it into government debt.

B.2 II. Regulation of Private Demand

First, government intervention and regulation served to stimulate the economy while limiting the response of *private* demand and containing potential inflationary dynamics (Boelcke 1992). As soon as economic activity increased and unemployment fell, the government, particularly the Ministry for Economic Affairs (*Reichswirtschaftsministerium*), began strictly regulating prices, wages and private corporate management. Especially agricultural goods were subject to the oversight of a price commissioner, wages could no longer be negotiated autonomously as unions were effectively banned, firms could no longer decide whom to hire or fire, corporations could no longer pay dividends, and workers were no longer freely able to switch employers (Boelcke 1992, Ritschl 1992). This also had consequences on the levels and sources of investment in the economy, as illustrated in Figure A3:



(a) Decomposition by Sector

(b) Decomposition by Source, Net of Depreciation

Figure A3: Investment in the Nazi Economy (1928 RM)

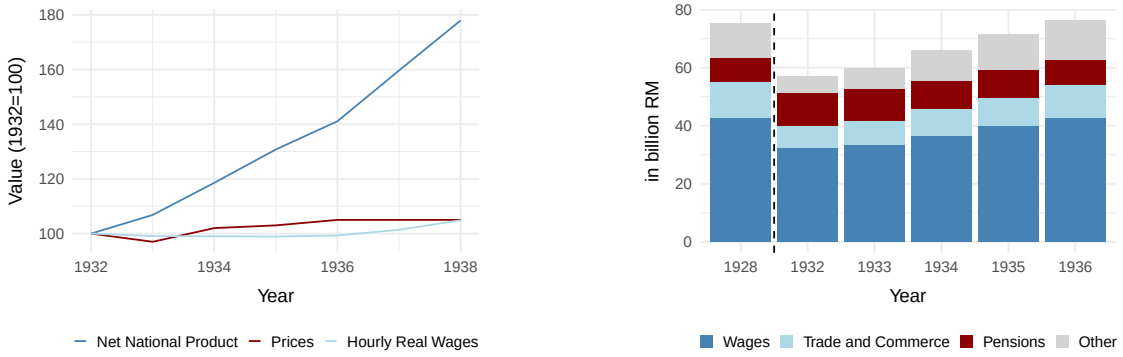
The left-hand side panel[20] shows the decomposition of *private* investment by sector. By 1936, aggregate private investment had still not reached levels before the Great Depression, shown by the numbers from 1928 for reference. Moreover, investment is mostly concentrated into industrial and production goods investment, at the expense of consumption goods, as investment therein remains well below 1928 levels. The right-hand side panel[21] shows the decomposition of aggregate investment *net of depreciation*. Unfortunately, this data series in the StJB is not published after 1934, but the growing importance of public investment is evident, while other sectors are still recovering from the downturn experienced following the Great Depression. This marked a clear policy departure from the previous Brüning administration.

Nevertheless, the economic rebound was so strong that, despite these measures, upward pressure on prices and wages emerged, leading to shortages and the rise of black markets – underscoring the speed with which the German economy approached full employment (Caesar and Hansmeyer 1976). Similarly, foreign trade, which had been governed by strict capital controls and bilateral clearing agreements since the 1931 banking crisis, was reduced as efforts were made to relocate production of war-relevant goods and materials into Germany (Ritschl 1992). To maintain control over the price level and the *Reichsmark*, foreign currency circulation was placed under the direct supervision of the Reichsbank, as reserve levels were constantly and critically low and largely used for

[20] *StJB 1937*, p. 539, *Die volkswirtschaftlichen Investitionen.*
[21] *StJB 1937*, p. 540, *Die Anlageinvestitionen der deutschen Industrie.*

essential raw material imports (Stucken 1964).

