Political Conflict and Development Dynamics: Economic Legacies of the Cultural Revolution

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As a multi-faceted socio-political movement in twentieth-century China, the Cultural Revolution (1966–1976) witnessed conflict and social upheaval. This paper investigates its economic legacies, exploiting geographic variation in revolutionary intensity, measured by the number of resulting deaths. Using a newly assembled county-level panel dataset over five decades, we find worse-affected areas performed slightly better at baseline, but were slower to industrialize. This effect was large in the early 1980s before diminishing to become insignificant by 2000. Using individual-level census data, we find more-exposed cohorts are less likely to obtain higher education degrees and to work in professional and entrepreneurial occupations.

The Great Proletarian Cultural Revolution (1966–76) was a multi-faceted socio-political movement in twentieth-century Chinese history that has been widely thought to have significant consequences for subsequent development (Esherick, Pickowicz, and Walder 2006). Despite its historical salience, systematic empirical evidence for its impacts on economic development trajectories of different regions remains scarce. This paper tries to fill this gap in the literature by using panel data spanning five decades for regions and cohorts to examine its dynamic effects.

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The revolution had a particular focus on culture and ideology in the beginning and then witnessed nationwide conflict and social upheaval (MacFarquhar and Schoenhals 2006; Wang 2006). Individuals with "bad class backgrounds" (e.g., former landlords and rich peasants) were perceived as "class enemies" and therefore particularly at risk (Su 2011). In official accounts, following reflections initiated by the government since 1978, tens of millions of persecuted individuals have been rehabilitated, and some responsible for criminal acts have been arrested (Ma 1991). With information collected from the local gazetteers, Walder (2014) estimates the number of revolution-related deaths to lie between 1.1 and 1.6 million during 1966–71. Much of the early historical literature has focused on prominent conflict episodes in major cities, although more recent work has shown that the revolution was also intense in more sub-urban and rural regions, where the majority of the population lived (Su 2011; Walder 2014).

To explore the revolution's economic legacies, we focus on post-1976 industrialization across regions, together with human capital and labor market outcomes across cohorts. We exploit county-level variation in the extent of violence, as measured by the number of revolution-related deaths (Walder 2014). We combine this with economic outcomes from five waves of census data (in 1953, 1964, 1982, 1990, and 2000) to measure the rate of industrialization as captured by the fraction of population working in non-agricultural sectors. Furthermore, we use the 2000 census micro-data to investigate individual-level impacts on human capital and labor market outcomes.

A key challenge to estimating the impact of revolutionary intensity on economic outcomes is the presence of unobserved heterogeneity since the distribution of conflict is non-random. For instance, we may be concerned that more revolutionary regions were more likely to have a prior history of conflict and were worse off economically to begin with, leading OLS to overestimate the true effects. On the other hand, it could be that areas that were more "capitalist" suffered disproportionately due to the presence of "bourgeois elements." If so, OLS would underestimate the true impacts. Data for a large sample of counties show the level of baseline industrialization (in 1964) is positively correlated with revolutionary intensity, a result in line with the latter scenario.

Our empirical strategy employs both regional and temporal variation, first at the county level and then at the individual level. A key advantage of this approach is the ability to control for all time-invariant county characteristics, region-invariant year/cohort effects, as well as region-specific time/cohort trends. Furthermore, such a strategy allows us to test whether there were any pre-existing differences in industrialization at the county level, as well as any educational or labor market impacts on older cohorts at the individual level.

Given this strategy, one may be concerned that high- and low-intensity counties do not share the same development trajectory due to unobserved heterogeneity, potentially biasing our estimates. To address this, we first collected an additional round of pre-revolution data on industrialization in 1953 for 600 counties from local gazetteers. Using this data, we find no pre-existing differential trends in industrialization before 1966. In addition, our regression specification includes further controls, such as differential province time trends, dynamic effects of a rich set of pre-revolution county characteristics (e.g., inequality, ethnic fragmentation, social capital), as well as geographic variables. Our findings show that worse-affected regions were slower to industrialize. In terms of magnitude, a 1 percent increase in revolutionary intensity translates into a 0.82 percent decrease in industrialization in 1982, and the effect dissipates through time and disappears by 2000. These results are further corroborated by analysis using a larger sample of 1486 counties, for which we have one round of pre-revolution data on industrialization (in 1964). Overall, the estimates indicate an initially large, but ultimately transient impact. These results are robust to controlling for potential reporting bias, famine experience, and using alternative measures of revolutionary intensity.

Besides investigating the revolution's effects on regional development, we also explore individual-level impacts. Here we focus on human capital and labor market outcomes, using the 2000 census micro-data and prefecture-level variation in revolutionary intensity. The estimates are close to zero for cohorts born between 1936 and 1946, whose education should have been completed by the onset of the revolution. In contrast, cohorts born between 1951–1965 in worse-affected areas are less likely to obtain higher education degrees. In terms of magnitude, compared with cohorts born during 1931–1935, an increase in revolutionary intensity from the 25th to the 75th percentile is associated with a 6.9 percent decrease in the odds of being a college graduate for cohorts born during 1956–1960. For cohorts born in the 1970s who would have started primary school after the end of the revolution, revolutionary intensity plays a much less significant role, suggesting an absence of long-term effects for cohorts who did not experience the revolution directly.

In terms of labor market outcomes, we find that cohorts born between the mid-1950s to mid-1960s in worse-affected areas are less likely to be in a professional occupation in 2000. These include scientific researchers, engineers, technicians, economic/finance/legal professionals, as well as teachers and professors. Compared with cohorts born during 1931–35, an increase in revolutionary intensity from the 25th to the 75th percentile is associated with a 4.6 percent decrease in the odds of working in a professional occupation for cohorts born during 1956–60. Such an increase in revolutionary intensity is also associated with a 10.4 percent decrease in the odds of working as an entrepreneur. These results show that more exposed cohorts from worse-affected regions are less likely to work in occupations that were targeted during the revolution.

Taken together, our results show that the revolution affected regional development, such that more exposed regions were slower to industrialize in the early 1980s. This effect dissipates over time and disappears by 2000. Using individual-level data, we find evidence for a deterioration in upper-tail human capital as well as reduced participation in professional and entrepreneurial occupations. These effects are most pronounced for those born in the 1950s and early 1960s, who were of schooling age during the revolution, and are absent for older and younger cohorts.

This paper is related to three strands of literature. First, numerous studies have examined the relationship between conflict and development, as surveyed in Blattman and Miguel (2010). Within these, Abadie and Gardeazabal (2003) look at the economic cost of conflict in the Basque Country; Acemoglu, Hassan, and Robinson (2011) examine the economic impact of the Holocaust in post-Soviet Russia; and Dell (2012) focuses on the Mexican Revolution. In the literature on economic dynamics after major shocks, our results are consistent with those of Cerra and Saxena (2008), who document macro-level rebounds in output after civil wars using cross-country data. Our paper adds to this literature by examining the dynamic effects of a countrywide shock in twentieth-century China using regional and temporal variation.

Second, this paper relates to the literature on the persistent effects of major historical shocks on development, such as the legacies of the Mita system (Dell 2010), the African slave trade (Nunn 2008), and the Scramble of Africa (Michalopoulos and Papaioannou 2016). Compared to these works, our paper studies a relatively recent event and finds more transient effects. By carrying out the analysis both at the region and the individual level, we can separately estimate the effects for various cohorts, differentiating between those who were directly exposed and those who were not. Doing so also allows us to study potential mechanisms in more detail.

Third, our paper also contributes to the growing economic literature studying the Cultural Revolution. There exists a rich body of work in political science, sociology, literature, anthropology, and history (Wu 2016; Yan 2016). Most of these have focused on political struggles as well as ideology, mass participation, and the political consequences of this period. Quantitative assessments of the revolution are still relatively scarce. Existing studies have focused on education and labor market outcomes (Meng and Gregory 2002, 2007; Giles, Park, and Wang 2019; Chen et al. 2020; Roland and Yang 2017; Li and Meng 2022), while other scholarly works have investigated the impacts of the send-down movement (Li, Rosenzweig, and Zhang 2010; Chen et al. 2020). Compared to these studies, our paper is the first systematic attempt to examine its macroeconomic consequences, exploiting regional variation in intensity, using panel data that captures both pre- and post-revolution outcomes at the county and individual level.

HISTORICAL BACKGROUND

The Great Proletarian Cultural Revolution was a multi-faceted socio-political movement during 1966–1976, with a particular focus on culture and ideology from the beginning. The campaign was officially launched after the "May 16 Notification" in 1966, which summarized its ideological guidelines as follows:

> *"The bourgeoisie is trying to use the old ideas, culture, customs, and habits of the exploiting classes to corrupt the masses, capture their minds and stage a come-back. The proletariat must do the opposite.... At present, our objective is to struggle against those persons in authority who are taking the capitalist road; to criticize and repudiate the reactionary bourgeois academics and the ideology of the bourgeoisie; to transform education, literature, art, and all other parts of the superstructure, so as to facilitate the consolidation and development of the socialist system."*

On 8 August 1966, a set of guidelines known as the Sixteen Points were issued by the 11th Plenum of the Party Central Committee, further outlining the nature, objectives, and targets of the revolution. Here we briefly discuss its education, conflict, and economic dimensions.

THE EDUCATION DIMENSION

The education system played a crucial role in the revolution. It was a key arena of "class struggle" and experienced significant disruptions (Unger 1982; Cai and Du 2003; Meng and Gregory 2002; Giles, Park, and Wang 2019). Most universities were closed throughout the period, while high schools were suspended between 1966 and 1968. The latter subsequently experienced a period of low-quality expansion in rural areas (Guo, Wang, and Han 2004). The lengths of primary, middle, and high schools were reduced by one year each, from a combined total of 12 to 9 years. There were also major changes to the curriculum, and schooling during this period was augmented by physical labor, working in factories, farms, and the military.

