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**September 2024**

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# A comment on “Populist Leaders and the Economy”<sup>\*</sup>

Shih-Hsien Chuang  
Matthew Holian  
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## Abstract

Funke, Schularick, and Trebesch (2023) investigate the impact of populist leaders on GDP growth in 60 countries. They build an original dataset identifying populist presidents and prime ministers from 1900 to 2020. They then examine changes in countries’ GDP growth rates following a populist leader using various empirical methods. They find that 5-15 years after a populist leader, the GDP per capita in that country is lower. Focusing on the panel regression results (Table 2), which we replicate, the authors find a reduction in GDP growth rates of 0.8-1 percentage point per year, with p-values ranging from 0.000 to 0.023. We successfully computationally reproduce these estimates.

Second, we recode the variable identifying populist leaders from the authors’ source and examine the sensitivity of the estimates to changing the sample time period to include the “war” years of 1915-1945. We find that the results in our main change – using the extended time-period sample – are qualitatively similar to the original results, though with smaller and noisier point estimates. Specifically, the 5-year estimate in Table 2 column 3 changes from -0.97 (p-value 0.02) to -0.43 (p-value 0.2), and the 15-year estimate in Table 3 column 3 changes from -0.73 (p-value 0.01) to -0.53 (p-value 0.17). We then turn to sensitivity analysis regarding small differences in research choices about how to code the start of populist spells and which spells to include in the sample. We find the original results are highly robust to these changes. For example, in our Table 3 column 3, the estimated effect changes from the original -0.73 (p-value 0.01) to -0.75 (p-value < 0.01) and -0.78 (p-value < 0.01).

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## 1 Introduction

[Funke et al. \(2023\)](#), henceforth FST, investigates the impact of populist leaders on their countries' economies in the short and medium run. To examine the effect of populists, they first create a new cross-country database of populist presidents and prime ministers from 1900 to 2020. To identify populist leaders, FST uses a “consensus definition of populism” from the academic literature in which populists place the narrative of “people versus the establishment” at the center of their political agenda. With this definition, FST uses a “big literature” approach in which they digitize more than 770 books and articles on populism and rely on this body of work to classify populists.

Having identified populists, FST then examine the impact of populist leaders on economic growth. The authors note that populist leaders are not randomly assigned, and no perfect strategy exists for estimating the causal effect of populist leadership. Because of this, they “use a variety of different empirical strategies that all paint a similar picture: populism has large economic costs.” These methods include two-way fixed effects models, inverse propensity weighted estimators, and synthetic control estimators, with the latter being their “main tool.” They describe the main result on p.3252 as ‘Over 15 years, real GDP per capita is 10 percent lower compared to the non-populist counterfactual, i.e., compared to a synthetic control economy that does not receive a populist “treatment.” ’

In our replication exercise, prepared for the Institute for Replication ([Brodeur et al. 2024](#)), we focus on one of these empirical strategies: a two-way fixed effects ordinary least squares regression from equation (1). We focus on this specification for several reasons. First, it captures the main claim of the paper. They summarize the results of this analysis on p.3267 as “In all specifications, the growth gap [between countries with populist vs. non-populist leaders] amounts to approximately 1 percentage point per year and is highly significant.” Second, unlike the synthetic control estimates, the OLS specification allows for easily computable p-values that are useful for the broader goals of the Replication Games. Third, the simplicity of this empirical approach allows for straightforward robustness checks that we could complete within the one-day Replication Games event. That said, we acknowledge that we are replicating only one of the several empirical strategies that support the

main conclusion of the paper.

In the present report, we investigate whether their analytical results in Table 2 are computationally reproducible and further test their replicability and robustness to: (1) independently coding the main independent variable from the raw data on populist episodes, (2) adding more years to the sample, and (3) examining the sensitivity to small changes in research choices about which populist spells to include in the sample.

In terms of computational reproducibility, we would like to acknowledge that the original study was successfully reproduced by the data editor's team at the American Economic Review. We also successfully reproduced FST's main tables (Table 2) using their codes, with a trivial difference in two of six replicated coefficient estimates (point estimate of -0.96 vs -0.97, as well as -0.72 vs -0.73).

