

A Report on “The Debt-Inflation  
Channel of the German  
(Hyper)Inflation” by Brunnermeier et  
al. (2025)

Reviewer 2

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v1



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I am wiser than this person; for it is likely that neither of us knows anything fine and good, but he thinks he knows something when he does not know it, whereas I, just as I do not know, do not think I know, either. I seem, then, to be wiser than him in this small way, at least: that what I do not know, I do not think I know, either.

Plato, *The Apology of Socrates*, 21d

To err is human. All human knowledge is fallible and therefore uncertain. It follows that we must distinguish sharply between truth and certainty. That to err is human means not only that we must constantly struggle against error, but also that, even when we have taken the greatest care, we cannot be completely certain that we have not made a mistake.

Karl Popper, 'Knowledge and the Shaping of Reality'

## Overview

**Citation:** Brunnermeier, M., Correia, S., Luck, S., Verner, E., and Zimmermann, T. (2025). The Debt-Inflation Channel of the German (Hyper)Inflation. *American Economic Review* 115(7): 2111–2150.

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**Abstract Summary:** This paper investigates how a large increase in the price level is transmitted to the real economy through firm balance sheets, using newly digitized macro- and micro-level data from the German inflation of 1919–1923. The study shows that inflation led to a large reduction in real debt burdens and bankruptcies, and that firms with higher nominal liabilities at the onset of inflation experienced a larger decline in interest expenses, a relative increase in equity values, and higher employment.

**Key Methodology:** Empirical analysis using newly digitized macro- and micro-level data from the German inflation of 1919–1923, employing a difference-in-differences specification to study cross-sectional variation in firm outcomes based on initial leverage.

**Research Question:** How does a large increase in the price level transmit to the real economy through firm balance sheets?

## Summary

### Is It Credible?

This article investigates the economic consequences of the German inflation of 1919–1923, arguing that the episode demonstrates a potent “debt-inflation channel” where unexpected inflation erodes the real value of nominal debt, thereby strengthening firm balance sheets and stimulating real economic activity. Brunnermeier et al. utilize newly digitized firm-level data to advance two primary claims: first, that the aggregate reduction in real debt burdens drove a massive decline in corporate bankruptcies; and second, that firms with higher initial leverage experienced significantly stronger employment growth compared to their less indebted peers. They further contend that this redistribution of wealth from creditors to shareholders was reflected in stock market valuations and operated despite the increasing flexibility of wages and prices.

The analysis provides strong evidence for the real effects of debt erosion on firm survival and employment. The aggregate relationship between rising inflation and falling bankruptcies is striking and theoretically consistent with the mechanism of debt dilution (p. 2125). More importantly, the cross-sectional finding that firms with higher leverage in 1917 expanded employment more aggressively during the subsequent inflation is robust to a battery of controls and alternative specifications (p. 2134). The magnitude of this effect is economically significant, with a one-standard-deviation increase in leverage associated with 7.5 percent higher employment (p. 2135). The authors bolster this claim by showing that the effects were concentrated among firms with long-term fixed-rate debt and smaller firms likely to face financing constraints (p. 2141). The mechanism is further supported by the analysis of book equity, which utilizes a long-difference approach from 1918 to 1924 to bypass the unreliable accounting data of the hyperinflation years,

revealing a substantial increase in real net worth for levered firms (p. 2143).

However, the attribution of employment growth solely to the debt-inflation channel faces a significant challenge regarding pro-cyclicality. High-leverage firms often possess higher market betas and naturally outperform during economic booms. While the authors attempt to rule this out via placebo tests on the 1926–1928 expansion, their own analysis reveals that the interaction between leverage and GDP growth is positive and statistically significant, comparable in magnitude to the interaction with inflation (p. 2137; Supp. App. Table C.3). This suggests that the observed outperformance of levered firms may be partly driven by the general economic expansion of 1919–1922 rather than the inflation shock exclusively. Additionally, the reliance on 1917 leverage as the identifying variation assumes that capital structures during World War I were not determined by factors—such as state-directed war production—that would also influence post-war growth. While instrumental variable strategies using pre-war leverage help mitigate this concern, the unique nature of the pre-treatment period remains a limitation.

The credibility of the findings regarding stock market valuations is considerably weaker than the employment results. For the employment analysis, the authors rigorously fix leverage in 1917 to ensure exogeneity. However, they acknowledge that this clean identification strategy yields statistically insignificant results for stock returns (p. 2144, footnote 36). The analysis then resorts firms annually based on lagged leverage, which yields a significant effect. This introduces endogeneity, as firms increasing leverage during the inflation might differ systematically from those that do not. Consequently, the claim that equity markets priced in the benefits of the debt-inflation channel is not robustly established by the causal framework applied elsewhere in the article. Furthermore, the reliance on accounting ratios for the analysis of interest expenses must be viewed with caution given the authors' admission that financial statements from 1923 were widely considered "unfit for any serious scrutiny" (p. 2140; Supp. App. p. 51).

