Human Capital Destruction and Economic Performance: Quasi-Experimental Evidence from China's Cultural Revolution

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Abstract

This thesis examines the effect that human capital exerts on per worker income, using China's Cultural Revolution as a natural experiment. The decade-long Revolution resulted in severe political persecution of millions among the educated class, and disrupted the schooling of millions more. I exploit cross-provincial variation in political casualties to identify the causal effect of lost human capital on incomes, using both two- and three-stage least squares estimators. My baseline estimates suggest that a one percent decline in human capital accumulation results in income reductions in the order of around 7 percent, which rises as the Revolution progresses, before diminishing in the later years. The finding remains robust to a host of robustness checks, including using alternative variable measures, additional controls, changes to the instrument set, the use of growth rather than levels, and falsification exercises. I also find some evidence that the human capital effect is transmitted more at the secondary and tertiary (rather than primary) levels, and on manufacturing and services, rather than agriculture.

Dedication

para Tita, mi única.

Acknowledgments

I thank Andrew Walder for making available his data on political persecutions, and to Bai Liang for additional pointers on data sources.

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As always, all errors and omissions belong solely to the author.

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I.

Introduction

The Great Proletarian Cultural Revolution is widely regarded as one of the greatest human tragedies in China's long history. For eleven bloody years between 1966 and 1976, as many as 1.6 million Chinese perished.[1] But unlike the previous Mao-era disaster—the Great Leap Forward, which resulted in three years of widespread famine and an estimated 45 million deaths[2]—the casualties incurred during the Revolution were not the result of severe weather shocks, amplified by policy missteps. Instead, they were due to overtly political reasons: victims faced discrimination on the basis of differences in perceived ideology or class, and many deaths resulted from the abuse and torture experienced by the estimated 36 million that endured some form of political persecution.[3] Many of these direct casualties were the educated class, and millions more suffered indirectly through forced disruptions to their education. Indeed, the Cultural Revolution can be understood as a particularly pernicious (if unintended) social experiment that resulted in the large-scale destruction of human capital.

Although seemingly secondary compared to the loss of life, shocks to human capital arguably exert a greater long-term influence, since the effects of forgone schooling cumulate over time, resulting in a lifetime of lost opportunities and stunted human development. During the Revolution, such effects operated at both ends of the educational spectrum: at the top, the country underwent an anti-intellectual purge, resulting in a lost generation as teachers and educated intellectuals underwent torture

[1] Andrew G. Walder, "Rebellion and Repression in China, 1966–1971", *Social Science History*, 38: no. 3–4 (Fall/Winter 2014) p. 532.

[2] Frank Dikötter, *Mao's Great Famine: The History of China's Most Devastating Catastrophe, 1958–1962* (London, England: Bloomsbury, 2010) p. 325.

[3] Andrew G. Walder and Yang Su, "The Cultural Revolution in the Countryside: Scope, Timing, and Human Impact", *China Quarterly*, 173: no. 1 (March 2003) p. 74.

and were relegated to hard labor in the countryside;[4] and across the bottom, many students experienced interruptions to their formal schooling,[5] while a host of others were denied university entry or faced grade discrimination on the basis of their class origins.[6]

In this thesis, I consider the effects of human capital on economic performance, using the politically-motivated reductions in education during the Cultural Revolution as a natural experiment. More specifically, I examine the effect of schooling enrollments on real gross provincial product per worker, after controlling for other standard determinants of production, such as the investment rate and the growth rate of the labor force. To identify the effect of human capital, I instrument schooling with province-specific data on politically-linked casualties. Because the victims of political persecution were typically teachers, intellectuals, and students undergoing formal education, such variations in the number of such casualties are closely related to changes in educational output at the margin. But since these losses were also the result of explicit political targeting, this approach allows me to rule out the confounding effects from other (unobservable) characteristics typically associated with human capital accumulation, such as individual cognitive ability[7] or the overall quality of educational

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[4]Roderick L. MacFarquhar and Michael Schoenhals, *Mao's Last Revolution* (Cambridge, MA: Belknap Press, 2008); Youqin Wang, *Student Attacks Against Teachers: The Revolution of 1966*, in *Proceedings of the Association of Asian Studies Annual Meeting* (Chicago, IL: AAS, March 14–16 1997b); idem, *The Second Wave of Violent Persecution of Teachers: The Revolution of 1968*, in *Proceedings of the 35th International Congress of Asian and North African Studies* (Budapest, Hungary: ICANAS, July 7–12 1997a).

[5]Zhong Deng and Donald J. Treiman, "The Impact of the Cultural Revolution on Trends in Educational Attainment in the People's Republic of China", *American Journal of Sociology*, 103: no. 2 (September 1997); Xin Meng and Robert G. Gregory, "The Impact of Interrupted Education on Subsequent Educational Attainment: A Cost of the Chinese Cultural Revolution", *Economic Development and Cultural Change*, 50: no. 4 (July 2002).

[6]Susan L. Shirk, *Competitive Comrades: Career Incentives and Student Strategies in China* (Berkeley, CA: University of California Press, 1982); Jonathan Unger, *Education Under Mao: Class and Competition in Canton Schools, 1960-1980* (New York, NY: Columbia University Press, 1982).

[7]Eric A. Hanushek and Ludger Wößmann, "Do Better Schools Lead to More Growth? Cognitive

service delivery.[8]

My approach adopts a two-step empirical strategy. First, I obtain predictions of provincial-level educational outcomes as a function of resources and political casualties, along with other covariates that may account for systematic variations in either education or casualties. In the second step, I use these predicted levels of human capital in cross-province regressions of per-worker income. By relying on casualties as a source of exogenous variation, I address endogeneity concerns, such as reverse causality or unobserved heterogeneity (perhaps due to, say, systematic productivity or cultural differences at the provincial level), which typically arise when direct measures of education are used instead. The reliance on instruments also allows me to sidestep potential challenges in measurement, as long as the exclusion restriction holds regardless. I go on to examine the quality of my estimates by running the baseline results through a battery of robustness checks, including alternative measures of the main independent and dependent variables, the inclusion of additional controls, variations in instrument choice, as well as basic falsification tests.