Another important change was the assessment criteria used in schools. While students were previously judged on academic merit, their class background and record of political activism became much more important for accessing benefits, such as educational opportunities and membership in the Communist Youth League (Rosen 1982). In terms of active participation, students were encouraged to organize themselves into "Red Guard" groups. These activities sometimes degenerated into factional conflict motivated by different factional interests. Frequent armed battles between rival groups contributed to the overall death toll (Lee 1980).

THE CONFLICT DIMENSION

A key feature of this period was that class struggle served as a guiding principle. The "class enemies" often included ex-landlords, rich peasants, intellectuals, among others. In addition, those who were suspected of being associated with the *Kuomintang*, having foreign acquaintances, or having ties with associations perceived to be disloyal were also legitimate targets. Given that political loyalty and support for the revolution were difficult to verify, the process of identifying "class enemies" often resulted in victimization.

Another contributing factor to the chaos was factionalization (into conservative and radical groups), where students, local cadres, workers, and sometimes the military participated in armed fighting. In this chaos, the dysfunction of public security and jurisdiction systems lasted for years and generated a *de facto* state of semi-anarchy with room for local interpretation (MacFarquhar and Schoenhals 2006).

THE ECONOMIC DIMENSION

Beyond social unrest and political upheaval, the revolution also impacted the economy. Shortly after it began, the value of total national production declined by 9.6 percent in 1967. This continued steadily in 1968—in southwest China, output was down by 41 percent; in Yunnan, output of state-owned industries dropped by almost two-thirds.

The tension between participating in revolution on the one hand, and engaging in production on the other, was a reoccurring theme throughout this period. Recognizing this tension, central government guidelines sought to emphasize that the revolution should be carried out while maintaining production in both urban and rural areas. For instance, the "Sixteen Articles" (August 1966) listed "Enhance Revolution, Boost Production" as one of its key items and claimed that "*... mass mobilization should ensure both the Revolution and economic production at the same time.*"

THE POST-REVOLUTION PERIOD

With changes in the political arena, the revolution ended in 1976. An official evaluation of the period came soon after, in 1981, and the local governments and courts duly started a systematic reconciliation process by reviewing cases of persecution during the years. According to official records, tens of millions of victims were rehabilitated, while those who committed crimes were arrested and sentenced (Ma 1991).

To sum up, the Cultural Revolution had a profound impact on Chinese society throughout its ten years. While these contemporary effects have been well documented, the extent to which they may have shaped the subsequent development of different regions and cohorts has not been systematically explored. The remainder of this paper presents the analysis.

DATA SOURCES

Our analyses make use of several datasets, including: (i) revolutionary intensity at the county level, proxied by the number of deaths, as introduced and documented by Walder (2014); (ii) industrialization at the county level, measured by the fraction of non-agricultural workers, compiled from population censuses and county gazetteers; and (iii) educational attainment, employment, and occupation at the individual level, from the 2000 census. The rest of this section describes each of these in more detail.

Revolutionary Intensity

Previous work has shown considerable geographic variation in revolutionary intensity (Walder and Su 2003; Su 2011). In this paper, we use a newly available county-level dataset on the number of revolution-related deaths, digitized from local gazetteers (Walder 2014). To the best of our knowledge, this is currently the only comprehensive dataset available below the province level where relevant information has been assembled for different localities. The coding involved systematic cross-checks by two independent teams, and the process took historical boundary changes into consideration for consistency. We carried out our own check of the data by collecting the same variable for a nationally representative sample of 105 counties and found a high degree of consistency, with a correlation coefficient of 0.90 between the two series.

Often considered as local encyclopedias, gazetteers are book-length volumes covering the geography, demography, economy, governance, education, and culture of a given region (Wilkinson 2015). Gazetteers covering our study period were mostly published in the 1980s and 1990s. Within these, relevant records were located in either: (1) a chronicle of major county events; or (2) a special section on the Cultural Revolution. The number of deaths is defined as those deaths that are related to the revolution, including suicides of individuals under persecution, deaths in clashes between rival factions or with military forces, deaths in struggle sessions or as a result of imprisonment or torture, and executions during political campaigns.

Figure 1 illustrates the regional variation in revolutionary intensity for our main analytical sample of 600 counties. Online Appendix Figure A2 shows it for a larger sample of 1,486 counties, for which we have one round of baseline data. The mean number of deaths is 85.8 in each county, with a standard deviation of 260. Broadly speaking, the southern and northeastern provinces appear to have experienced a higher degree of violence than the central and eastern regions.

Regional Economic Variables

Our main county-level outcome is the extent of industrialization, measured by the fraction of local population working in non-agricultural sectors. According to official definitions (National Bureau of Statistics of China 1997), the non-agricultural population includes "*those who are predominantly working in the secondary or tertiary sectors on a regular basis plus their dependents in both urban and rural areas.*" In line with other work in the macro/development literature (Herrendorf, Rogerson, and Valentinyi 2014), this measure of industrialization aims to capture the broader process of structural change and economic development rather than the growth of the manufacturing sector alone.

Data for a large sample of regions are available from multiple census waves, covering 1964, 1982, 1990, and 2000, respectively. The data for

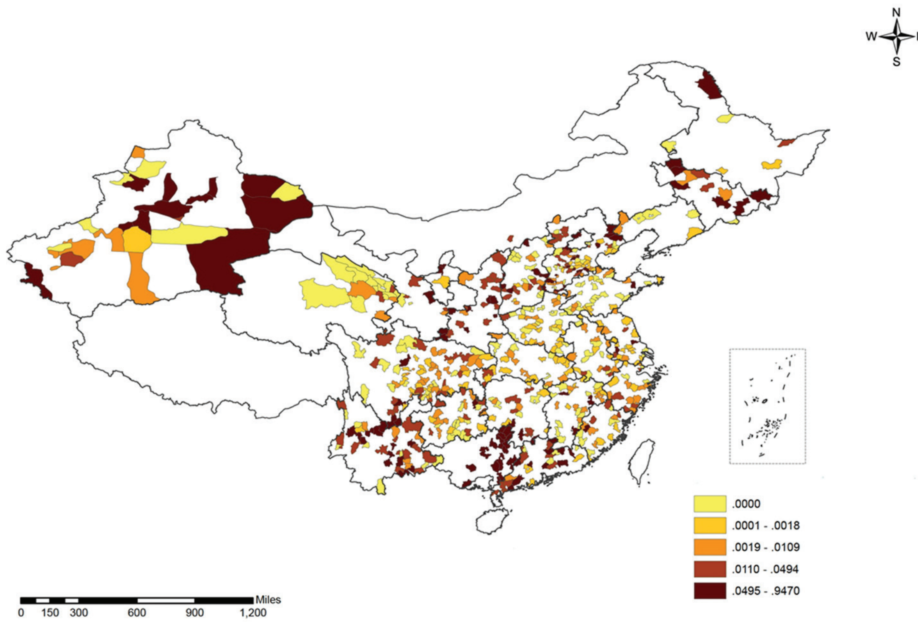

FIGURE 1
REGIONAL DISTRIBUTION OF REVOLUTIONARY INTENSITY (QUINTILES)

Notes: This figure maps revolutionary intensity at the county level, as proxied by revolution-related deaths as a proportion of 1964 population, formally Log(1 + deaths/population* 100). Cutoff thresholds are quintiles. Shaded counties correspond to our 1953-base year sample (N = 600). Unshaded counties are excluded either because of missing values in revolutionary intensity or industrialization in one or more sample years (1953, 1964, 1982, 1990, 2000).
Source: Walder (2014). For the distribution of revolutionary intensity for our 1964-base year sample (N = 1486), please refer to Online Appendix Figure A2.

1953 (the year of the first population census since the founding of the PRC) were collected by the authors from county gazetteers, to have a sample with two rounds of pre-revolution outcomes.[1] In Table 1, we show that the two samples are similar in terms of their mean observable characteristics. In Online Appendix Figure A3, we further show that the distributions of both the explanatory and outcome variables are statistically indistinguishable from each other. With a panel dataset on industrialization for the sample of 600 counties between 1953 and 2000, we have two observed periods before the revolution, which facilitates the empirical analysis.

We focus on industrialization due to both theoretical and practical considerations. First, the process of industrialization has played a major role in raising per capita income as well as government revenue in post-1976 China (Jin and Qian 1998). Moreover, it correlates strongly with

[1] For an image of the gazetteers used, see Online Appendix Figure A1.

TABLE 1
SUMMARY STATISTICS: COUNTY-LEVEL ANALYSIS

	A. 1953 Base-Year Sample N = 600				B. 1964 Base-Year Sample N = 1486					T-test
Variable Name	Mean	S.D.	Min	Max	Mean	S.D.	Min	Max	Sources	p-value
Panel A. Cultural Revolution Variables										
Number of deaths	85.8	259.6	0	2600	75.0	241.5	0	4519	A	0.37
Log proportion of deaths	0.03	0.09	0	0.95	0.03	0.07	0	0.95	A	0.31
Panel B. Economic Outcomes										
Percent of non-agri population, 1953	6.81	6.57	0.08	70.0	n/a	n/a	n/a	n/a	B	n/a
Percent of non-agri population, 1964	7.55	7.01	1.46	79.8	8.29	7.93	1.20	79.8	C	0.05
Percent of non-agri population, 1982	8.47	7.38	0.80	43.1	9.00	7.75	0.40	65.0	C	0.15
Percent of non-agri population, 1990	19.2	11.9	4.84	70.4	19.69	12.01	2.89	86.0	C	0.36
Percent of non-agri population, 2000	27.8	13.5	0	73.6	26.68	13.23	0	100	C	0.08
Panel C. Other County Characteristics										
Population, 1964 (10,000 persons)	28.7	20.8	0.72	111.3	29.09	20.81	0.39	111.3	C	0.73
Pre-revolution Gini coefficient	0.61	0.09	0.32	0.82	0.61	0.09	0.32	0.82	D	0.95
Pre-revolution ethnic fragmentation index	0.13	0.21	0.00	0.75	0.13	0.21	0.00	0.76	D	0.78
Pre-revolution charity organizations	2.05	6.71	0	107.4	2.16	6.98	0	107.4	E	0.75
Log distance to province border	10.6	0.91	7.09	13.1	10.63	0.90	7.10	13.1	F	0.76
Log distance to provincial capital	5.18	0.67	2.40	7.12	5.19	0.69	2.38	7.12	F	0.88
Panel D. Gazetteer Information										
Publication date	1994	3.66	1980	2009	1994	3.97	1980	2009	A	0.69
Word count in Chronicle	7.74	0.67	0	9.36	7.73	0.75	0	9.63	A	0.73
Word count in GPCR section	4.75	3.67	0	10.1	4.73	3.68	0	10.44	A	0.92
Post-Deng period (Pub. >1997)	0.21	0.40	0	1	0.21	0.41	0	1	A	0.72