Combined, this limited the growth of private consumption despite the economic upswing. Albers (1976) argues that increased employment did not result in higher real wages during the early 1930s, while consumption was depressed by rationing, taxation[22] or the deterioration of the quality of goods, thus having virtually no effect on living standards. According to Barkai (1990), between 1933 and 1936, public expenditure rose by 18.7% *per year*, while private consumption only rose by an annual 3.6%. Ritschl (1990) estimates that private consumption reached pre-Great Depression levels only in 1936. The left-hand side panel of Figure A4, expressed in 1932 prices and levels, shows that despite sustained growth in real net national income, prices and real hourly wages grew by barely 5% by 1938. Real hourly wages returned to pre-Great Depression levels only between 1936 and 1937.[23]



(a) Indexed to 1932 = 100

(b) Decomposition of National Income, 1928 RM

Figure A4: National Income, Prices, and Wages

Looking at national accounts paints a similar picture: the right-hand side panel of Figure A4 shows a decomposition of national income expressed in 1928 Reichsmark:[24] Taken together, these first two pillars served to generate *excess purchasing power*: stimulate the economy while containing public expenditure. Official Reichsbank documents lay out this economic policy by using the word *Kaufkraftabschöpfung*, which literally translates to *absorption of purchasing power*. This then fed into the third and final pillar.

[22]Indeed, the Nazi administration maintained most tax rates from the Brüning deflationary era. Some consumption and income tax rates were even increased (Ritschl 1992).

[23]NNP: Albers (1976). Prices and wages: *Statistisches Handbuch von Deutschland*, p. 463, *Reichsindexziffern für die Lebenshaltungskosten*, and p. 472, *Indexziffern der Arbeitsverdienste*.

[24]National income as per StJB 1938: *Volkswirtschaftliche Bilanzen, Das deutsche Volkseinkommen.*, p. 559.

B.3 III: Financial Repression

Lastly, the Nazi state established an elaborate system of shadow banking to siphon purchasing power to finance work creation and rearmament by absorbing excess savings through the financial system.[25] As individuals had few opportunities to consume and corporations few opportunities to invest, savings were channeled into the remaining asset which the Reich was happy to supply in abundance: government debt (Oshima 2006). Through a framework called the *Geräuschlose Methode*, or *silent method*, the Nazi state created a closed credit cycle through which all private and non-state economic actors became, directly or indirectly, creditors of the Reich (Caesar and Hansmeyer 1976). This served to conceal the true nature of government (notably military) expenditure. While a comprehensive treatment of this opaque system lies beyond the scope of this paper, several key elements are necessary to grasp the full extent to which the Nazi state directed the economy to finance its military objectives. [26]

[25] To quote an internal Reichsbank document: *"Every nation must save, and a nation with extraordinary tasks especially so."* BA R2501/7132, author's translation.

[26] This section is based on Grebler (1937), Stucken (1964), Caesar and Hansmeyer (1976), Oshima (1991), and Oshima (2006).