My baseline estimates suggest that for every one percent decline in human capital accumulation, per capita incomes fall by around 7 percent as a result. This effect is statistically significant, and rises as the Revolution progresses, although the effect diminishes and becomes indistinguishable from zero in the later years. The finding that human capital affects income also remains largely robust to a battery of robustness checks, including the use of alternative measures the dependent and main independent variables, the inclusion of additional controls, changes to the instrument set, the use of growth rates rather than levels to measure the variables, and simple falsification

Skills, Economic Outcomes, and Causation", *Journal of Economic Growth*, 17: no. 4 (December 2012).

[8]Sanjeev Gupta, Hamid R. Davoodi and Erwin R. Tiongson, "Corruption and the Provision of Health Care and Education Services", chap. 6 in Arvind K. Jain (ed.), *The Political Economy of Corruption* (London, England: Routledge, 2001).

exercises. I also find some evidence that the human capital effect is transmitted more at the secondary and tertiary than primary level (although all channels are statistically significant), and affect manufacturing and services output, rather than agriculture.

The analysis pairs historical official provincial-level data—which includes proxies for the standard factors of production (capital, labor, and human capital) common in augmented Solow-style growth models[9]—with records of political casualties drawn from local annals (*difangzi*).[10] This sets up the study for a number of secondary analyses. Since the official data are available as a panel, I am able to run repeated cross-sections to analyze the evolution of the influence of human capital on income over the course of the Revolution. Furthermore, I am able to pair the granular local data on casualties with county-level statistics from a more recent period, and explore the degree to which these historical shocks exhibit persistence over long time periods. Here I essentially follow the line of inquiry in the literature that considers path dependency in economic outcomes, especially the extent to which historical human capital shocks cast long shadows on latter-day economic performance, similar to effects found in other developing economies such as Peru and Bolivia.[11]

The primary contribution of this work is to provide a *causal* estimate of the effect of human capital on economic output, with identification that exploits the quasi-experimental setting of the politically-motivated persecution of educators during the Cultural Revolution. In addition, by documenting transmission mechanisms by which

[9]Robert M. Solow, "A Contribution to the Theory of Economic Growth", *Quarterly Journal of Economics*, 70: no. 1 (February 1956); N. Gregory Mankiw, David Romer and David N. Weil, "A Contribution to the Empirics of Economic Growth", *Quarterly Journal of Economics*, 107: no. 2 (May 1992).

[10]Walder and Su, 2003:, *op. cit.* (as in n. 3); Walder, 2014:, "Rebellion and Repression in China, 1966–1971", *op. cit.* (as in n. 1).

[11]Melissa Dell, "The Persistent Effects of Peru's Mining Mita", *Econometrica*, 78: no. 6 (November 2010); Nathan Nunn, "The Importance of History for Economic Development", *Annual Review of Economics*, 1: no. 1 (September 2009).

human capital shocks operate—whether these effects are more pronounced in agriculture, manufacturing, or services, and whether shocks to different degrees of educational attainment are more significant—I offer additional insight that can help mediate between important ongoing debates in the profession. For instance, establishing the importance of human capital for manufacturing vis-à-vis services output can clarify the role that education plays in economies undergoing premature deindustrialization.[12] As another example, the findings here can help distinguish between whether it is the accumulation of tertiary-level human capital that is key to improving productivity and growth,[13] or of it is the quality of educational inputs at the foundational level[14] that is central instead.

The results will also have implications for policymakers—especially those in developing countries with a significant deficit in educational attainment—seeking to understand the mediating effects of human capital on growth. A defensible causal estimate of the marginal contribution of changes in human capital to economic outcomes can inform policymakers that want a sense of the magnitude of this contribution for the purposes of formulating growth-inducing policy. By distinguishing between alternative channels by which such growth occurs, they can shift education policy between, for instance, one that targets greater access for the broad population base, versus one that directs more expenditure toward research and development or expands resources that support innovation in science and technology.

The rest of the thesis is organized as follows. In Section II, I define the main key

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[12]Dani Rodrik, "Premature Deindustrialization", *Journal of Economic Growth*, 21: no. 1 (March 2015).

[13]Anna A. Valero and John Van Reenen, "The Economic Impact of Universities: Evidence from Across the Globe", Working Paper 22501 (Cambridge, MA: National Bureau of Economic Research, 2016); Mara P. Squicciarini and Nico A. Voigtländer, "Human Capital and Industrialization: Evidence from the Age of Enlightenment", *Quarterly Journal of Economics*, 130: no. 4 (November 2015).

[14]Hanushek and Wößmann, 2012:, *op. cit.* (as in n. 7).

terms and concepts used in the study. This is followed, in Section III, by a discussion of the historical context—paying special attention to the political motivations behind the Cultural Revolution that gave rise to its anti-intellectual nature—as well as an overview of the related literature. I then discuss the methodological approach, including details concerning the data, identification, theoretical framework, and empirical specification (Section IV). Section V then reports the baseline results along with a suite of robustness checks. A penultimate section discusses a number of further explorations of the results, including transmission channels and legacy effects. The final section concludes with policy implications, limitations of the present study, and implied future research avenues.

II.

Definition of Terms

Cultural Revolution: Known formally as the "Great Proletarian Cultural Revolution," the Revolution was a sociopolitical movement initiated by Mao Zedong to preserve "true" Communist ideology and purge Chinese society from capitalistic elements. In practice, the movement sought to reinstate Mao's ideology as the dominant one within the Chinese Communist Party (CCP), and consolidate his power alongside the so-called "Gang of Four" officials. The official dates of the revolution fall between May 1966 and September 1976, although Mao declared it over in 1969, and its most active phase lasted until the death of military leader Lin Biao in 1971. The Revolution ushered in a period of persecution of class enemies, including teachers and intellectuals, and forgone educational opportunities as students dropped out of school (either to participate as Red Guards or due to the ensuing turmoil), or experienced discrimination in their university studies.