Overall, Brunnermeier et al. convincingly document that the German inflation caused a massive redistribution of wealth that prevented bankruptcies and relaxed financial constraints for levered firms. The extrapolation that wiping out debt could account for an “18 percent increase in employment” serves as a useful illustration of the channel’s potential magnitude in partial equilibrium, though it abstracts from the aggregate costs borne by savers and the banking sector (p. 2146). The study successfully highlights that financial frictions can transmit inflation to the real economy even when prices and wages are flexible, provided debt contracts remain nominal and rigid.

### **The Bottom Line**

This study provides credible evidence that the German hyperinflation stimulated employment and reduced bankruptcies at levered firms by eroding the real value of their debt. The core finding that firms with higher pre-inflation leverage grew faster is robust, although it is difficult to fully distinguish this effect from the standard tendency of levered firms to outperform during economic booms. The claim that stock market investors rewarded this leverage is less reliable, as it depends on a methodological choice that abandons the strict identification strategy used for the employment results.

## Potential Issues

**Endogenous identification strategy for stock market analysis:** The article's analysis of stock market returns abandons the core identification strategy used for its main employment findings, raising significant concerns about endogeneity. For the employment analysis, the authors use a firm's leverage from 1917 to ensure the measure is exogenous to the subsequent inflation. However, for the stock market analysis, the authors acknowledge in a footnote that this more robust approach yields insignificant results: "The estimates are smaller and usually statistically insignificant when fixing leverage before the inflation (in 1917 or 1918)" (p. 2144, footnote 36). Instead, the analysis resorts firms each year based on their lagged leverage, which yields a significant effect. This time-varying measure is endogenous, as firms that actively increase their leverage during the inflation may possess other characteristics, such as better growth opportunities or political connections, that also drive higher stock returns. The fact that the causally-identified specification shows no effect, while the endogenous specification does, substantially weakens the evidence for the article's fourth contribution regarding stock market valuations.

**Difficulty in disentangling the debt-inflation channel from pro-cyclicality:** A central challenge for the article's identification strategy is separating the effect of the debt-inflation channel from the standard pro-cyclicality of high-leverage firms. The main finding is that high-leverage firms expanded employment more rapidly during the 1919–1922 economic boom. This is observationally consistent with a simple business cycle story where high-beta firms outperform during an expansion. The authors acknowledge this is "an important identification concern" and attempt to address it in two ways (p. 2131). First, they conduct a placebo test showing that high-leverage firms did not grow faster during the non-inflationary boom of 1926–1928 (p. 2137; Supp. App. Figure C.14). Second, they test for an interaction between leverage and GDP growth. However, their own results show that the interaction be-

tween leverage and GDP growth is positive and highly statistically significant, with a magnitude and significance level comparable to the interaction with inflation (Supp. App. Table C.3). This finding confirms that the business cycle confounder is empirically powerful and makes it difficult to attribute the observed employment growth solely to the debt-inflation channel, a limitation the authors recognize by stating that “fully separating the role of inflation and GDP growth is challenging” (p. 2137).

**Potential violation of the parallel trends assumption:** The article’s difference-in-differences design rests on the assumption that high- and low-leverage firms would have followed parallel trends in employment in the absence of the post-war inflation. The validity of this assumption is questionable because the pre-treatment period used to establish the trend (1914–1918) coincides with World War I. The war was a massive economic shock that fundamentally altered firm operations, and a firm’s leverage in 1917 could be correlated with its role in the war economy (e.g., receiving state-directed credit for war production), which in turn could determine its post-war growth trajectory. The authors acknowledge a potential pre-trend in the raw data but argue it is not robust to the inclusion of industry-year fixed effects (p. 2132). While the article presents visual evidence to support this claim (Figure 7, Panel B, p. 2132), the use of such a turbulent pre-period remains a significant challenge to the identification strategy. The authors attempt to mitigate this concern through robustness checks, such as instrumenting 1917 leverage with pre-WWI leverage, but the core assumption remains difficult to verify conclusively (p. 2115; Supp. App. Table C.9).