In terms of robustness replication, our main change is to add more years to the sample. In our main change, we examine the sensitivity to extending the sample. The original paper uses data from 1946 to 2019, with the sample for populist spells restricted to the years between 1946 and 2004. We extend this time frame to use data from 1915 to 2019, with the sample for populist spells as those reported in Appendix Table A1, before the year 2014 (Change 1). We find that the results in this extended time-period sample are qualitatively similar to the main results, with smaller and noisier point estimates. This is perhaps due to extending the sample to include the turbulent period of 1915-1946.<sup>1</sup> Specifically, the 5-year estimate in column 3 of Table 2 changes from -0.97 (p-value 0.02) to -0.43 (p-value 0.2), and the 15-year estimate in column 3 of Table 3 changes from -0.73 (p-value 0.01) to -0.53 (p-value 0.17). In summary, the estimates become smaller in magnitude and noisier when extending the sample of populist spells from 1946-2004 to 1915-2014 and the years included in the sample from 1946-2019 to 1915-2019.

We then turn to sensitivity analysis regarding small differences in research choices about how to code the start of populist spells and which spells to include in the sample. We find the original results are highly robust to these changes in both magnitudes and statistical significance.

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<sup>1</sup>We choose 1915 as the starting point to ensure accurate measurement of populist regimes in the 15-year pre-period, because our main specification includes a 5- or 15-year lag of the start of a populist regime.

## 2 Computational Reproducibility

We used the replication package here: [FST Replication Package](#). The cleaning codes and raw data were provided in the replication package. We successfully computationally reproduced the results in Table 2 and Appendix Table B3, and did not attempt a replication of other results. See Table [A1](#) for details.

Table 2 has three columns. We reproduced the coefficients in the first two columns perfectly and found a trivial difference in the coefficients in the third column (which adds in institutional, crises and macroeconomic controls). In particular, we find a coefficient equal to -0.97 in the top panel (5-year aftermath) whereas the reported coefficient was -0.96. The standard errors were the same. In the bottom panel, column three, we find a coefficient of -0.73 whereas the published coefficient was -0.72. The standard errors were the same.<sup>2</sup>

Table B3 has five columns. We reproduced the coefficients in the first four columns perfectly but found a minor difference in the coefficients in the fifth column (which has the same control variables as in column 3, Table 2.) In particular, we find (in the top panel, 5-year differences) a value of -0.971 whereas in the published Online appendix it is -0.963. In the bottom panel (15-year difference) we find -0.726 whereas in the published table it is -0.724. The standard errors are all the same except we find 0.425 in the top panel, column 5, whereas the authors report 0.427. Table B3 column 5 and Table 2 column 3 report on the same model, but there are three digits reported in Table B3 but only two in Table 2.

## 3 Robustness Replication with New Coding and Data

We now turn our attention to our robustness replication. Using the information identifying the country-years with populist leaders reported by FST in Online Appendix Table A1, we independently coded an indicator variable  $Populist_{i,t}$  which equals one if country  $i$  in year  $t$  had a populist leader, using the full sample years of 1900-2019.

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<sup>2</sup>There is a minor typo in the caption to Table 2. It should read, “Column 3 includes country and year fixed effects as well as controls for institutional quality, financial crises, inflation, and trade” instead of “Column 1...”.

Equation (1) in FST, reproduced here, is

$$g_{it} = \beta_P \text{Populist}_{i,t-k} + \delta X_{i,t-1} + \alpha_i + \alpha_t + \epsilon_{it}$$

where  $g_{it}$  is the real annual GDP per capita growth rate and  $\text{Populist}_{i,t-k}$  is the year a populist leader came into power, considering  $k \in \{5, 15\}$  years (e.g., if a populist leader came into power in 1990, the  $k = 5$  indicator would equal one for years 1991-1995).