Endogeneity: The statistical problem of a correlation between the error term and a given explanatory variable of interest in a regression model. In the context of this study, endogeneity in the human capital measure results from the possibility of an additional omitted confounding variable, or from simultaneity (and in particular reverse causality).

Human capital: The stock of economically-relevant knowledge and skills embedded within a given economic agent, whether at the individual or provincial level. My proxy measure of human capital relies, as a benchmark, on average student enrollment aross institutions of education at the primary, secondary, and tertiary level.

Omitted variable bias: The bias to an estimate of the effect of human capital on economic outcomes that results from the failure to control for (typically unobserved) confounding variables. These variables may be those directly related to the measurement of human capital, such as individual cognitive ability or quality of educational service delivery, or to economic performance more generally, such as institutional governance or cultural norms, for which proxy measures are either imperfect or unavailable for inclusion as controls.

Ordinary least squares (OLS): The standard approach to regression analysis, which minimizes the sum of squared errors in a fit of a dependent variable on independent variables. OLS requires a number of assumptions, known as the Gauss-Markov conditions, be satisfied in order for the coefficient estimates to be unbiased. Consequently, an appropriate evaluation of the effects of human capital on output requires alternative approaches, other than OLS, which explicitly account for endogeneity.

Path dependency: The tendency for a given shock to propagate and persist across time, thereby generating contemporaneous outcomes that are crucially dependent on historical events. Common examples of path-dependency are the choice of the Qwerty versus Dvorak (the latter, despite being more ergonomically efficient, lost out to the former as it became entrenched following its early advantage in accommodating the slower mechanical movement of early typewriters), or the success of VHS versus Betamax. In the current context, path dependence can result from lost human capital during the Revolution as less-educated parents have a reduced ability to provide supplementary home education to their offspring, hence passing on lower levels of human capital accumulation to future generations.

Political persecutions and casualties: Total recorded number of either unnatural deaths or (non-fatal) victims of various forms of political persecution, transcribed from local annals, for the entire period between June 1966 through December 1971. These statistics of casualties are the result of internal investigations directly related to the Cultural Revolution. Throughout this thesis, I will rely on the term *political perse-*

cutions to refer to the *concept* of a politically-motivated human capital shock, while *political casualties* will be used to refer to the actual *data* of deaths and victims.

Province-year: The primary unit of analysis in this thesis, corresponding to one of 31 polities (provinces are the next level of political organization below the central government, similar to U.S. states) for various years during the Cultural Revolution (1966–76). For example, Fujian province in the year 1966 corresponds to one observation in the baseline analysis. Certain secondary analyses consider years before 1966 and after 1976, as well as more disaggregated data corresponding to counties (the next, lower level of political organization below provinces, similar to U.S. local governments such as counties or municipalities).

Real gross provincial product (GPP): The total value of final goods and services produced within a province's borders within a given period. Real GPP is obtained by deflating the nominal value of GPP by the general price index for the province. This measure is typically normalized by the size of the local labor force (total number of employed persons) to obtain real GPP per worker, or by the size of the provincial population, to obtain real GPP per capita.

Simultaneity: The bias to an estimate of the effect of human capital on economic outcomes that results from the codetermination of both variables. Of particular concern is reverse causality, since higher incomes can enable the greater acquisition of education, and hence bring about human capital accumulation, rather than just *vice versa.*

Simultaneous equation model: A statistical model comprising of multiple equations specifying relationships between variables. The simplest estimation technique associated with simultaneous equation modeling is that of instrumental variables (IV), which attempts to circumvent the problem of endogeneity in nonexperimental data by introducing an additional variable—known as an instrument—that is correlated with the endogeneous human capital variable (the *relevance condition*) and uncorrelated with the error term in the primary (second-stage) regression equation—of output on human capital—conditional on other covariates (the *exclusion restriction*). If additional variables are utilized in addition to the instrument, the estimation is performed with two-stage least squares (2SLS), with these variables included in the first-stage regression and the predicted values of the first stage used as instruments in the second-stage regression. If further controls are required that delineate additional relationships between the variables, estimation via three-stage least squares (3SLS)—or, more generally, seemingly unrelated regression (SUR)—allows for specifications with multiple auxiliary regressions, whose results then enter into the primary regression.

III.

Background on the Problem

III.I Historical Background

As with virtually all complex historical events, the origins of the Cultural Revolution are varied and disputed. Some Sinologists have stressed the international context that led up to the Revolution. After the death of Stalin in 1953, there was a perception that, under Khrushchev, the Soviet Union had begun to drift away from Marxist-Leninist ideology; this Sino-Soviet split precipitated an attempt by Mao to preempt a similar backslide within China by appealing to a return to Maoist thought.[15] This was further exacerbated by rising cross-strait tensions with Taiwan, which culminated in the 1958 Taiwan Strait crisis. To this end, the call to revolution was inspired, in part, by a desire to bolster support for the Chinese Communist Party (CCP) and extinguish any residual sympathies for the *Kuomindang* on the mainland.[16]

International dimensions aside, the more important instigators of the Revolution were to be found domestically. Following the failed Hundred Flowers Campaign in 1956—a brief period of liberalism where the CCP relaxed its hold on Maoist orthodoxy—Mao sought to flush out any remaining dissidents through further constraints on the intelligentsia.[17] The partial rollback of collectivist agricultural policy—implemented to counter the famine that resulted from the disastrous Great Leap Forward—also likely played a part. Mao had regarded these liberal policies as in-

[15] Sergey Radchenko, *Two Suns in the Heavens: The Sino-Soviet Struggle for Supremacy, 1962–1967* (Palo Alto, CA: Stanford University Press, 2009).

[16] Roderick L. MacFarquhar, *The Origins of the Cultural Revolution, Vol. II: The Great Leap Forward, 1958–1960* (New York, NY: Columbia University Press, 1983).