Notes: This table reports summary statistics for the key variables in our analytical samples at the county level.
Sources: A: Walder (2014); B: county gazetteers; C: Historical China County Population Census Data with GIS Maps (1953–2000); D: China Population Census 1982 (IPUMS); E: China Historical Charity Organizations (Wang 2013) and ArcGIS calculation for intertemporal regional matching; F: Authors' calculation based on ArcGIS. Great circle distance is calculated.

measures of GDP.[2] Crucially, as output data at the county level is not available for the pre-revolution period, focusing on industrialization enables us to carry out placebo tests of pre-trends. Among the counties in our sample, which excludes districts of prefecture-level cities, the average fractions of non-agricultural population in 1953 and 1964 were 6.8 percent and 7.6 percent, respectively. This number increases rapidly over time, reaching 8.5 percent in 1982, 19.2 percent in 1990, and 27.8 percent in 2000.

We also make use of data on other county characteristics, including geography (average terrain slope, distance to provincial capital, distance to provincial border), as well as pre-revolution measures of inequality, ethnic fragmentation, and social capital. Inequality is measured by the educational Gini coefficient among individuals born before 1966 using the 1982 census, and ethnic fragmentation is calculated as in Alesina et al. (2003) for the same sample. Finally, pre-revolution social capital is proxied by the number of charitable organizations during the Ming and Qing dynasties, as in Wang (2013).

Individual-Level Data

To complement our county-level analysis and to investigate potential mechanisms, we also utilize individual-level measures of human capital and labor market outcomes.

For human capital, we use both years of schooling and college graduation status. These are constructed from self-reported educational attainment levels in the 2000 census (the 1 percent sample from IPUMS). In particular, graduates of primary, junior middle, senior middle schools, and college are assigned 6, 9, 12, and 16 years of schooling, while those of junior and senior technical schools are assigned 12 and 14 years, respectively. Since this data only contain prefecture rather than county identifiers, we aggregate the revolutionary intensity variable for this part of the analysis, covering 323 prefectures in total. For labor market outcomes, we examine: (i) employment status, (ii) number of days worked during the past week, and (iii) occupation. Here we focus on occupations that were most targeted, namely professionals and entrepreneurs. For the former, we use a dummy variable to indicate working as a scientific researcher, engineer, technician, economic/finance/legal professional, as well as teacher and professor. For the latter, we employ a dummy variable to indicate working as someone responsible for a business.

[2] The correlation coefficient between these two measures was 0.565 in 1982 and 0.492 in 2000.

EFFECTS ON INDUSTRIALIZATION

Variation in Revolutionary Intensity

Before discussing the main results, we examine whether a broad set of regional characteristics are correlated with revolutionary intensity in the cross section. Table 2 reports OLS results for our two samples, respectively: (1) 1953 base-year sample (600 counties) and (2) 1964 base-year sample (1486 counties). For sample (1), since we have two rounds of pre-revolution data, we can test for selection both in levels (using industrialization in 1953) and in growth trajectories (using change in industrialization between 1953 and 1964). Columns (1)–(3) progressively add geographic characteristics and pre-revolution socio-economic variables (inequality, ethnic fragmentation, and social capital). Most importantly for our analysis, revolutionary intensity is weakly *positively* correlated with both the level of industrialization in 1953 and its growth between 1953 and 1964. In Columns (5)–(7), we conduct a parallel analysis using the larger 1964 base-year sample. Here we can only test for selection in levels, using industrialization in 1964. The results confirm the presence of *positive* selection. By comparison, most other measures of pre-revolution socio-economic characteristics are uncorrelated with revolutionary intensity.

Among the set of geographic characteristics examined, regions with a higher average terrain slope appear to have experienced the revolution more intensely. One conjecture could be that higher-gradient land is on average more marginal and less productive agriculturally. While the theoretical relationship between economic productivity and conflict propensity is ambiguous (Dube and Vargas 2013), this result seems to be consistent with the existing empirical literature, which tends to find a negative relationship between the two (Blattman and Miguel 2010).

Identification Strategy

Analyzing the revolution's dynamic impacts on economic development has several challenges. Most important is the likely endogeneity of revolutionary intensity. Here, the bias can go in either direction. On the one hand, more revolutionary regions could be more likely to have a prior history of conflict and be worse off economically to begin with, leading OLS to overestimate the true effects. Alternatively, worse-affected regions could have had more "bourgeois elements" (such as

TABLE 2
DETERMINANTS OF REVOLUTIONARY INTENSITY

	A. 1953 Base-year Sample (N=600)				B. 1964 Base-year Sample (N=1486)			
Dep. Var: Log Deaths (% of County Pop.)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Industrialization 1953	0.003	0.003	0.004	0.005				
	(0.004)	(0.004)	(0.005)	(0.005)				
Industrialization 1964–1953	0.006	0.005	0.006	0.006				
	(0.005)	(0.005)	(0.005)	(0.005)				
Industrialization 1964					0.008*	0.007*	0.008*	0.008*
					(0.004)	(0.004)	(0.004)	(0.004)
Average terrain slope	0.004**	0.004**	0.004**	0.003**	0.002**	0.002**	0.002**	0.002**
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Distance to provincial border	–0.004	–0.004	–0.003	–0.004*	–0.002	–0.002	–0.002	–0.002
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Distance to provincial capital	–0.003	–0.004	–0.004	–0.004	0.002	0.002	0.002	0.002
	(0.004)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Pre-CR educational Gini coefficient		0.026	0.025	0.019		0.007	0.006	0.007
		(0.036)	(0.035)	(0.033)		(0.020)	(0.019)	(0.018)
Pre-CR ethnic fragmentation index		0.002	0.002	0.002*		0.001	0.001	0.001
		(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)
Pre-CR social capital		0.001	0.003	0.001		–0.001	–0.000	–0.002
		(0.003)	(0.003)	(0.003)		(0.002)	(0.002)	(0.002)
Publication during post-Deng era			0.018	0.011			0.008	0.005
			(0.012)	(0.011)			(0.007)	(0.006)
Word count in chronicle				0.007*				0.001
				(0.004)				(0.002)
Word count in GPCR section				0.003***				0.003***
				(0.001)				(0.001)
R-squared	0.569	0.571	0.577	0.595	0.398	0.399	0.401	0.420
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* = Significant at the 10 percent level.
** = Significant at the 5 percent level.
*** = Significant at the 1 percent level.
Notes: Average terrain slope, pre-revolution educational Gini coefficient, ethnic fragmentation index, social capital, distances to province border and distance to provincial capital are in logarithm forms. Pre-revolution inequality is measured by the educational Gini coefficient, which is calculated using the 1982 census among individuals born before 1966.
Sources: Pre-revolution ethnic fragmentation index is calculated following Alesina et al. (2003). Pre-revolution social capital is proxied with the number of historical charity organizations (Wang 2013).

entrepreneurs and intellectuals) and thereby greater growth potential, leading OLS to underestimate the true effects. The results from Table 2 are consistent with this latter scenario.

Our empirical strategy uses both regional and temporal variation. The key advantage of such an approach is the ability to control for both time-invariant county characteristics and region-invariant year effects. Our baseline regression equation is as follows:

$$Industrialization_{c,t} = \alpha + \Sigma_t \beta_t Intensity_c \times Year_t + \gamma' X_{c,t} + \theta_t + \mu_c + \epsilon_{c,t}, \quad (1)$$

where $Intensity_c$ is measured by the number of revolution-related deaths in county c as a fraction of its 1964 population (in logs), $X_{c,t}$ is a vector of potentially time-varying county characteristics, μ_c represents county fixed effects, and θ_t year fixed effects. The dependent variable is the industrialization of county c in year t, measured by the proportion of non-agricultural population (in logs). The key coefficients of interest are the β_ts, which identify the effects of revolutionary intensity on economic outcomes in each year. For the 1953 base-year sample, we can estimate four interaction terms between the intensity variable and year indicators for 1964, 1982, 1990, and 2000, respectively. For the 1964 base-year sample, we report the interactions for 1982, 1990, and 2000.

Given such a strategy, we test for the presence of pre-trends between high- and low-intensity regions using the 1953 base-year sample to see whether they had similar development trajectories before the revolution. At the county level, we have two periods of baseline data, from the censuses of 1953 and 1964. At the individual level, we take advantage of cohort differences and test for pre-trends in educational attainment and labor market outcomes. Here, the older cohorts should be less exposed to the revolution, at least those who had completed their schooling before 1966. For replication files, please see Bai and Wu (2023).