From our recoding of populist spells, we identify the year a populist came into power as  $\text{NewPop}_{i,t} = \text{Populist}_{i,t} - \text{Populist}_{i,t-1} = 1$ , and then form  $\text{Populist}_{i,t-k} = \mathbf{1}\{\sum_{j=0}^{k-1} \text{NewPop}_{i,t-j} > 0\}$  for  $k = 5, 15$ . There are two minor differences between our coding of the variable  $\text{Populist}_{i,t-k}$  and that of the original paper:

1. Our coding covers all populist spells reported from 1900 to 2014, while the original coding covers spells that between the years 1946 to 2004<sup>3</sup>.
2. Peru in 1990 transitions from a left-wing populist leader to a right-wing populist leader. Our coding, which relies on a change in whether the leader is populist (irrespective of their political position), does not count this as the start of a regime. The original coding in FST does.

We test the robustness of the results to these differences between our independent coding and the original coding of the variable.

### 3.1 Results

To begin, we reproduce the results of FST Table 2 using the authors' choices about sample period, which spells to include, and how to code Peru in 1990. We exactly reproduce the authors results once these changes are made (reported in our Table 2 and Table 3, panel "Original."), except for the trivial differences in column 3 (point estimate of -0.96 vs -0.97, as well as -0.72 vs -0.73).

We consider three different changes (see Table 1). First, in our main change, we examine the sensitivity to extending the sample. The original paper uses data from 1946 to 2019, with the sample for populist spells restricted to years between

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<sup>3</sup>Therefore, if a spell began in 1944, it would not be counted in FST; e.g. Velasco in Ecuador).



1946 and 2004. We extend this time frame to use data from 1915 to 2019, with the sample for populist spells as those reported in Appendix Table A1 before the year 2014 (Change 1). We start the sample in 1915, as opposed to 1900, to allow for accurate measure of populists during the 15-year pre-period that enters through the  $Populist_{i,t-k}$  independent variable. We find that the results in this extended are qualitatively similar to the main results, though with smaller and noisier point estimates. This is likely due to extending the sample to include the especially turbulent period of 1915-1946. Specifically, the 5-year estimate in Table 2 column 3 changes from -0.97 (p-value 0.02) to -0.43 (p-value 0.20), and the 15-year estimate in Table 3 column 3 changes from -0.73 (p-value 0.01) to -0.53 (p-value 0.17).<sup>4</sup>

We next examine two minor differences in our independent coding that were due to differences in subjective choices, not coding errors. In the list of populist spells, there were two consecutive terms of populists in Peru. The authors coded it as a populist taking over<sup>5</sup>. We changed that from 1 to 0 and re-estimate it using the original sample (Change 2). The results are quantitatively comparable to the original results.

Third, in the original sample, the authors include only spells beginning in 1946 or later, using a sample period of 1946-2019. In our independent coding, we would have included spells that were ongoing at the start of the sample. For example, Ecuador has a populist spell beginning in 1944, so in our coding  $Populist_{i,t-5}$  would equal one for the sample years 1945-1949 and  $Populist_{i,t-15}$  would equal one for the sample years 1945-1959, even though the spell began before the start of the sample data which covers 1946-2019. We re-estimate the model on the original sample years of 1946-2019 but including these spells that overlap with the start of the sample period (Change 3). We find that the results are robust. Thus, in summary, we are able to replicate the original results using our own choices about which populist spells to include.

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<sup>4</sup>The results for the 15-year sample are less comparable as our sample extends up to the year 2014.

<sup>5</sup>This was probably due to change in the political regime from left-wing to right-wing, although that is not a consideration in the type of populism otherwise.

## 4 Conclusion

In this report, we attempt to replicate the results in FST Table 2 and consider a few changes to test the sensitivity of the results. We computationally replicate FST Table 2. Additionally, our replications also suggest that results are largely consistent with the main text of the paper and are not sensitive to the small changes and subjective choices made. Future replications could also consider adopting the definition of a populist in other related papers as listed in Online Appendix Table A1, and conduct sensitivity analysis accordingly.

## References

Brodeur *et al.*: 2024, Mass Reproducibility and Replicability: A New Hope, *I4R Discussion Paper Series* .