[17] Idem, *The Origins of the Cultural Revolution, Vol. I: Contradictions Among the People, 1956–1957* (New York, NY: Columbia University Press, 1974).

imical to Marxist ideology, and the Revolution was his response.[18] Perhaps most centrally, a power struggle had emerged at the top between Mao and his foreign-educated lieutenants—especially Liu Shaoqi, Deng Xiaoping, and to a lesser extent Zhou Enlai—all of whom Mao became increasingly suspicious of, with the former two ultimately even labeled as traitors to the cause. As Mao sought to consolidate his position within the Party and secure his legacy, initiating the Cultural Revolution served to sideline these potential challengers to his power; Liu and Deng, for example, were denounced as the foremost and second-most among the "capitalist roaders" (*zhouzipai*) when the Revolution began, while Zhou underwent political persecution in its later stages.[19]

Although the precise starting date of the Revolution is occasionally disputed,[20] it is fair to regard the directive issued by Mao on August 1966—the so-called "Sixteen Points"—as establishing the definitive contours within which the Revolution would follow. In particular, Mao proclaimed the overthrow of the educated bourgeoisie by the proletariat, who was to (*emphases* added)

> . . . struggle against and crush those persons in authority who are taking the capitalist road, to criticize and repudiate the reactionary *bourgeois academic* "authorities" . . . and *transform education*, literature, and art . . . to facilitate the consolidation and development of the socialist system. . . . The cultural revolutionary groups, committees, and congresses are excellent new forms of organization whereby under the leadership of the Party the

[18]Dikötter, 2010:, *op. cit.* (as in n. 2).

[19]Idem, *The Cultural Revolution: A People's History, 1962–1976* (London, England: Bloomsbury, 2016); Roderick L. MacFarquhar, *The Origins of the Cultural Revolution, Vol. III: The Coming of the Cataclysm, 1961–1966* (New York, NY: Columbia University Press, 1999).

[20]Ping Hao, "Reassessing the Starting Point of the Cultural Revolution", *China Review International*, 3: no. 1 (Spring 1996).

masses are *educating themselves*.[21]

Youth all over China left their classrooms to respond to Mao's appeal, forming bands of Red Guards (Figure 1(a)) that sought to identify and persecute class enemies.[22] Mao left little doubt over who the vigilantes were to target: they were to "rebel against the reactionaries… [such as] the teachers, students, administration, and workers of Peking University… or anywhere in China."[23] Their goal was "to delete, to abandon" traditional teaching materials; Mao even sanctioned the reduction of "incorrect or repetitious" materials "by one third to one half."[24]

In addition to the public purging of Liu, Deng, and Zhou, a host of other intelligentsia—as many as a third—were also removed from their administrative positions.[25] But high-profile intellectuals were not the only target. All over China, teachers and professors came to be derided as the "stinking old ninth" (*choulaojiu*) class, and big-character posters appeared across campuses (Figure 1(b)), condemning teachers for perpetuat-

[21]Mao Zedong, "The *Sixteen Points*: Guidelines for the Great Proletarian Cultural Revolution (1966)", in William Theodore de Bary and Richard J. Lufrano (eds.), *Sources of Chinese Tradition: From 1600 Through the Twentieth Century* (New York, NY: Columbia University Press, 2000) pp. 474–475, *emphases* added.

[22]Andrew G. Walder, *Fractured Rebellion: The Beijing Red Guard Movement* (Cambridge, MA: Harvard University Press, 2009).

[23]Mao Zedong, "A Letter to the Red Guards of Tsinghua University Middle School", in *Selected Works of Mao Tse-Tung* (Hyderabad, India: Sramikavarga Prachuranalu, 1994a) p. 276.

[24]Idem, "Speech at a Meeting with Regional Secretaries and Members of the Cultural Revolutionary Group of the Central Committee", in *Selected Works of Mao Tse-Tung* (Hyderabad, India: Sramikavarga Prachuranalu, 1994b) p. 273. The first major campaign of the Revolution was the "Destruction of Four Olds," which sought to eliminate customs, culture, habits, and ideas deemed insufficiently Maoist. However, the specific customs, cultures, habits, and ideas that were to be replaced were never clearly defined. See Xing Lu, *Rhetoric of the Chinese Cultural Revolution: The Impact on Chinese Thought, Culture, and Communication* (Columbia, SC: University of South Carolina Press, 2004).

[25]Richard K. Diao, "The Impact of the Cultural Revolution on China's Economic Elite", *China Quarterly*, 42: no. 1 (April 1970) p. 66.

ing bourgeois, revisionist curricula.[26] In the months of August and September 1966, teachers were subjected to large-scale violence, with between a quarter to a third of middle schools experiencing one teacher killing.[27] Those that survived were often relegated to hard labor in the countryside,[28] and those who could not endure the physical and mental trauma were often led to take their own lives.[29] This violence was, at the least, condoned by the Party—viewing it as "acceptable collateral damage" to which the government should "turn a blind eye. . . as an inevitable by-product"[30]—and, at worst, encouraged by the regime's praise of their actions as "valiant" and worthy of emulation as "national models."[31]

This persecution of teachers and the educated class persisted through much of the Revolution. Although the objective of the 1968–69 "Cleansing of Class Ranks" (*qingli jieji duiwu*) campaign was ostensibly to remove all manner of "bad people. . . [that] have come out of the sky and wormed their way into [the] ranks,"[32] in reality, it was typically teachers and other intellectuals that became the primary target. Such persecution resulted in many deaths that resulted directly from beatings and fatal torture; the humiliation from public struggle sessions subsequently led many more to suicide.[33] Similarly, while the official objective of the 1970–72 "One Strike, Three Anti" (*yida*

[26]MacFarquhar and Schoenhals, 2008:, *op. cit.* (as in n. 4).

[27]Wang, 1997b:, *op. cit.* (as in n. 4).

[28]MacFarquhar and Schoenhals, 2008:, *op. cit.* (as in n. 4).

[29]Wang, 1997b:, *op. cit.* (as in n. 4).

[30]Walder, 2009:, *op. cit.* (as in n. 22) pp. 148–149.