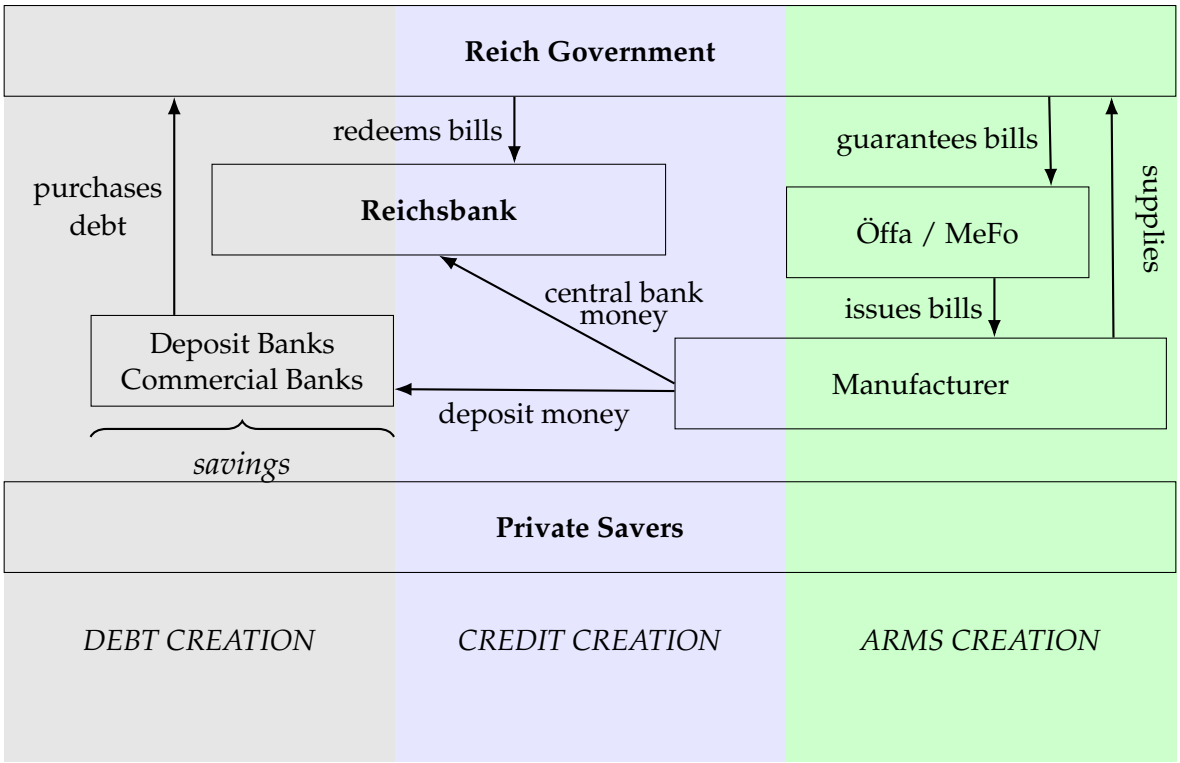


Figure A5: Shadow Credit in the Nazi Economy. Author's Illustration

A simplified illustration is provided in Figure A5. The key instruments used were specifically created bills, known as *Wechsel*, issued by shadow companies that only circulated in the financial system.

Between 1932 and 1937, the Nazi government invested into work creation programs by emitting work creation bills through various government subsidiaries, notably the *Deutsche Gesellschaft für Öffentliche Arbeiten*, or *Öffa*. These bills were used by the *Öffa* to pay for work creation-related projects, such as construction or infrastructure. Importantly, these bills were guaranteed by the Reich, making them eligible for discounting at the Reichsbank. Hence, bills were stored by the Reichsbank or commercial banks as a liquid asset. [27] Oshima (1991) estimates that around 5 billion RM were invested in work creation programs, 37% of which was financed through work creation bills.[28]

[27] These instruments were, however, no Nazi invention, and many work creation programs had already been passed under the previous von Papen and von Schleicher governments by 1932. The Nazis did, however, use *Wechselfinanzierung* at an unprecedented scale with full cooperation of the Reichsbank.

[28] *Tabelle 15, Arbeitsbeschaffungsmaßnahmen der Reichsregierung.*

The same principle was used to finance rearmament by creating a fictitious company, the *Metallurgische Forschungsgesellschaft mbH*, which emitted *Mefo-bills*. These bills had a three-month maturity, but were often extended up to five years.[29] An arms contractor would receive Mefo-bills as payment, which could then be discounted for cash at the Reichsbank or deposited at commercial banks. The Reich would then use revenue from emitting government debt to redeem those bills at the Reichsbank once matured. This meant that a large part of government expenditure was financed by shadow debt instruments outside official government accounts. Oshima (1991) estimates that *Mefo*-bills accounted for 60-70% of Wehrmacht expenditure between 1934 and 1937.

The Nazi economy was, thus, geared towards military conflict from the beginning. The initial focus on reducing unemployment was a strategic consideration to channel the productive forces of the German economy into the ultimate goal of rebuilding its military, to which full employment was both a practical and ideological *means*.[30] This was achieved by placing credit creation at the Reich's disposal. In the Nazi state, unemployment and rearmament went hand in hand.

[29] In theory, the Reich was supposed to use this system as a temporary instrument, and finance military expenditure through regular government revenue in the medium term. By 1939, billions worth of such bills were scheduled to be repaid by the Reich. Hitler, however, had no intention of doing so, which led to the resignation of the Reichsbank board the same year.

[30] This becomes abundantly clear from internal Reichsbank documents, for instance BA R2501/7132 or R3102/2482, which elaborate how the Reichsbank saw itself as an integral component to fulfilling the goals of the National Socialist state by catering the credit supply to its "needs". See also Barkai (1990) and Spoerer and Streb (2013), ch. 6.