Funke, M., Schularick, M. and Trebesch, C.: 2023, Populist leaders and the economy, *American Economic Review* **113**(12), 3249–3288.

## 5 Tables

Table 1: Labels and Descriptions of Robustness Checks

<b>Label</b>	<b>Description</b>
Original	We follow the original coding and sample restrictions, which limit the sample to 1946-2019, and restrict the Populist terms for those that begin after 1946 and before 2004. Additionally, include the Populist term for Peru in 1990.
Change 1	Using the information available in FST Online Appendix Table A1, we extend the sample to include all years and populist spells in 1915-2019. Additionally, include the Populist term for Peru in 1990.
Change 2	Peru in 1990 transitions from one populist leader to another populist leader. The original coding of FST (and our coding in Change 1) counts 1990 as the start of a populist regime. In this change, we do not count this event as the start of a populist regime.
Change 3	Using the original sample period of 1946-2019, we include populist regimes even if they began before 1946.

Changes made to variable “Populist leader” and estimation sample in Table 2.

Table 2: Growth Rate—Years after Populists Come to Power versus Normal Years: 5 Year Aftermath

	(1)	(2)	(3)
<i>Original</i>			
Populist leader	-0.97** (0.41) [0.02]	-1.01** (0.41) [0.01]	-0.97** (0.43) [0.02]
Observations	4249	4249	3205
<i>Change 1</i>			
Populist leader	-0.19 (0.34) [0.56]	-0.36 (0.33) [0.27]	-0.43 (0.33) [0.20]
Observations	5743	5743	4342
<i>Change 2</i>			
Populist leader	-1.03** (0.42) [0.01]	-1.19*** (0.42) [0.01]	-1.10** (0.43) [0.01]
Observations	4249	4249	3205
<i>Change 3</i>			
Populist leader	-0.61 (0.41) [0.14]	-0.81** (0.41) [0.05]	-0.79* (0.42) [0.06]
Observations	4249	4249	3205

Standard errors are in parentheses and p-values in square brackets; \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$

*Note:* Original refers to the regressions from Table 2 in the paper. Change 1: the end year of the Populist variable is changed to 2014. Change 2: the change to Fujimori in 1990 is not included. Change 3: Populist regimes are included, even if they began before 1946.

Table 3: Growth Rate—Years after Populists Come to Power versus Normal Years: 15 Year Aftermath

	(1)	(2)	(3)
<i>Original</i>			
Populist leader	-1.04*** (0.22) [0.00]	-0.81*** (0.25) [0.00]	-0.73*** (0.26) [0.01]
Observations	4249	4249	3205
<i>Change 1</i>			
Populist leader	-0.35 (0.26) [0.18]	-0.31 (0.35) [0.37]	-0.53 (0.39) [0.17]
Observations	5743	5743	4342
<i>Change 2</i>			
Populist leader	-1.06*** (0.22) [0.00]	-0.84*** (0.26) [0.00]	-0.75*** (0.27) [0.00]
Observations	4249	4249	3205
<i>Change 3</i>			
Populist leader	-0.87*** (0.23) [0.00]	-0.82*** (0.26) [0.00]	-0.78*** (0.27) [0.00]
Observations	4249	4249	3205

Standard errors are in parentheses and p-values in square brackets; \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$

*Note:* Original refers to the regressions from Table 2 in the paper. Change 1: the end year of the Populist variable is changed to 2014. Change 2: the change to Fujimori in 1990 is not included. Change 3: Populist regimes are included, even if they began before 1946.

## 6 Appendix Tables

Table A1: Replication Package Contents and Reproducibility

<b>Replication Package Item</b>	<b>Fully</b>	<b>Partial</b>	<b>No</b>
Raw data provided	✓		
Cleaning code provided	✓		
Reproducible from raw data	✓		

*Notes:* This table summarizes the replication package contents contained in [Funke et al. \(2023\)](#). We conducted a replication of Table 2 in FST, and did not attempt a full replication of all results.