[31]Yongyi Song, "Chronology of Mass Killings During the Chinese Cultural Revolution (1966–1976)", in Claire Andrieu (ed.), *Online Encyclopedia of Mass Violence* (Paris, France: SciencesPo, 2011) p. 4.

[32]MacFarquhar and Schoenhals, 2008:, *op. cit.* (as in n. 4) p. 253.

[33]Wang, 1997a:, *op. cit.* (as in n. 4).

(a) Red Guards (b) Big-character posters

Figure 1: Mao Zedong and Lin Biao with Red Guards (left panel) and students posting big character posters (right panel). Red Guards were usually constituted of students, who wore red armbands and often carried the little red book of *Quotations from Chairman Mao*. The first group emerged out of Tsinghua University Middle School, and criticized the administrations of Tsinghua and Peking universities. Big-character posters appeared across Chinese academic institutions, and ranged from a few sentences to 10,000-character treatises. They typically featured criticisms of teachers, who were held responsible for teaching non-revolutionary ideals.

sanfan) campaign was to punish graft, embezzlement, profiteering, and extravagance, it was extended to include university students guilty of committing "counterrevolutionary" offenses,[34] as well as those who had criticized the Revolution,[35] many of whom—akin to the aborted Hundred Flowers campaign—were educated members of society.

This anti-intellectual purge froze schools and universities across the country. All urban primary schools were shut between 1966 and 1968,[36] and most secondary and tertiary-level institutions also suspended operations. Universities remained closed at

[34] MacFarquhar and Schoenhals, 2008:, *op. cit.* (as in n. 4).

[35] Song, 2011:, *op. cit.* (as in n. 31).

[36] Junsen Zhang, Pak-Wai Liu and Linda C.W. Yung, "The Cultural Revolution and Returns to Schooling in China: Estimates Based on Twins", *Journal of Development Economics*, 84: no. 2 (November 2007).

least until 1972, and college entrace exams (*gaokao*) were reinstated only as late as 1977.[37] Control of the State Science and Technology Commission was wrested away from intellectuals and handed over to untrained revolutionaries, and many teaching and research institutions were either suspended or dissolved entirely.[38] As the knowledge infrastructure ground to a halt, an entire generation experienced severe disruptions to their education, ranging from missed years of schooling, to schooling completion without a normal curriculum, to delayed or denied university access,[39] especially for children deemed as belonging to "bad" class origins.[40] Perhaps more damaging was the fact that the worker-run schools that displaced the traditional ones were dedicated not to advancing human capital accumulation, but to fostering revolutionary values;[41] students were generally assessed on the basis of political virtue, rather than academic achievement.[42]

The overall reach of the Cultural Revolution was extensive. Few students remained untouched by the movement; one estimate suggests that the Revolution affected more than half a million students across 434 universities, 6.4 million students in 56,000

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[37] Thomas P. Bernstein, *Up to the Mountains and Down to the Villages: The Transfer of Youth from Urban to Rural China* (New Haven, CT: Yale University Press, 1977).

[38] Cong Cao, "Science Imperiled: Intellectuals and the Cultural Revolution", in Nancy Chunjuan Wei and Darryl E. Brock (eds.), *Mr. Science and Chairman Mao's Cultural Revolution: Science and Technology in Modern China* (Lanham, MD: Lexington Books, 2013).

[39] Meng and Gregory, 2002:, *op. cit.* (as in n. 5).

[40] The "bad class origin" label applied to students from families of former capitalists, intellectuals, pre-liberation landlords or rich peasants, and criminals. Such students received lower weights for the purpose of university admission; in contrast, children of revolutionary cadres, soldiers, or martyrs, as well as those of the working class, received favorable rankings. See Jonathan Unger, *Education Under Mao: Class and Competition in Canton Schools, 1960-1980* (New York, NY: Columbia University Press, 1982), *op. cit.* (as in n. 6).

[41] Ellen K. Ong, "Education in China Since the Cultural Revolution", *Studies in Comparative Communism*, 3: no. 3–4 (July–October 1970).

[42] Shirk, 1982:, *op. cit.* (as in n. 6).

high schools, and 100 million students in China's million-strong primary schools.[43] Many students, bereft of school, became politically socialized into joining the Red Guard.[44] Factions quickly developed, and as inter-faction conflict erupted, the return to educational normalcy was delayed further. Yet as fighting intensified, the factions themselves became confused by the escalating violence. One former Guard confesses that he "could not sort out the logic of the situation."[45] Another recalls the factions' conflicting objectives, with one "huge faction cooperating with the army" and another wanting instead "to seize power from the military."[46] Inter-factional warfare resulted in massive upheaval across the county, threatening central control.

When Mao did eventually regain political control, he did so by implementing a large-scale program of rural rustication, which required one in every three urban adolescents to be sent to the countryside.[47] Although the banishment of a number of these restless elements did restore certain order to the urban areas, the policy simply shifted much of the Cultural Revolution to rural China. Starting in 1968, the Revolution's momentum steadily built up in the rural regions, until it met or even surpassed the violence in urban areas, with as many as 36 million Chinese undergoing some form of political persecution.[48] The military was eventually sent in to stabilize public

[43] Julia C-S. Kwong, *Cultural Revolution in China's Schools, May 1966–April 1969* (Stanford, CA: Hoover Institution Press, 1988).

[44] Anita K-W. Chan, *Children of Mao: Personality Development and Political Activism in the Red Guard Generation* (Seattle, WA: University of Washington Press, 1985).