Results

Before discussing the estimated coefficients, we first provide some descriptive results. As a visual test of potential pre-trends at the county level, Figure 2 illustrates the change in industrialization between 1953 and 1964. Compared to the pattern in Figure 1, where northeastern, northwestern, and southwestern counties experienced the revolution more intensely, the pre-revolution economic trajectories do not reveal any systematic patterns. Figure 3 maps the change in industrialization

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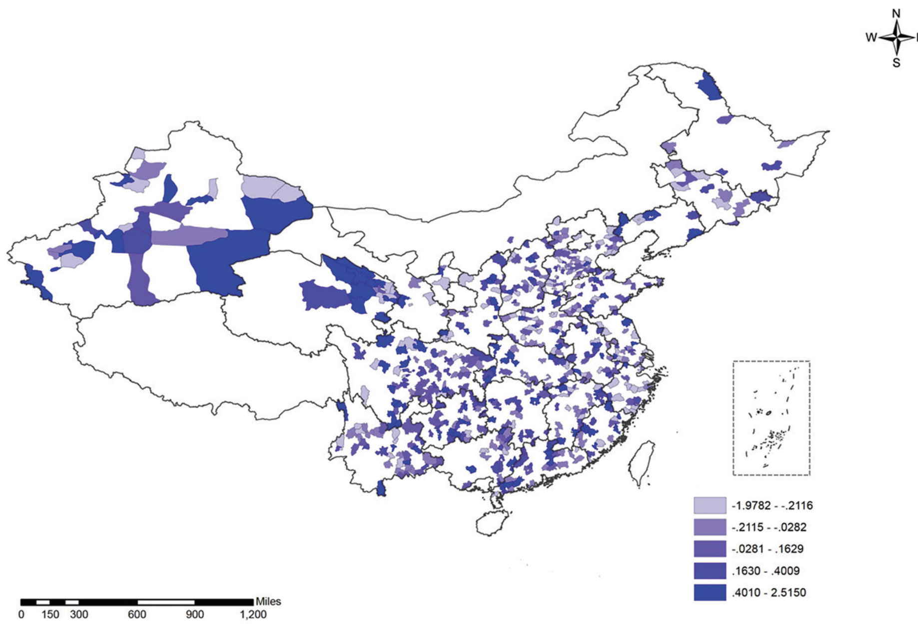

FIGURE 2
REGIONAL DISTRIBUTION OF CHANGE IN INDUSTRIALIZATION RATES 1953–1964 (QUINTILES)

Notes: This figure maps industrialization at the county level, as proxied by the change in share of local population working in non-agricultural sectors between 1953 and 1964, formally Log(Industrialization1964) − Log(Industrialization1953). Cutoff thresholds are quintiles. Shaded counties correspond to our 1953-base year sample (N = 600). Unshaded counties are excluded either because of missing values in revolutionary intensity or industrialization in one or more sample years (1953, 1964, 1982, 1990, 2000).
Source: Population census and county gazetteers.

between 1964 and 1982, the two sample years closest to the revolution on either side. Here, a couple of regions are note-worthy. First, the Shandong peninsula in the east has become relatively more industrialized. Second, the southwestern provinces of Yunnan, Guizhou, and Guangxi have moved in the opposite direction. Looking back at Figure 1, the former region experienced relatively little violence, while the latter suffered much more heavily. To illustrate this pattern more systematically, Figure 4 plots the distribution of industrialization rates in each sample year separately for counties in the top and bottom quartiles of our intensity measure. In both 1953 and 1964, the kernel densities of counties with high and low revolution intensities are almost identical. A Kolmogorov-Smirnov test for equality, while insignificant, shows that worse-affected counties had slightly higher rates of industrialization in

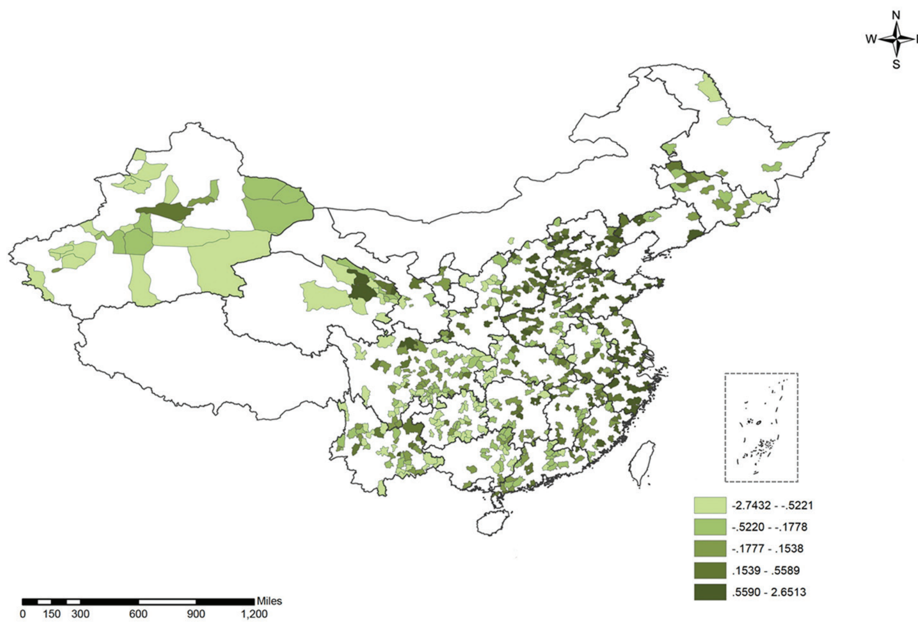

FIGURE 3
REGIONAL DISTRIBUTION OF CHANGE IN INDUSTRIALIZATION RATES 1964–1982 (QUINTILES)

Notes: This figure maps industrialization at the county level, as proxied by the change in share of local population working in non-agricultural sectors between 1964 and 1982, formally Log(Industrialization1982) − Log(Industrialization1964). Cutoff thresholds are quintiles. Shaded counties correspond to our 1953-base year sample (N = 600). Unshaded counties are excluded either because of missing values in revolutionary intensity or industrialization in one or more sample years (1953, 1964, 1982, 1990, 2000).
Source: Population census and county gazetteers. For the distribution of change in industrialization rates from 1964 to 1982 for our 1964-base year sample (N = 1486), please refer to Online Appendix Figure A4.

1953 and 1964, consistent with the results in Table 2. Over time, however, the bottom-quartile distribution, corresponding to the least-affected counties, clearly shifts more to the right compared to the top-quartile distribution.

Building on these descriptive results, Table 3 presents our regression estimates according to Equation (1). Panel A corresponds to the 1953 base-year sample, and Panel B is the 1964 base-year sample. All specifications include county and year fixed effects. Robust standard errors are clustered at the county level, and each observation is weighted using its 1964 population. In the first row of Panel A, the interaction term between revolutionary intensity and the 1964 year dummy is consistently insignificant. Moreover, the coefficients are close to zero, indicating the absence

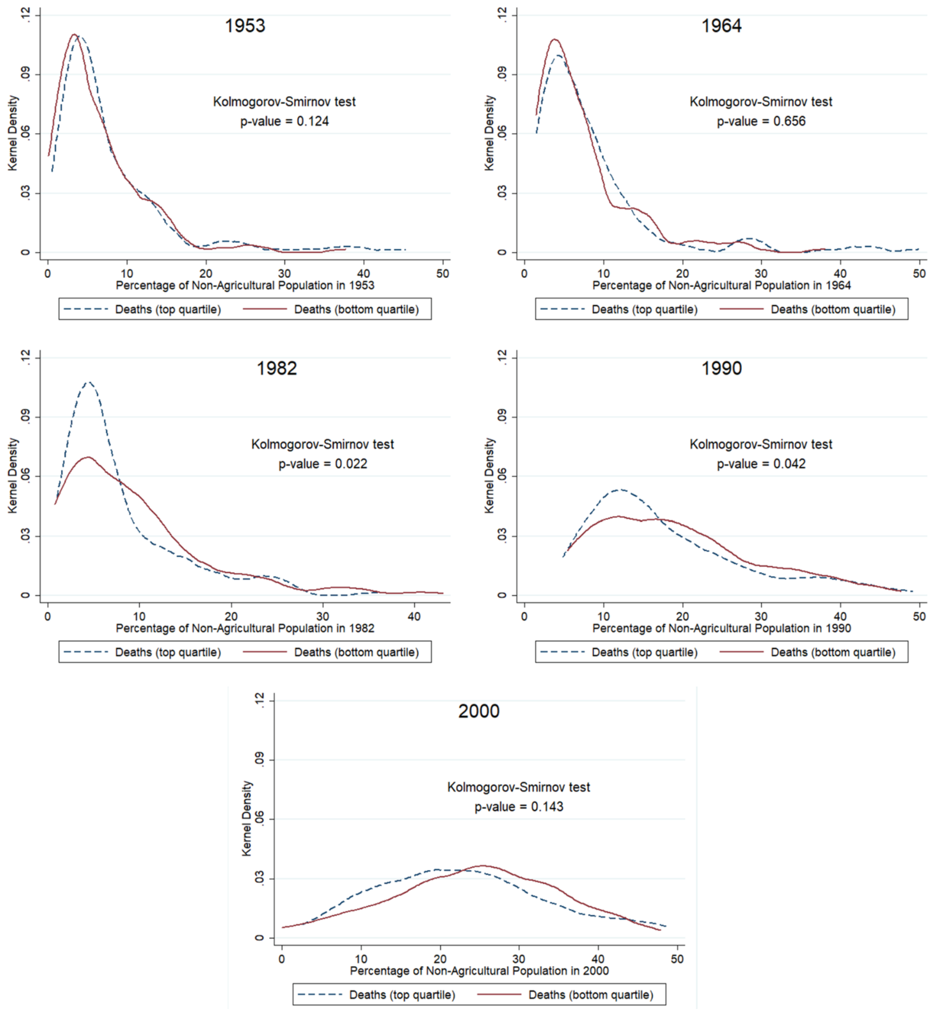

FIGURE 4
REVOLUTIONARY INTENSITY AND INDUSTRIALIZATION

Notes: This figure plots the distribution of the share of non-agricultural population in selected counties for the census years 1953, 1964, 1982, 1990, and 2000. The solid line corresponds to counties in the bottom quartile of revolution-related deaths, while the dashed line corresponds to counties in the top quartile. The sample consists of the 600 counties for which we have data in all five census years.
Source: The source for revolutionary intensity is Walder (2014). The sources for regional industrialization are population census data and county gazetteers.

of pre-trends. From Columns (1) to (4), we progressively account for different sets of time-varying county controls so as to capture differential time trends driven by county characteristics.