**Reliability of hyperinflation-era accounting data:** The article’s analysis of the mechanisms driving its main results relies on accounting data from a period of hyperinflation, which introduces concerns about data quality. The authors are transparent about these limitations, citing a contemporary source stating that financial statements were “unfit for any serious scrutiny” and noting that their own data shows a spike in accounting identity violations in 1923 (Supp. App. p. 51; Supp. App. Figure D.4). This is why their main identification strategy prudently relies on pre-

inflation leverage and non-accounting outcomes like employment. However, the analysis of interest expense ratios is still subject to these data quality issues, as the authors' assumption that expense ratios are "less likely to be distorted" is difficult to verify (Table 4, p. 2140; p. 2119, footnote 8). In contrast, the analysis of the change in real book equity is designed to bypass these distortions by calculating the long-difference from 1918 to 1924, when the new and more accurate Goldmark balance sheets became available (Table 5, p. 2143). While this methodological choice for the book equity analysis is commendable, the unreliability of the underlying data for the interest expense analysis limits the confidence that can be placed in those specific quantitative results.

**Construct validity of the primary leverage measure:** The article's primary measure of a firm's exposure to the debt-inflation channel is the ratio of total liabilities to total assets. This broad measure includes liabilities, such as "accrued wages, unpaid taxes, and pensions" and "trade credit," that are short-term in nature and would likely adjust quickly to inflation, thus not fitting the theoretical mechanism which relies on the erosion of fixed, long-term nominal debt (p. 2130). This introduces potential measurement error. The authors acknowledge this limitation and provide robustness checks using a narrower measure of "financial debt to total assets," which yield similar results (Table 2, p. 2134). They further address the issue by conducting a specific analysis showing that the effects are strongest for firms with a higher proportion of long-term debt (Figure 9, p. 2142). While these steps mitigate the concern, the primary specification relies on the broader, less precise measure.

**Limited external validity:** The findings are drawn from an extreme historical episode characterized by hyperinflation and fiscal dominance, where the central bank passively financed government deficits. This context is fundamentally different from modern advanced economies where independent central banks typically respond to rising inflation with monetary tightening. The authors explicitly acknowledge this limitation in their conclusion, stating that in a modern setting,

an aggressive monetary policy response “can offset the expansionary effects from the reduction in real debt burdens” (p. 2146). Therefore, the results may not be generalizable to periods of moderate inflation under a modern monetary policy regime.

**Generalizability of the sample:** The study’s firm-level analysis is based on a sample of approximately 700 large, publicly traded firms listed in *Saling’s Börsen-Jahrbuch* (p. 2118). These firms, with an average of over 4,000 employees, are not representative of the broader German economy, which included a large number of smaller, privately held businesses (Table 1, p. 2119). The authors attempt to address this by showing that the effects are stronger for “smaller firms” within their sample, but this is a standard limitation of historical financial research due to data availability (p. 2142).

**Interpretation of cross-sectional estimates:** The article’s conclusion contains a calculation suggesting that the complete elimination of debt for nonfinancial firms could account for an “18 percent increase in employment” (p. 2146). This figure is derived by extrapolating the cross-sectional regression coefficient to an out-of-sample scenario. The authors appropriately qualify this calculation by stating it is “Abstracting from general equilibrium effects.” However, such a large headline number, derived from a partial equilibrium analysis, risks being interpreted as a precise estimate of the aggregate impact, whereas it is intended to illustrate the potential magnitude of the channel.

**Presentation and transparency issues:** Several minor issues related to presentation and transparency exist in the text and its supplement. First, the text makes a quantitative comparison between “above-median” leverage firms and “lowest tercile” firms, but the referenced figure only shows a median split (p. 2132, Figure 7). Second, a key statistic used to calculate the channel’s explanatory power—the 34.5 percent standard deviation of employment growth—is stated in the text but not documented in any summary table (p. 2135). Third, the industry classification scheme, which is

important for the fixed-effects strategy, is described as “approximately” two-digit SIC without a detailed mapping from the historical source (p. 2129, footnote 20). Fourth, a robustness check on a “balanced panel” is presented without reporting the number of firms in the restricted sample (Supp. App. Figure C.15, p. 26). Finally, the article uses a more aggressive data trimming choice for its interest expense analysis (95th percentile) than for other variables, a decision that is noted but not fully justified against alternatives (Supp. App. p. 57).

## Future Research

**Disentangling pro-cyclicality:** Future work could exploit historical episodes of stagflation where high inflation coincides with low or negative growth. Analyzing the performance of high-leverage firms in such a setting would allow researchers to separate the benefits of debt erosion from the confounding effects of pro-cyclicality and high market beta.

**Improved identification of asset pricing:** To rigorously test whether equity markets price the debt-inflation channel, future research could utilize high-frequency event studies around unexpected inflation news or stabilization announcements. By fixing leverage prior to the shock, this approach would avoid the endogeneity inherent in re-sorting portfolios based on time-varying leverage.

**General equilibrium quantification:** Future research could develop a structural model calibrated to these micro-level estimates to assess the net aggregate effect of the debt-inflation channel. Such a model would need to explicitly account for the losses sustained by the banking sector and household savers to determine whether the employment gains at levered firms outweighed the contractionary effects of wealth destruction elsewhere in the economy.

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