C Supplementary Results

C.1 Level Regression

As an additional robustness check, I re-estimate Equation 1 in levels, but including district fixed-effects, that is:

$$\Delta U_{dt} = \alpha_d + \gamma_t + \beta \frac{LW_{dt}}{\text{Pop}_{d,1932}} + \epsilon_{dt}$$

This yields the following result:

Table 8: Unemployment Relative to 1932

	(I) Insured	(II) Crisis	(III) Ins. + Crisis
Panel A: 1933 Plant Locations			
LW_{dt}	-0.0060**	-0.0106**	-0.0166**
	(0.0027)	(0.0053)	(0.0077)
Num. Obs.	2459	2459	2459
R^2	0.916	0.868	0.901
R^2 Within	0.008	0.023	0.024
Std. Errors	District	District	District
Panel B: 1938 Plant Locations			
LW_{dt}	-0.0101***	-0.0223***	-0.0324***
	(0.0024)	(0.0047)	(0.0062)
Num. Obs.	2459	2459	2459
R^2	0.917	0.870	0.903
R^2 Within	0.010	0.043	0.039
Std. Errors	District	District	District
Fixed Effects	✓	✓	✓

[1] *This includes estimated shadow budgets resulting from* Wechsel-*circulation.*
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C.2 Baseline Results Using Wehrmacht Total

Table 9: Baseline Using Wehrmacht Total (OLS)

	(I) Insured	(II) Insured	(III) Ins. + Crisis
Panel A: Wehrmacht Exposure 1933			
LW_{dt}^{Wehr}	-0.0060* (0.0035)	-0.0015* (0.0009)	-0.0052** (0.0026)
Num. Obs.	1755	1755	1755
R^2	0.604	0.604	0.597
R^2 Within	0.016	0.016	0.040
Std. Errors	District	District	District
Panel B: Wehrmacht Exposure 1938			
LW_{dt}^{Wehr}	-0.0099*** (0.0023)	-0.0026*** (0.0006)	-0.0107*** (0.0020)
Num. Obs.	1755	1755	1755
R^2	0.605	0.605	0.610
R^2 Within	0.019	0.019	0.071
Std. Errors	District	District	District
Fixed Effects	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Baseline Using Wehrmacht Total (DiD)

	(I) Insured	(II) Insured	(III) Ins. + Crisis
Panel A: Wehrmacht Exposure 1933			
LW_{dt}^{Wehr}	-0.0182** (0.0079)	-0.0057** (0.0025)	-0.0118*** (0.0032)
Num. Obs.	1755	1755	1755
R^2	1.000	1.000	1.000
R^2 Within	1.000	1.000	1.000
Std. Errors	District	District	District
Panel B: Wehrmacht Exposure 1938			
LW_{dt}^{Wehr}	-0.0213** (0.0094)	-0.0067** (0.0030)	-0.0180*** (0.0044)
Num. Obs.	1755	1755	1755
R^2	1.000	1.000	1.000
R^2 Within	1.000	1.000	1.000
Std. Errors	District	District	District
Fixed Effects	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Spillovers Using Wehrmacht Totals

	(I) Insured	(II) Crisis	(III) Ins. + Crisis
Panel A: OLS			
LW_{dt}^{Wehr}	-0.0003* (0.0002)	-0.0008*** (0.0002)	-0.0011*** (0.0004)
Num. Obs.	1623	1623	1623
R^2	0.611	0.448	0.580
R^2 Within	0.003	0.013	0.010
Std. Errors	District	District	District
Panel B: DiD			
LW_{dt}^{Wehr}	-0.0004 (0.0003)	-0.0016*** (0.0005)	-0.0020** (0.0008)
Num. Obs.	1590	1590	1590
R^2	1.000	1.000	1.000
R^2 Within	1.000	1.000	1.000
Std. Errors	District	District	District
Fixed Effects	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C.3 *Länder*-level Regression