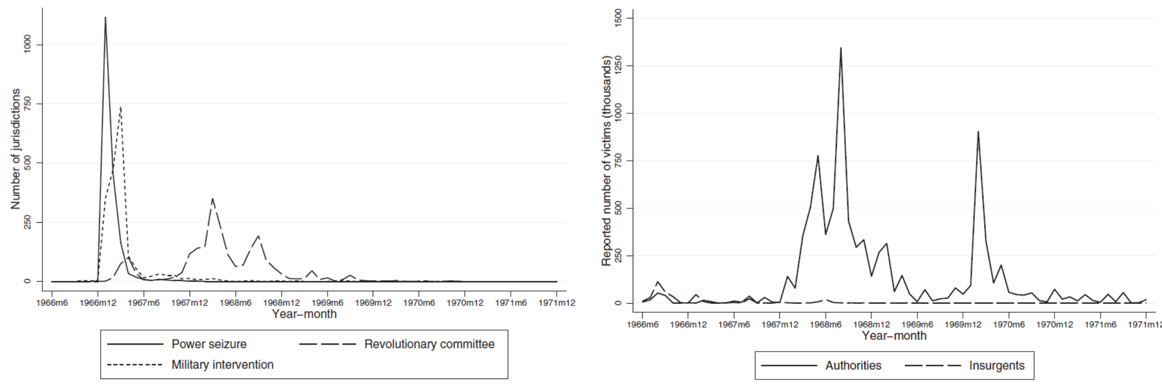
[45] Yuan Gao, *Born Red: A Chronicle of the Cultural Revolution* (Palo Alto, CA: Stanford University Press, 1987) p. 208.

[46] Liang Heng and Judith Shapiro, *Son of the Revolution* (New York, NY: Vintage Books, 1984) p. 132.

[47] Hongbin Li, Mark R. Rosenzweig and Junsen Zhang, "Altruism, Favoritism, and Guilt in the Allocation of Family Resources: Sophie's Choice in Mao's Mass Send-Down Movement", *Journal of Political Economy*, 118: no. 2 (February 2010).

[48] Walder and Su, 2003:, *op. cit.* (as in n. 3).

order following power seizures and clashes over revolutionary committee formation (Figure 2(a)), but skirmishes between authorities and student insurgents resulted in political casualties until as late as 1971 and beyond (Figure 2(b)).[49]



(a) Events

(b) Casualties

Figure 2: Temporal distribution of political events (left panel), by event type, and aggregate reported political casualties, by perpetrator (right panel). Events tended, on average, to follow the seizure-intervention-committee formation pattern, and the two major peaks correspond, respectively, to the initial "Destruction of Four Olds" and related military intervention to stabilize public order, and the end of popular insurgency. The two major peaks for authority-generated casualties correspond to the "Cleansing of Class Ranks" campaign that was initiated after the initial establishment of revolutionary committees in 1967, and the "One Strike, Three Anti" campaign in 1970. The spikes due to insurgent activity are much smaller, and correspond to rebel attacks on authorities in the 1966, 1967, and 1968. Original figures from Idem, 2014:, "Rebellion and Repression in China, 1966–1971", *op. cit.* (as in n. 1), 520, 523.

One common theme that resonates across this capsule history of the period is that traditional, non-Marxist education suffered significantly as a result of the Revolution. The effects were experienced at both the top and bottom ends of the educational ladder. At the top, teachers, professors, and other intellectuals were purged from their positions, and many suffered intense political persecution over the decade. At the bottom, the closure of schools at all levels over much of 1966–1971, coupled with lost schooling opportunities and disruptions due to Red Guard vigilantism, meant

[49] Although it is clearly possible to disaggregate the data temporally, the currently publicly-available dataset does not afford such a temporal breakdown, and so the analyses that follow rely casualties aggregated across the full 1967–71 period.

that students experienced a significant setback in their educational career. Moreover, this destruction of human capital was explicitly political in motivation, condoned—and perhaps even encouraged—by the Party. Overall, the outright destruction, compounded by the opportunity cost of lost human capital, was probably the most significant negative economic shock that resulted from the Cultural Revolution.

III.II Related Literature

Even after Mao's death in 1976, the reluctance to engage in any direct criticism of Mao and his ideas typically precluded any reliable, systematic study of the period. This was further compounded by the absence of data, which was likewise not readily available due to perceived political sensitivities. One study[50] of trends in China's economy in the immediate aftermath of the Revolution, for instance, relies primarily on anecdotal evidence and limited statistical records to explore local economic activity and inter-province trade. More data-driven studies are often hampered by data constraints. For example, much of the discussion in another study[51] simply documents the various pre- and post-Revolution policy debates, and the analysis of the short-run economic impact is based on externally-imputed estimates of key macroeconomic variables, rather than official Chinese data. Even more comprehensive, careful analyses[52] of the economic losses accruing to the Great Leap Forward and Cultural Revolution have been limited

[50] Audrey Donnithorne, "China's Cellular Economy: Some Economic Trends since the Cultural Revolution", *China Quarterly*, 52: no. 1 (Oct–Dec 1972).

[51] Robert F. Dernberger, "Radical Ideology and Economic Development in China: The Cultural Revolution and Its Impact on the Economy", *Asian Survey*, 12: no. 12 (December 1972).

[52] Gregory C. Chow, "Capital Formation and Economic Growth in China", *Quarterly Journal of Economics*, 108: no. 3 (August 1993).

to just a handful of national-level economic sectors and the aggregate economy.[53]

Beginning in the late 1980s, however, the publication of local annals and other Cultural Revolution-era documents has enabled researchers to study the period in a more systematic fashion.[54] These primary documents, along with the digitization of long-dated regional and sub-regional statistics by the National Bureau of Statistics, has seen the rise of a small cottage industry of papers that quantitatively examine various economic consequences of the period, along with the longer-run effect of this shock on contemporaneous economic outcomes.

Understandably, most of these papers focus on educational issues. One early study[55] relies on census data to analyze how the Cultural Revolution reduced the advantages that accrued to children from an educated family. Although the massive state intervention in favor of educational equality was positive for access initially, the authors also find that this effect rapidly diminishes and returns to the secular trend within seven years. Perhaps more damning is the fact that these interruptions were detrimental to educational attainment, overall. For example, one estimate[56] suggests that the Revolution lowered high school and college completion rates by 7 and 6 percentage points, respectively, while another[57] finds that the probability of university degree attainment fell by 55 percent—mainly as a result of lost secondary education—

[53]The results of this study point to fairly small economic effects due to the Revolution; deviations of sectoral output between 1966 and 1976 range from -0.3 percent (industry in 1968) to +0.14 percent (construction in 1970).