In Column (1), we first control for the time-varying effects of pre-revolution socio-economic factors, including inequality, ethnic fragmentation,

TABLE 3

COUNTY-LEVEL PANEL RESULTS: REVOLUTIONARY INTENSITY AND INDUSTRIALIZATION

	A. 1953 Base-Year Sample				B. 1964 Base-Year Sample			
Dep. Var: Percent of Non-Agr. Population	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Deaths (% of county pop.) x D1964	–0.034	0.047	0.059	0.058				
	(0.367)	(0.364)	(0.378)	(0.375)				
Deaths (% of county pop.) x D1982	–1.214**	–0.848*	–0.816*	–0.824*	–1.128***	–0.874***	–1.139***	–1.101***
	(0.508)	(0.457)	(0.483)	(0.491)	(0.248)	(0.227)	(0.288)	(0.293)
Deaths (% of county pop.) x D1990	–0.802	–0.502	–0.461	–0.506	–0.585***	–0.413**	–0.796***	–0.768***
	(0.527)	(0.490)	(0.538)	(0.547)	(0.206)	(0.197)	(0.276)	(0.284)
Deaths (% of county pop.) x D2000	–0.434	–0.088	–0.036	–0.098	0.141	0.429	–0.101	–0.173
	(0.565)	(0.536)	(0.622)	(0.640)	(0.343)	(0.332)	(0.422)	(0.435)
Pre-CR Social Capital x Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-CR Ethnic Frag. x Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-CR Inequality x Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg. Terrain Slope x Year dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Dist. to Prov. Capital x Year dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Dist. to Prov. Border x Year dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Provincial time trend	No	No	Yes	Yes	No	No	Yes	Yes
Word Count x Year dummies	No	No	No	Yes	No	No	No	Yes
Post-Deng Period Pub. x Year dummies	No	No	No	Yes	No	No	No	Yes
Observations	3,000	3,000	3,000	3,000	5,944	5,944	5,944	5,944
R-squared	0.804	0.813	0.848	0.849	0.825	0.830	0.868	0.869
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* = Significant at the 10 percent level.
** = Significant at the 5 percent level.
*** = Significant at the 1 percent level.

Notes: Robust standard errors—clustered at the county level—are in parentheses. The years 1953, 1964, 1982, 1990, and 2000 are census years. All results are weighted using 1964 population. Pre-revolution inequality is measured by the educational Gini coefficient, which is calculated using the 1982 census among individuals born before 1966.

Sources: Pre-revolution ethnic fragmentation index is calculated following Alesina et al. (2003), using the same sample. Pre-revolution social capital is proxied with the number of historical charity organizations (Wang 2013). Province time trends are interaction terms of province dummies with a linear time trend.

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and social capital, which may all have long-term impacts on economic development while being correlated with revolutionary intensity.[3] The estimates show a transient effect—a 1 percent increase in revolutionary intensity translates into approximately a 1.21 percent decrease in industrialization in 1982 (significant at the 5 percent level), while the effects are no longer significant in 1990 and 2000. In Column (2), we further control for potentially time-varying effects of geographic characteristics, including terrain slope, distance to the provincial border, and distance to the provincial capital. Here the estimated coefficients are smaller; for 1982, it falls by 30 percent from –1.21 to –0.85 and is only significant at the 10 percent level. Column (3) adds province-specific linear time trends, and the results are essentially unaffected.

A very similar, but more precisely estimated, set of coefficients emerges in the larger sample of 1486 counties (Panel B, Table 3). Here we once again gradually add control variables that allow for time-varying effects of county socio-economic and geographic characteristics as well as provincial time trends. Taking Column (7) as an example, a 1 percent increase in revolutionary intensity translates into a 1.14 percent decrease in industrialization in 1982, a 0.80 percent decrease in 1990, and a 0.10 percent decrease in 2000. Figure 5 provides a visual summary of the main results.

Potential Reporting Bias

As our revolution-related death data come from regional gazetteers, a potential concern is measurement error due to censorship and/or misreporting. In particular, we are concerned about non-classical measurement error, where the extent of misreporting is systematically correlated with revolutionary intensity. In this subsection, we first discuss the role of censorship and incentives for misreporting during the 1980s and 1990s (when most of the gazetteers were published), before employing two proxies to control for potential reporting bias.

After the revolution ended in 1976, the central government carried out an official reassessment of the period, which concluded that "*[the Cultural*

[3] In the case of inequality, the underlying mechanism could be that pre-1966 inequality resulted in higher revolutionary intensity and also hindered economic development in the long run. A similar mechanism could be at play in the case of ethnic fragmentation. Indeed, Arbatlı et al. (2020) find that more diverse populations are more prone to conflict. For social capital, the revolution may have magnified any pre-existing differences, such that counties with low social capital to begin with may have experienced more conflict during 1966–76, and, in turn, have worse economic performance in the long run. This interpretation would be similar to the local continuity of cultural traits, as in Voigtländer and Voth (2012, 2015).

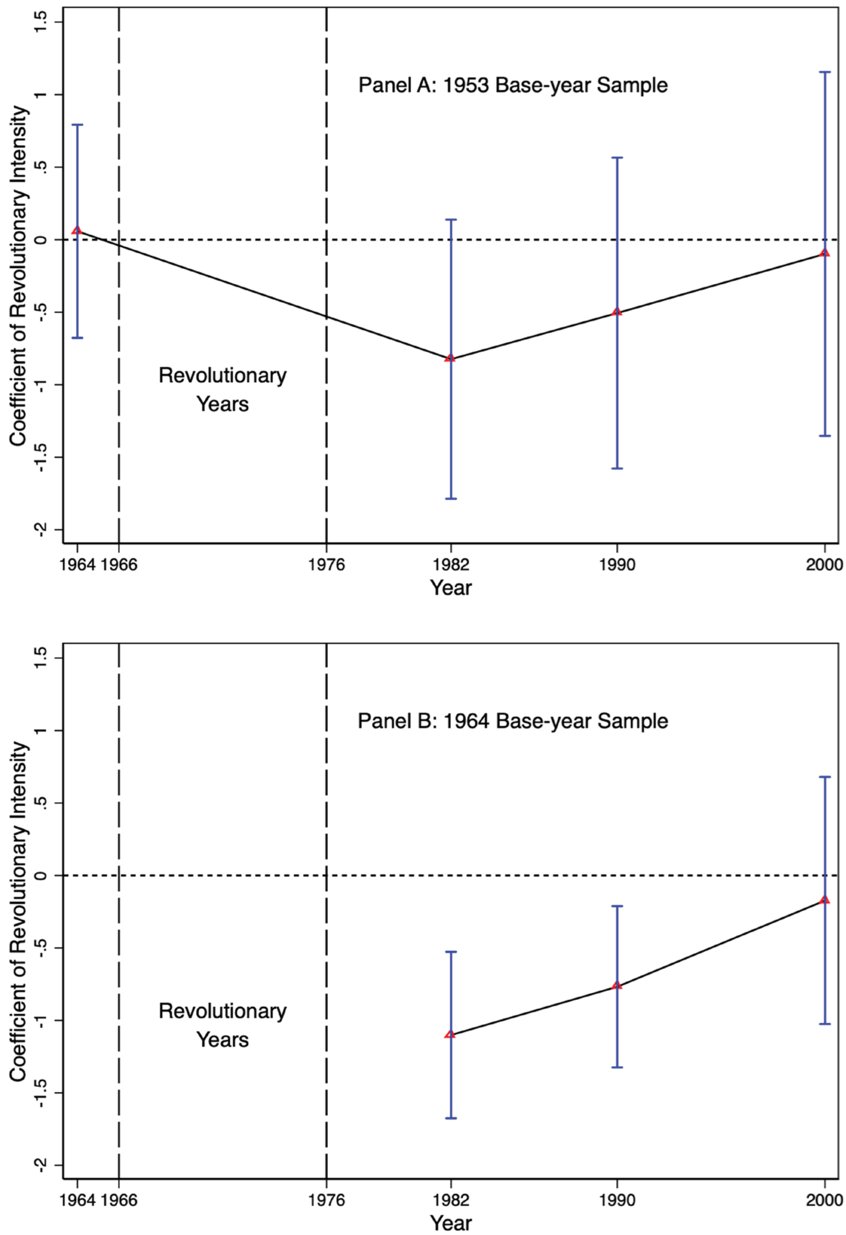

FIGURE 5
REVOLUTIONARY INTENSITY AND INDUSTRIALIZATION

Notes: The dependent variable is the extent of industrialization, as proxied by the fraction of local population working in non-agricultural sectors. We control for county and year fixed effects, province time trends, as well as numerous baseline county characteristics and their interactions with census-year dummies.
Sources: This figure plots coefficients on the interaction between revolutionary intensity and census-year dummies, corresponding to Columns (4) and (8) of Table 3. The analysis is carried out using county-level data from all census years between 1953–2000.

Revolution] was an upheaval that was mistakenly mobilized, manipulated by counter-revolutionary cliques, which resulted in severe disaster and turmoil among the Party and the Chinese people." Furthermore, the government initiated a process of rehabilitation for victims who were wrongly persecuted. This involved compensation for affected family members as well as the pursuit of criminal cases. Meanwhile, the balance of political power within the Party shifted toward those leaders who themselves had been persecuted during the revolution, and it is not *ex ante* clear that local officials would have faced significant incentives to systematically under-report the severity of the revolution.

We further employ two sets of proxy variables to control for the extent of any reporting bias in our empirical analysis. The first proxy is based on the publication dates of gazetteers, which range from 1980 to 2009. Specifically, we defined a dummy variable indicating whether a particular gazetteer was published during the post-Deng period (after 1997). The rationale is to allow for potentially different reporting incentives corresponding to changes in the political climate. The second set of proxy variables is derived from measures of narrative content, specifically the number of words devoted to documenting the revolution. Since most relevant records are located in either (1) the chronicle of major county events or (2) a special section on the Cultural Revolution, we use the number of words in each of these two sections separately. Here, our working assumption is that both word counts would be negatively correlated with any reporting bias.