Table 12: State-Level Coefficients

	(I)	(II)
Panel A: Full Sample		
LW_{dt}^{1933}	-0.0251*	
	(0.0136)	
Industrial Exposure	-0.0004	-0.0003
	(0.0006)	(0.0006)
Ind. Empl. p.c. 1925	0.1182***	0.1288***
	(0.0306)	(0.0315)
Ind. Empl. p.c. 1907	-0.3865**	-0.4074**
	(0.1371)	(0.1461)
Lagged Unemployment	-0.0432	-0.0278
	(0.2620)	(0.2713)
LW^{1938}		-0.0446
		(0.0343)
Num. Obs.	86	86
R^2	0.876	0.874
R^2 Within	0.569	0.562
Std. Errors	Land	Land
Panel B: Until 1936		
LW_{dt}^{1933}	-0.0504***	
	(0.0164)	
Industrial Exposure	-0.0010	-0.0009
	(0.0007)	(0.0008)
Ind. Empl. p.c. 1925	0.1210***	0.1370***
	(0.0312)	(0.0330)
Ind. Empl. p.c. 1907	-0.2540	-0.2842*
	(0.1452)	(0.1566)
Lagged Unemployment	-0.1807	-0.1598
	(0.2208)	(0.2333)
LW^{1938}		-0.0945*
		(0.0445)
Num. Obs.	73	73
R^2	0.884	0.882
R^2 Within	0.580	0.572
Std. Errors	Land	Land
Fixed Effects	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C.4 District-level Regression With Additional Controls

Table 13: Unemployment Relative to 1932

	(I)	(II)	(III)	(IV)	(V)
Exposure 1933	-0.0235**	-0.0219**	-0.0243**	-0.0253**	-0.0233**
	(0.0109)	(0.0108)	(0.0115)	(0.0117)	(0.0103)
Lagged Unemployment p.c.	-0.1581***				-0.0632
	(0.0299)				(0.0481)
Population Level		-0.0000			-0.0000
		(0.0000)			(0.0000)
Industrial Exposure			-0.0002		0.0003***
			(0.0002)		(0.0001)
Ind. Empl. p.c. 1925				-0.0089	-0.0243***
				(0.0088)	(0.0041)
Ind. Empl. p.c. 1907				-0.0358**	-0.0491**
				(0.0161)	(0.0212)
Num. Obs.	1402	1754	1755	1690	1350
R^2	0.299	0.606	0.599	0.611	0.332
R^2 Within	0.093	0.056	0.045	0.080	0.136
Std. Errors	District	District	District	District	District
Fixed Effects	✓	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

D Bayesian Estimation of Aggregate Effects

This paper has, so far, made only limited claims about the nature of the aggregate effects of military spending in the Nazi economy. Identifying from such effects is difficult from Nazi-era macrodata, given the low number of observations. Conversely, while my empirical approach identifies local, relative effects, extrapolating aggregate effects from them would suffer from the missing intercept problem (see for instance Moll and Hanney (2025)).

In the absence of a structural model, I use an alternative methodology proposed by Matthes, Nagasaka, and Schwartzman (2025). The authors propose a framework in which aggregate effects can be identified by jointly estimating the local cross-sectional and aggregate time-series effect in a Bayesian model. Their additional identifying assumption is that "the comovement across units in the panel data are well captured by a factor model, with, in the example, one of the factors representing an aggregate government spending shock" (Matthes, Nagasaka, and Schwartzman (2025), p. 3). In my case, this would require that the local Luftwaffe effect is informative enough about the aggregate military spending shock. Then, I can use the local effect to inform the aggregate relationship in a Bayesian model as follows.

Suppose the local OLS estimate is a noisy measurement of the military spending estimand:

$$\hat{\beta} \mid \beta \sim \mathcal{N}(\beta, \widehat{se}^2)$$

Then, I can estimate the aggregate effect from time-series data using a flexible time-series likelihood:

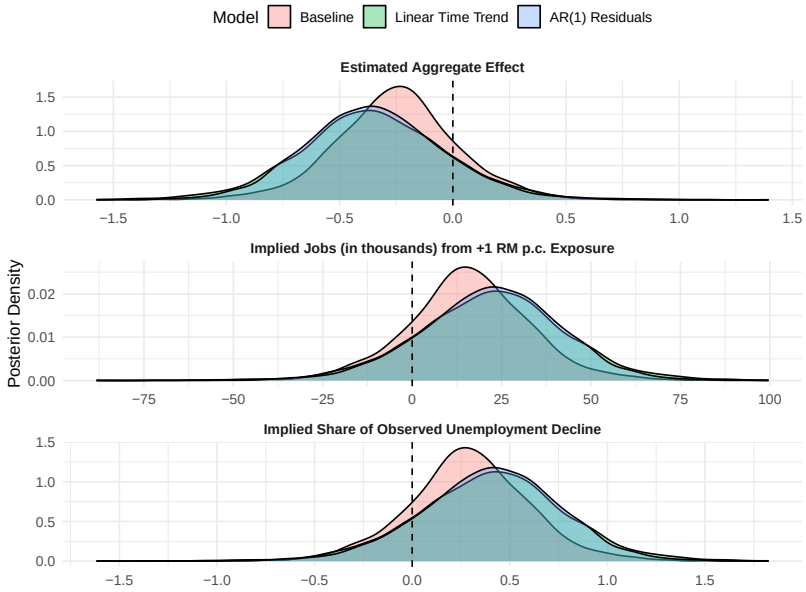
$$\Delta \overline{U}_{t-1932} = \delta \Delta \overline{Exposure}_{t-1932} + e_t$$

where δ is the aggregate effect. The missing intercept is thus $\theta = \delta - \beta$. Standardizing variables allows me to use weakly informative priors for both δ and β.[31]

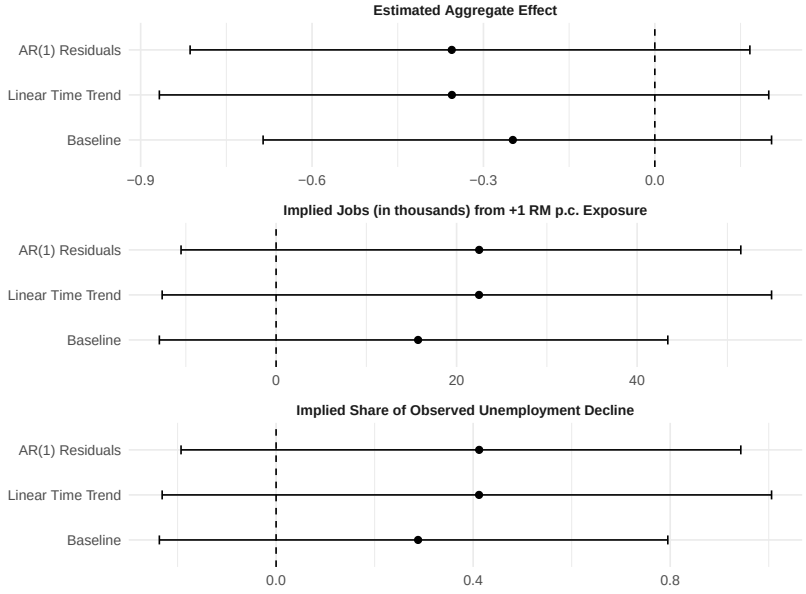
In other words, the Bayesian model estimates the aggregate effect while using by baseline OLS estimator to inform itself on how much of the global variation could be explained by local effects. This remains a challenging exercise, as the number of aggregate time-series observations is low. To address this, I estimate two additional specifications of this

[31] The results do not change when imposing $\mathcal{N}(0,1)$ or $\mathcal{N}(0,2)$ on both parameters.

Bayesian model: first, I include linear time trends to absorb aggregate movements in unemployment not related to military spending. Second, I use AR(1) residuals to allow a persistence of the aggregate shock. The results are reported in Figure A6:



(a) Density Plot



(b) Median Posteriors

Figure A6: Distribution of Posteriors

These results reveal several interesting things. First, due to the low number of aggre-

gate observations, posteriors are widely distributed, and identification is weak. However, when looking at the posterior medians, they suggest sizable aggregate effects, or equivalently, a large missing intercept. Concretely, the estimated aggregate effect, shown in the top-most panel, ranges from about -0.3 to -0.5 - much larger than the local, relative effects identified previously.

The model-implied aggregate job effect extrapolated from the Luftwaffe spending shock is around 10-25 thousand jobs per RM per capita spent. Given that the Luftwaffe budget between 1932 and 1936 was around 80 RM per capita, this amounts to a total job creation of 800 000 up to two million jobs. For comparison, Fremdling and Stäglin (2015) estimate rearmament-induced jobs by 1935 at somewhere between 1.9-2.5 million jobs. This could represent up to half of the observed decline in unemployment in this period.