[54]Song, Yongyi (ed.), *The Chinese Cultural Revolution Database* (Hong Kong, China: Chinese University of Hong Kong, 2006); Eduard B. Vermeer, "New County Histories: A Research Note on Their Compilation and Value", *Modern China*, 18: no. 4 (October 1992).

[55]Deng and Treiman, 1997:, *op. cit.* (as in n. 5).

[56]John T. Giles, Albert F. Park and Meiyan Wang, "The Great Proletarian Cultural Revolution, Disruptions to Education, and Returns to Schooling in Urban China", HKUST IEMS Working Paper 2015-21 (Hong Kong, China: Hong Kong University of Science and Technology, 2015).

[57]Meng and Gregory, 2002:, *op. cit.* (as in n. 5).

due to the Revolution.[58] The effects are also long-lasting: one study[59] reports that the interrupted schooling attributable to the Revolution has required adult investment in continuing education, and in some cases was responsible for inducing a low-education trap.

The negative shock from the Revolution also affected more than just quantity outcomes. Three studies point to significant *positive* estimates for returns to education, although the magnitudes of estimates differ. For instance, one analysis[60] reports IV estimates that suggest Mincerian returns of around 8 percent, while another,[61] focusing on rural post-primary education, establishes a positive return of as high as 20 percent. A third paper[62] that relies on twins data demonstrates that Revolution cohorts achieve only modestly higher returns (of between 1 and 1.4 percent) compared to non-Revolution cohorts.[63] One explanation for higher returns is that the shortage of skilled labor, resulting from the Revolution, may have pushed up the education premium.

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[58] Since this approach effectively constructs a counterfactual, it is able to reveal possible losses that are obscured in analyses that rely on difference-in-differences approaches, such as that of Zhong Deng and Donald J. Treiman, "The Impact of the Cultural Revolution on Trends in Educational Attainment in the People's Republic of China", *American Journal of Sociology*, 103: no. 2 (September 1997), *op. cit.* (as in n. 5).

[59] Jun Han, Wing C. Suen and Junsen Zhang, "Picking Up the Losses: The Impact of the Cultural Revolution on Human Capital Reinvestment in Urban China", Economics department working paper (Hong Kong, China: Chinese University of Hong Kong, 2016).

[60] Giles et al., 2015:, *op. cit.* (as in n. 56).

[61] Terry Sicular and Juan Yang, "The Returns to Schooling in Rural China: Evidence from the Cultural Revolution Education Expansion", CIBC Working Paper 2015-2 (London, ON: University of Western Ontario, 2015).

[62] Zhang et al., 2007:, *op. cit.* (as in n. 36).

[63] It is important to keep in mind—for the first of these two papers especially—that these analyses are pure cohort studies, and so it is generally difficult to ascertain the counterfactual of exactly how different returns *would have been* in the absence of the Revolution. Indeed, the more appropriate comparison of inter-cohort twins embedded in the final paper suggests that the difference, while still positive, is generally small.

Other papers that go beyond educational outcomes are decidedly more discouraging. One study[64] calculates, on the basis of cohort data, that the Revolution had a nontrivial negative impact on average annual earnings: around -11 percent, with the losses mainly transmitted through the forced rustication channel. This mechanism has also been found to exert psychological costs, such as increased guilt in parents that chose rustification for a particular child,[65] or a shaken beliefs about the efficacy of individual effort.[66] Other studies[67] also identify health costs resulting from the Revolution: the former documents elevated risks of miscarriage and stillbirth during the period, and the latter finds significant losses to child health, as measured by height.

What is common across all this body of work is that, while they explore various aspects of human capital development, they do not go on to examine the effects of these changes in human capital on the broader economy. To my knowledge, only one other study[68] treats the Cultural Revolution as a natural experiment *and* considers the broader macroeconomic implications of the event. However, the study is concerned more with the effect of conflict-related disruptions due to the Revolution, rather than the human capital channel, which I regard—for reasons raised earlier—as far more consequential. Consequently, I regard the systematic exploration of the causal effects of human capital on economic performance at the cross-provincial level, using the

[64]Dong Zhou, "The Long-term Impacts of the Cultural Revolution: A Micro-Analysis", *Labour*, 30: no. 3 (September 2016).

[65]Li et al., 2010:, *op. cit.* (as in n. 47).

[66]Gerard Roland and David C. Yang, "China's Lost Generation: Changes in Beliefs and their Intergenerational Transmission", NBER Working Paper 23441 (Cambridge, MA: National Bureau of Economic Research, 2017).

[67]Yong Cai and Wang Feng, "Famine, Social Disruption, and involuntary Fetal Loss: Evidence from Chinese Survey Data", *Demography*, 42: no. 2 (May 2005); Qihui Chen, "Three Essays on Human Capital Investment in China", Unpublished phd dissertation, University of Minnesota (Minneapolis, MN: University of Minnesota, 2012).

[68]Liang Bai and Lingwei Wu, "Economic Legacies of the Cultural Revolution" (2017).

Revolution as a natural experiment, as important contribution to the extant literature.

To the (limited) extent that I examine the long-run effects of historical phenomena, this thesis also speaks to a growing literature that explores the channels by which history can give rise to path-dependent outcomes. A large literature emphasizes how cross-country economic performance may be causally determined by key historical factors,[69] especially operating through the medium of micro- or macro-level institutions.[70] More recently, a second generation of papers has sought to clarify precisely *how* historical persistence matters, through a better understanding of the relevant channels of transmission[71]. These papers have gone on to examine the contingency of modern economic outcomes to historical events, such as slavery[72] and colonization.[73] More specific to China, a pair of papers have explored legacy effects, in terms of educational

[69] K. Daron Acemoğlu, Simon Johnson and James A. Robinson, "Institutions as a Fundamental Cause of Long-Run Economic Growth", in Philippe M. Aghion and Steven N. Durlauf (eds.), *Handbook of Economic Growth*, volume 1 (Amsterdam, The Netherlands: Elsevier, 2005); Simeon Djankov et al., "The New Comparative Economics", *Journal of Comparative Economics*, 31: no. 4 (December 2003).