The cross-sectional correlations between these proxy variables and the measure of revolutionary intensity are shown in Columns (4) and (8) of Table 2. Here we see that publication during the post-Deng period does not predict more reported deaths, while a higher word count does. This is especially true for the special section about the revolution. Given these results, we proceed to control for potentially time-varying effects of these proxy variables in our main county-level panel regressions (Columns (4) and (8) of Table 3). Doing so leaves the estimated coefficients virtually unchanged, suggesting that misreporting is unlikely to bias our results significantly.

Robustness: Alternative Specifications

ALTERNATIVE OUTCOME MEASURE

While our baseline specification uses a logged measure of industrialization, the results are robust to using a raw measure instead (Online Appendix Table B1). One difference is that the 1990 coefficients are larger than those in the logged specification for the 1953 base-year sample. The main finding, that the revolution had an adverse effect on regional development during the 1980s, which then diminished over time, remains unchanged.

ALTERNATIVE EXPLANATORY VARIABLE

Another way of measuring revolutionary intensity is to use total deaths instead of baseline population in the denominator. Unfortunately, data for total deaths is not available at the county-year level for this period. To make progress, we construct an estimate by combining: (i) province-level mortality rates during 1966–76;[4] and (ii) county population shares in 1964. Hence, total deaths during the revolution in county i of province p is imputed as:

$$\widehat{TotalDeaths_i} = TotalDeaths_p \times Share_i^{1964}, \tag{2}$$

where $Share_i^{1964} = \dfrac{Population1964_i}{Population1964_p}$. Total deaths in province p during the revolution is calculated as $TotalDeaths_p = \sum_{t=1966}^{1976} Population_{p,t} \times DeathRate_{p,t}$. The underlying assumption is that total deaths in county i during this period are proportional to its population share in 1964. Using the imputed county-level total deaths as an alternative denominator, the new intensity measure is: $Log(CR\,deaths_i / \widehat{Total\,deaths_i} \times 100 + 1)$. Furthermore, we also consider a version of total deaths during the period of 1966–1971. The findings are similar to those from our baseline specification, where the revolution had a negative, but time-limited impact on industrialization (Online Appendix Table B2). This effect is largest for 1982 before diminishing so as to be insignificant by 2000. As in the case of Table 3, the coefficients are more precisely estimated for the larger, 1964-base-year sample (Columns (2) and (4)). In terms of magnitude, the alternative denominator yields larger numerical values of revolutionary intensity by construction and therefore smaller coefficient estimates.

BINARY TREATMENT

Our main specification employs a continuous measure of revolutionary intensity. We have also considered a binary definition of treatment, where

[4] The data source is *Comprehensive Statistical Data and Materials on 50 Years of the People's Republic of China (1999).*

counties with zero revolution-related deaths serve as the "control" group. This enables us to make a more clear-cut comparison between counties with and without revolution-related deaths. However, less variation is being used for identification. With the binary measure, we find consistent qualitative patterns, as in the baseline results, that indicate that the revolution adversely affected industrialization in the early 1980s before diminishing in influence by the 1990s (Online Appendix Table B3).

Robustness: Alternative Interpretations

THE GREAT FAMINE

The first alternative interpretation of our results is related to another important historical episode—the Great Famine (1959–1961). Given its temporal proximity to the revolution and the magnitude of the shock, it is possible that famine severity is a potentially confounding factor in our empirical analysis. To control for this, we follow the existing literature (Meng, Qian, and Yared 2015; Chen et al. 2020) and employ a cross-sectional measure of famine severity, using the relative cohort size of survivors as a proxy to capture the relative population loss:

$$FamineSeverity_p = 1 - \frac{FamineCohortSize(1959-61)_p}{PreFamineCohortSize(1955-57)_p}, \qquad (3)$$

where p denotes prefecture, $FamineCohortSize(1959 - 61)_p$ is the average cohort size during the Famine years (1959–1961) in prefecture p, and $PreFamineCohortSize(1955 - 57)_p$ is the average cohort size during the pre-Famine years (1955–1957) in prefecture p. Thus, greater famine severity is captured by a larger relative loss in terms of the population during the famine. As with our other pre-revolution control variables (social capital, ethnic fragmentation, inequality), this measure is constructed at the prefecture level because the 1982 census only contains prefecture, rather than county, identifiers.[5] Next, we directly control for the time-varying effects of famine severity by including interaction terms between it and year dummies as regressors. Compared to our baseline results (Table 3), the coefficients of interest are slightly larger in magnitude and, in some cases, more significant (Online Appendix Table B4). Thus, the results do not appear to be driven by the Great Famine.

[5] A prefecture is a mid-level administrative unit between a province and a county. As of 2018, there are 2851 counties and 333 prefectures in total. Thus, one prefecture includes around nine counties on average.

SEX RATIO

Another way to think about our results is that the revolution may have affected regional development simply via changes in demographic structure. This could happen if more men died as a result of revolution-related violence, and they are more likely to take up non-agricultural employment. To test this, we examine the dynamic effects of revolutionary intensity on the sex ratios of different regions by estimating the following regression:

$$SexRatio_{p,t} = \alpha + \sum_{k=2}^{10} \beta_k Intensity_p \times 5YearBin_k + \theta_t + \mu_p + \epsilon_{p,t}, \quad (4)$$

where $Intensity_p$ is measured by the number of revolution-related deaths in province p as a fraction of its 1964 population, μ_p capture province dummies, and θ_t year fixed effects. The dependent variable is the population sex ratio in province p and year t. To flexibly estimate the dynamic effects of the revolution, we divide the sample period (1951–2000) into ten five-year bins, where $5YearBin_k$ is an indicator variable for a specific bin k. The omitted/comparison period is 1951–1955, and the coefficients of interest are the β_ks. The estimates are small in magnitude and statistically insignificant (Figure A6 in the Online Appendix), which indicate that worse-affected provinces do not experience a divergent demographic structure in terms of their sex ratios.

MIGRATION AND POPULATION DENSITY

Another way through which the revolution may have affected subsequent outcomes of different regions is by changes in migration and/or population density. Since our analysis at the county level estimates relative impacts only, it could be that migration exacerbated the impact if skilled individuals moved away from worse-affected regions. On the other hand, it could be that better-educated individuals had an incentive to move into worse-affected regions post-1976, given their shortage of skilled labor and potentially higher returns. Inter-regional migration was very limited before the 1990s (Brandt and Rawski 2008), so comparisons between the first two periods—1964 and 1982—are unlikely to be affected by major migration patterns.[6] That said, one may be concerned about comparisons during the later years of our sample and about the potential correlation between revolutionary intensity and population density/migration.

[6] Using individual-level census data from 1990, we found that 97.2 percent of the population were residing in the same county where their Hukou was registered, and 92.6 percent were residing in the same county as five years ago.

To control for these factors, we first calculate the proportion of migrants among the local labor force (aged 16–60) in each of 1964, 1982, 1990, and 2000 using individual-level census data. Here, migrant status is defined as those who were not residing in the county of Hukou registration at the time of the census. For population density, we divide total population (at the county-year level) by total land area (at the county level). Logged measures of migration and population density are then included as regressors. This is done using the 1964-base year sample since we only have information on these variables from 1964 onward (Online Appendix Table B5). Compared to our baseline estimates (Column (8), Table 3), the coefficients are virtually unchanged, both in terms of magnitude and statistical significance. This suggests our results are unlikely to be driven by differences in migration or population density across regions.

MEAN REVERSION

Finally, given the positive correlation between baseline industrialization and revolutionary intensity (Table 2), one might be concerned that the results are simply driven by mean reversion. To test this, we use changes in industrialization (during 1964–1982 and 1964–1990, respectively) as the dependent variable in a cross-sectional regression. Here we are interested in whether the coefficient estimate of revolutionary intensity changes after controlling for baseline industrialization (in 1964). If mean reversion is driving our results, we should find the effects of revolutionary intensity become much weaker. While there is evidence consistent with mean reversion—more industrialized counties at baseline do have slower growth in industrialization in subsequent periods—controlling for baseline industrialization does not diminish the effect of revolutionary intensity, either in economic magnitude or statistical significance (Online Appendix Table B6). Therefore, mean reversion does not appear to confound our baseline results.

Heterogeneous and Non-Linear Effects

QUANTILE REGRESSION ESTIMATES

To explore potentially heterogeneous treatment effects, we use quantile regression to estimate the revolution's impacts on different segments of the industrialization distribution. Taking the temporal dimension of our data into consideration, we implement the "correlated random effects" (Mundlak) estimator (Abrevaya and Dahl 2008; Machado and Santos

Silva 2011). In order to make the analysis more comparable, we keep the temporal dimension the same for both samples (1964–2000) in this analysis. Overall, the effect of revolutionary intensity is negative across all quantiles of industrialization in both 1982 and 1990 (Online Appendix Table B7). In the case of 1982, the effect is larger for the upper quantiles of the 1953-base year sample (Panel A), while being more uniform throughout the distribution of the 1964-base year sample (Panel B). In the case of 1990, the effect is relatively uniform for both samples and more precisely estimated for the larger, 1964-base year sample.

NON-LINEAR SPECIFICATION

Our baseline analysis assumed a linear relationship between revolutionary intensity and industrialization. To explore potential non-linear effects, we report estimates from a specification including quadratic terms of revolutionary intensity (Online Appendix Table B8). To understand the revolution's overall effects, we combine the coefficient estimates of the linear and quadratic terms. While some of the quadratic interaction terms are statistically significant, particularly for the 1964 base-year sample, their effects are small in magnitude. This is largely due to the small values of our main revolutionary intensity variable (mean = 0.03). Another way to see this is by calculating the value of revolutionary intensity corresponding to the minimum of industrialization. For instance, looking at the effects in 1982 from Column (8), we can calculate the turning point of the U-shaped relationship: it is located where revolutionary intensity is equal to $0.56\left(=\frac{1.917}{2\times 1.722}\right)$. This falls within the 99th percentile value of revolutionary intensity, so nearly all of the counties in our sample are in the downward-sloping region of the quadratic curve, suggesting an absence of major non-linearities.

Having studied the dynamic effects of the revolution on regional development at the county level, we next complement this analysis by examining how it may have affected human capital and labor market outcomes at the individual level. In doing so, we hope to shed light on some of the likely mechanisms underlying the earlier results.

INDIVIDUAL-LEVEL ANALYSES

Effects on Education

To investigate the revolution's effect on educational outcomes, we use individual-level census data and geographic variation in revolutionary

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intensity. In particular, we use the 2000 population census 1 percent sample to estimate a series of flexible effects by 5-year cohort bins:

$$Edu_{ipt} = \alpha + \Sigma_t \beta_t Intensity_p \times BirthCohort_t + \gamma' X_{ipt} + \theta_t + \mu_p + \kappa_p \times t + \epsilon_{ipt}, \quad (5)$$

where $Intensity_p$ is measured by the number of revolution-related deaths in prefecture p as a fraction of its population in 1964, μ_p are prefecture fixed-effects and θ_t are cohort fixed-effects;[7] X_{ipt} are individual controls including gender, ethnicity, and household registration status, while $\kappa_p \times t$ are province cohort trends. The dependent variables Edu_{ipt} are: (i) years of schooling, and (ii) a dummy for graduating from college and above for individual i born in year t living in prefecture p. Here we have a series of interaction terms between intensity and birth-cohort indicators, ranging from 1936–40 to 1976–80. Our sample includes everyone aged between 20 and 70 at the time of the census. Those born during 1931–35, therefore, make up the omitted comparison group.

The results are shown in Table 4 and Figure 6, where two main patterns emerge. First, revolutionary intensity does not appear to have affected years of schooling on average (Panel A, Figure 6 and Column (1), Table 4). This result is perhaps unsurprising, given that during this period individuals of normal graduation age for middle or high schools were often given diplomas even though they did not complete a traditional middle or high school education. The picture changes significantly if we instead focus on whether an individual has obtained a college degree, as a proxy for upper-tail human capital. Here, it appears that cohorts born during the mid- to late-1950s experienced the largest impacts. In terms of magnitude, compared with cohorts born during 1931–35, an increase in revolutionary intensity from the 25th to the 75th percentile is associated with a 6.9 percent decrease in the odds of being a college graduate for cohorts born during 1956–60. Using alternative comparison groups (cohorts born in 1936–40 or 1941–45) leave the results unchanged (Online Appendix Figures A7 and A8).

It is reassuring to note that the estimated coefficients are statistically insignificant and consistently close to zero for cohorts born between 1936 and 1946, whose education should have been completed by the onset of the revolution. Furthermore, for cohorts born in the 1970s who would have started primary school after 1976, revolutionary intensity does

[7] The finest level of geographic identifier in the 2000 individual-level census data is that of a prefecture. Hence, we aggregate the revolutionary intensity variable to the prefecture level for the analyses in this section. Online Appendix Table B9 reports a prefecture-level version of the earlier industrialization results. The coefficient estimates are very similar to those from our county-level analysis. The smaller sample size does, however, make the estimates less precise.

TABLE 4
INDIVIDUAL-LEVEL RESULTS: REVOLUTIONARY INTENSITY, EDUCATION, AND LABOR MARKET OUTCOMES

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Years of Schooling	College or Above	Employed	Days Worked	Professional	Entrepreneur
Comparison Group: Cohort Born during 1931–1935	Least Squares	Logit	Logit	Least Squares	Logit	Logit
Deaths (% of pref. pop.) x D1936–40 (log)	0.121	0.210	–0.034	–0.311	0.278	–0.860
	(0.076)	(0.242)	(0.133)	(0.337)	(0.406)	(0.853)
Deaths (% of pref. pop.) x D1941–45 (log)	0.082	0.482	0.481	0.064	–0.105	–1.398
	(0.098)	(0.482)	(0.284)	(0.456)	(0.598)	(0.893)
Deaths (% of pref. pop.) x D1946–50 (log)	0.086	0.074	0.482	–0.095	–0.227	–1.677*
	(0.110)	(0.297)	(0.375)	(0.483)	(0.519)	(1.012)
Deaths (% of pref. pop.) x D1951–55 (log)	0.127	–0.805*	0.461	–0.039	–0.369	–1.995*
	(0.130)	(0.473)	(0.481)	(0.585)	(0.483)	(1.083)
Deaths (% of pref. pop.) x D1956–60 (log)	0.137	–1.791***	0.377	–0.101	–1.180**	–2.740**
	(0.143)	(0.658)	(0.558)	(0.651)	(0.519)	(1.257)
Deaths (% of pref. pop.) x D1961–65 (log)	0.055	–1.070**	0.036	–0.205	–0.879*	–2.409**
	(0.166)	(0.462)	(0.617)	(0.713)	(0.529)	(1.161)
Deaths (% of pref. pop.) x D1966–70 (log)	0.119	–0.048	–0.107	–0.288	–0.074	–1.513
	(0.186)	(0.400)	(0.680)	(0.771)	(0.548)	(1.221)
Deaths (% of pref. pop.) x D1971–75 (log)	0.229	0.064	–0.222	–0.485	–0.117	–1.142
	(0.205)	(0.417)	(0.782)	(0.855)	(0.600)	(1.270)
Deaths (% of pref. pop.) x D1976–80 (log)	0.323	–0.315	–0.472	–0.735	–0.558	–1.593
	(0.224)	(0.570)	(0.938)	(0.997)	(0.684)	(1.487)
Observations	7,268,154	7,268,154	7,268,154	7,268,154	7,268,154	7,268,154
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Province cohort trends	Yes	Yes	Yes	Yes	Yes	Yes

* = Significant at the 10 percent level.
** = Significant at the 5 percent level.
*** = Significant at the 1 percent level.
Notes: Robust standard errors—clustered at the prefecture level—in parentheses. Since the 2000 census only contains a four-digit prefecture identifier, we use prefecture-level revolutionary intensity. Individual controls include gender, ethnicity and household registration status. Province cohort trends are interaction terms between province dummies and a linear cohort trend. Our sample includes cohorts born between 1931 and 1980. We take as the comparison group cohorts born during 1931–35, and show robustness to using alternative comparison groups in Online Appendix Figures A7 and A8.
Sources: 2000 Census data.

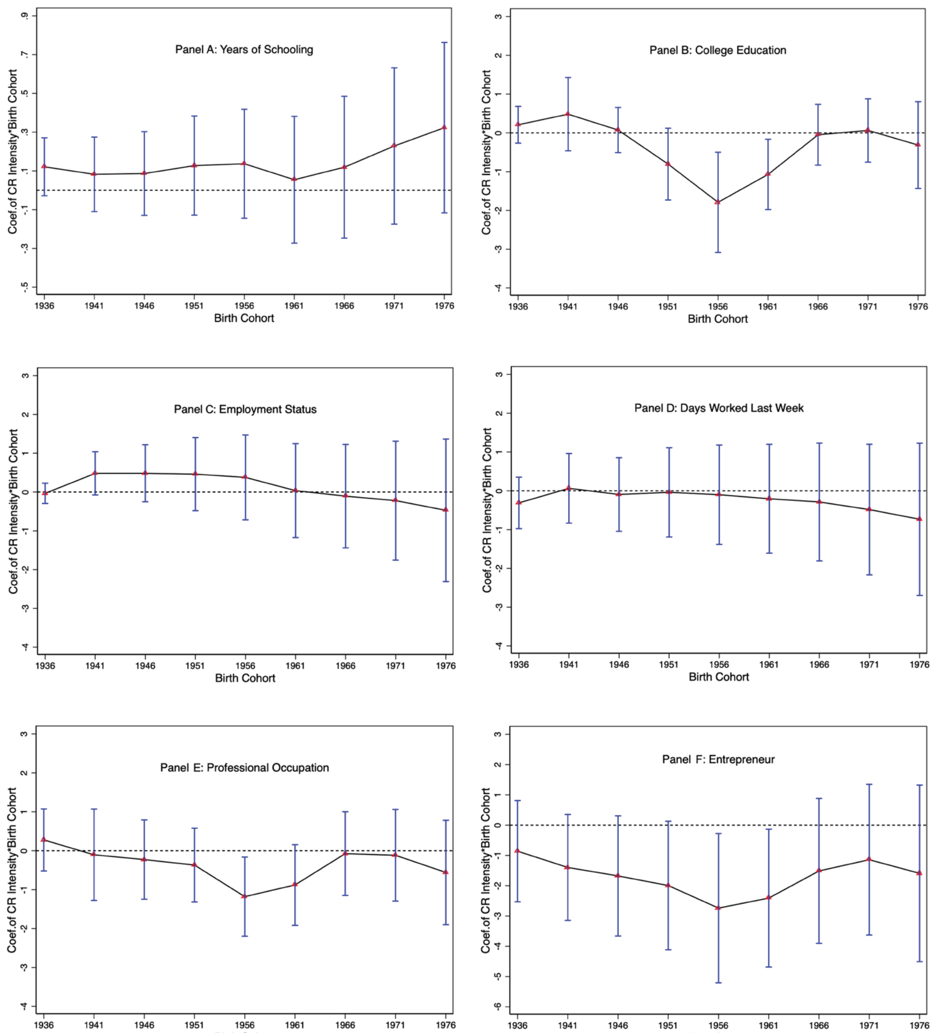

FIGURE 6

REVOLUTIONARY INTENSITY AND LABOR MARKET OUTCOMES—1931–35 COHORTS AS COMPARISON GROUP

Notes: We control for prefecture and birth cohort fixed effects, individual demographics (gender, ethnicity and household registration status), and province cohort trends. Our sample includes cohorts born between 1931 and 1980. We take as the comparison group cohorts born during 1931–35, and show robustness to using alternative comparison groups in Online Appendix Figures A7 and A8.

Sources: This figure plots coefficients on the interaction between revolutionary intensity and 5-year cohort indicators, corresponding to Columns (1)–(6) of Table 4. The analysis is carried out using individual-level data from the 2000 census. The dependent variables are years of schooling (log) coded based on educational attainment (Panel A), a dummy for graduating from college (Panel B), employment status (1=employed, 0=otherwise) (Panel C), number of days worked during past week (Panel D), being in a professional occupation (Panel E), and being an entrepreneur (Panel F). Since the census only contains a four-digit prefecture identifier, we use prefecture-level revolutionary intensity.

not play a significant role. This suggests there were no major long-term adverse effects in terms of education for cohorts who did not experience the revolution directly. Taken together, our findings are in line with and add to the results from several earlier studies (Cai and Du 2003; Meng and Gregory 2002, 2007; Giles, Park, and Wang 2019; Li and Meng 2022). Compared to these studies, our empirical approach differs in two respects. First, our identification strategy exploits both cohort variation in exposure and regional variation in intensity. This allows us to control for cohort fixed effects as well as province-specific cohort trends, which may serve as confounding factors in earlier studies. Second, our analysis makes use of a much larger sample from census micro data, rather than survey data. This not only helps with external validity but also with statistical power and therefore precision of estimates.

There are multiple channels through which the revolution could affect school attainment, both on the demand and supply sides. Here we provide some suggestive evidence for one demand-side channel, the perceived value of education. The hypothesis is that exposure to the revolution may reduce one's perceived value of education. This is because the "uselessness of education" was a prominent slogan at the time. Academic performance was no longer the key to a student's future career success. In fact, a preoccupation with academic studies at the cost of revolutionary engagement may be interpreted as a sign of "anti-socialist professionalism." To study this more systematically, we employ data from the China General Social Survey (CGSS). Its 2006 wave includes the following question: "How important is education for success?" The answer is measured on a Likert scale ranging from 1 to 5. In the empirical analysis, we normalize and standardize the scale so that a higher value corresponds to greater importance being attached to education.

As in our other individual-level analyses, we adopt an empirical strategy with both cohort variation in exposure and regional variation in intensity. Unfortunately, only province indicators are available in the CGSS data, so we have to further aggregate the intensity measure. Both province and cohort fixed effects are controlled for, and the comparison group is those born between 1936–1940 (Online Appendix Table B11). The results are consistent with the idea that more exposed cohorts (those born between 1956–1960) from worse-affected regions had a lower perceived value of education. It should be noted, however, that the evidence is only suggestive since the estimates are less precise and the revolutionary intensity measure is at the province level. Therefore, there could be other important complementary channels as well (e.g., via the supply of educational resources).

Effects on Labor Market Outcomes

Besides education, another important mechanism through which the revolution may have affected economic development is employment. This could involve both the level and nature of employment. To investigate this, we once again use census micro data from 2000. The first set of dependent variables is employment status (1 = employed, 0 = otherwise) and the number of days worked during the past week. The estimates show the revolution does not appear to have affected either employment status or the number of days worked when we include the full set of controls (Columns (3) and (4), Table 4). The key patterns for each cohort are visualized in Panels C and D of Figure 6.

Extending the analysis further, we also examine the revolution's effects on the *nature* of employment (across occupations) and thereby the allocation of talent. During the revolution, individuals from certain occupations were much more likely to be targeted because of their perceived lack of political loyalty. These covered professional occupations (particularly those in the education sector), as well as those with an entrepreneurial background.[8]

We investigate these potential effects by studying the following outcomes: (i) a dummy variable for working in a professional occupation (including scientific researchers, engineers, technicians, economic/finance/legal professionals, as well as teachers and professors); and (ii) a dummy variable for being an entrepreneur (someone responsible for the running of a business.) As before, our sample includes everyone born between 1931 and 1980, with those born between 1931–35 serving as the omitted comparison group. The results indeed show an effect on occupations for the most exposed cohorts (Table 4). Compared with those born during 1931–35, an increase in revolutionary intensity from the 25th to the 75th percentile is associated with a 4.6 percent decrease in the odds of working in a professional occupation for cohorts born during 1956–60. This effect is strongest for those born during the mid-1950s to mid-1960s, which is consistent with the earlier results on college education. This could be because higher qualifications are required for certain professional occupations. Similarly, such an increase in revolutionary intensity is associated with a 10.4 percent decrease in the odds of working as an entrepreneur for cohorts born during 1956–60. Again, using alternative comparison groups (cohorts born in 1936–40, or 1941–45) leave the results unchanged (Online Appendix Figures A7 and A8).

[8] The crucial roles of professionals and entrepreneurs in promoting economic growth have been well documented (Iyigun and Owen 1998, 1999; Baumol 1990; Galor and Michalopoulos 2012; Maloney and Valencia Caicedo 2022).

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In principle, these results could be driven by either demand (through individuals' occupational preferences) or supply (through local labor market conditions). There is also likely to be heterogeneity across occupations; while centralized job assignment played an important role for professional occupations in the 1980–90s, the decision to become an entrepreneur is likely a more personal one. Taken together, these findings add to the literature in two ways. First, compared to existing studies (Meng and Gregory 2002, 2007), our analysis employs both regional and cohort variation and shows that more-exposed cohorts experienced long-lasting effects in terms of their educational and labor market outcomes. Second, they also add to the literature on occupational choice and entrepreneurship (Levine and Rubinstein 2017) by showing the salience of historical events in shaping occupational outcomes at the individual level.

Discussion

The county- and individual-level results shed light on the revolution's economic legacies at the macro- and micro-levels, respectively. In this subsection, we discuss the relationship between these two sets of results before considering potential mechanisms that could explain the initially large but time-limited effects on regional development.

When thinking about the results together, one issue is that while the largest effect on industrialization appears in the early 1980s, the most exposed cohorts (those born between 1955–65) should only become the backbone of their local economies in the late 1980s and early 1990s. There are some caveats to consider when making such a link, however. In particular, a direct link would require further assumptions on how the age-specific profiles of human capital affect aggregate economic performance. Moreover, the county-level results likely reflect effects stemming from a multitude of mechanisms, some of which go beyond what we have examined in the individual-level analyses.

These caveats notwithstanding, there are two existing patterns in the data that can help us connect these results. First, estimates for the 1964-base year sample (Column (8) of Table 3) make clear that we cannot rule out the negative effects on industrialization remaining large during the late 1980s and early 1990s. By this time, the most exposed cohorts would have become a key part of the labor force. This is also true for the 1953-base year sample, when we use a raw measure of industrialization (Online Appendix Table B1). Second, although the most statistically significant individual-level results are for the 1955–65 cohort, the point estimates are also negative for the 1951–55 cohort in the case of college education. In the case of entrepreneurship, the effects are negative for an even wider window of cohorts, who would have been part of the labor force already in the early 1980s.

The individual-level results can also help us better understand why the effects of industrialization dissipate over time. In particular, while the most exposed cohorts (those born between 1955–65) experienced long-lasting effects in terms of their human capital and labor market outcomes (measured in 2000), younger cohorts (those born in the 1970s) without first-hand experience of the revolution appear much less affected. As we move toward the end of our sample period, these latter cohorts start to become an increasingly significant part of the workforce, thereby potentially tempering the effects of the revolution on local economic development.

Moreover, some of the early impact on industrialization could be due to a reallocation effect. The revolution's widespread nature, as well as its lengthy duration, meant that restoring political and social stability, rehabilitating victims, and returning to normal industrial production took time to achieve. Therefore, some of the early effects could reflect an incomplete reallocation of labor, capital, and other resources away from political campaigning toward economic production. By the late 1980s and 1990s, however, this direct reallocation effect should no longer be significant.

Finally, in addition to human capital and entrepreneurship, there is existing work suggesting the revolution may have affected local economic development through the speed and scope of reform. For instance, Xu (2011, p. 1079) writes "*The Cultural Revolution ... and its ensuing destruction of communist institutions and society has led to disillusionment with communist ideology, a change of the legitimacy base of the Chinese Communist Party, ..., paving the road for post-Mao reforms*." In such a scenario, worse-affected areas may have been faster in moving away from the earlier planned-economy regime during the reform period, enabling them to catch up toward the end of our sample period.

CONCLUSION

This paper investigates the economic legacies of the Cultural Revolution, both in terms of the development trajectories of regions and the education/labor market outcomes of individuals. The analysis exploits regional variation in revolutionary intensity as well as time/cohort variation in exposure. The empirical approach uses multiple rounds of census data, as well as original data collected from gazetteers, to investigate its dynamic effects.

The county-level results show that worse-affected areas performed slightly better at baseline but were slower to industrialize afterwards. This effect was large in the early 1980s before diminishing to become insignificant by 2000. This effect is transient and indicates a loss of economic output, and thus welfare, during the period immediately following the revolution.

Results at the individual level show those born during the 1950s and early 1960s in worse-affected areas are less likely to obtain higher education degrees and to work in professional and entrepreneurial occupations as adults. While the most exposed cohorts experienced long-lasting effects, younger cohorts (those born in the 1970s) without first-hand experience of the revolution appear to be much less affected.

Several caveats should accompany these findings. First, the revolution was a nationwide movement. To the extent it shaped the economic trajectory of the country as a whole, we are unable to capture it in the analysis, which exploits sub-national variation in revolutionary intensity. Second, the estimated impacts should be considered as the effects of the revolution as a whole since the data do not allow us to separately study different aspects of the revolution (for instance, violence versus ideology). Third, although the revolution appears to have affected the economic development of affected regions for a period of time, it may have helped pave the way for market-oriented reforms in the 1980s and 1990s (Xu 2011).

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