[70] By micro-level institutions, I mean mechanisms such as the legal framework governing corporate finance or those regulating the entry of new firms that can influence economic behavior. See Rafael La Porta et al., "Law and Finance", *Journal of Political Economy*, 106: no. 6 (December 1998) and Simeon Djankov et al., "The Regulation Of Entry", *Quarterly Journal of Economics*, 117: no. 1 (February 2001), respectively. Macro-level institutions are generally the formal and informal rules in place, such as political stability or democratic voice and accountability. See Idem, "The New Comparative Economics", *Journal of Comparative Economics*, 31: no. 4 (December 2003), *op. cit.* (as in n. 69) and K. Daron Acemoğlu, Simon Johnson and James A. Robinson, "Institutions as a Fundamental Cause of Long-Run Economic Growth", in Philippe M. Aghion and Steven N. Durlauf (eds.), *Handbook of Economic Growth*, volume 1 (Amsterdam, The Netherlands: Elsevier, 2005), *op. cit.* (as in n. 69) (and the references therein) for in-depth discussions of each respective aspect of institutions.

[71] Nunn, 2009:, "The Importance of History for Economic Development", *op. cit.* (as in n. 11).

[72] Idem, "The Long-Term Effects of Africa's Slave Trades", *Quarterly Journal of Economics*, 123: no. 1 (February 2008); Nathan Nunn and Leonard Wantchekon, "The Slave Trade and the Origins of Mistrust in Africa", *American Economic Review*, 101: no. 7 (December 2011).

[73] K. Daron Acemoğlu and Simon Johnson, "Unbundling Institutions", *Journal of Political Economy*, 113: no. 5 (October 2005); Dell, 2010:, *op. cit.* (as in n. 11); Lakshmi Iyer, "Direct versus Indirect Colonial Rule in India: Long-Term Consequences", *Review of Economics and Statistics*, 92: no. 4 (November 2010).

outcomes of children born to Revolutionary parents,[74] and on the rate of industrialization post-Revolution.[75] The present study adds to this literature, by tracing the persistence of historical human capital shocks on per capita incomes.

[74]Xin Meng and Guochang Zhao, "The Long Shadow of the Chinese Cultural Revolution: The Intergenerational Transmission of Education", IZA Discussion Paper 10460 (Bonn, Germany: Institute of Labor Economics, 2016).

[75]Bai and Wu, 2017:, *op. cit.* (as in n. 68).

IV.

Research Methodology

IV.I Data Sources and Description

Most of the economic data are drawn from the annual yearbooks of the *China Yearly Provincial Macro-economy Statistics* and *China County Statistics*, compiled by the National Bureau of Statistics of China. These yearbooks provide comprehensive official statistics on economic and social development indicators for the 31 provinces, autonomous regions, and province-level municipalities of mainland China (which we refer to simply as provinces), as well slightly more limited socioeconomic data for its over 2,000 counties and county-level cities (which we refer to uniformly as counties). The data are available since 1951 and run through 2014, but most of the historical analysis will rely on statistics for the period just prior to the Revolution and immediately after. The unit of analysis in the baseline, and most robustness checks, is repeated cross-sections of provinces for various years between 1966 and 1976. Since more disaggregated data are available since 1998, I also draw on analogous indicators from 2005 and 2012, which provides two additional cross-sections at the county level for the purposes of additional discussion.

For the baseline regressions, I utilize real gross provincial product (GPP) per worker as my main dependent variable, obtained by deflating the nominal value of GPP by the general retail price index for the province; this is then normalized by the size of the labor force. Alternative measures of output I consider are GPP measured using the expenditure (instead of production) method, provincial consumption per worker.

My main independent variable of interest is the human capital stock, which I proxy with average student enrollment in institutions of education at the primary, secondary, and tertiary level, in the given year. Since enrollment is a flow variable—and may not capture contemporaneous changes to the stock of human capital, which, in principle, is what enters the production function—I also consider the average number of full-time teachers in primary and secondary schools, which may better capture (albeit still imprecisely) the stock characteristic of this variable.[76]

The political casualties data were drawn from local annals (*difangzi*), and comprise 2,213 distinct jurisdictions, corresponding to the county level. The dataset is part of a larger, two-decade effort by two teams of researchers that manually coded relevant data and metadata for the period June 1966 to December 1971; the full database is described in detail in an interim report[77] and a later paper,[78] among others. Here I restrict the discussion to a summary of the key features of the original source and its compilation, as well as a number of stylized facts; the interested reader is referred to the cited papers for additional details.

The annals are local chronicles, numbering between 500 (for rural counties) to more than 1,000 (for medium-sized cities) pages of material. These annals record a variety of historical information, ranging from chronologies of major events, to local economy surveys, to the identities of prominent families. Crucially for the purposes of data integrity, annals cover similar topics and follow a relatively standard format (albeit with some provincial variation). Annals dating to the Cultural Revolution period also include separate sections on political affairs, which compile statistics resulting from internal investigations conducted following the death of Chairman Mao.

The coding process was performed by two teams of coders, who transcribed summary data on the number of casualties that resulted from political activities and details

[76]It is important to note that while both these proxies are indisputably measured with error, the IV approach affords some flexibility in this regard; the independent variable (whether enrollment or teacher count) can be measured with error, so long as the instrument satisfies a number of conditions. This is discussed in greater detail in subsection IV.II.

[77]Walder and Su, 2003:, *op. cit.* (as in n. 3).

[78]Walder, 2014:, "Rebellion and Repression in China, 1966–1971", *op. cit.* (as in n. 1).

on specific political events. The former comprises "the number of 'unnatural' deaths and the number of other 'victims' [from] various forms of political persecution,"[79] and the latter includes event type, dates, and number of deaths and injuries generated by the event in question. The rules for coding casualties was conservative, so unless a specific numerical figure was provided in the text, casualties were recorded as zero (rather than missing). Errors were minimized by designating two teams that independently undertook the coding process, with unmatched instances reconciled by referencing the original sources.

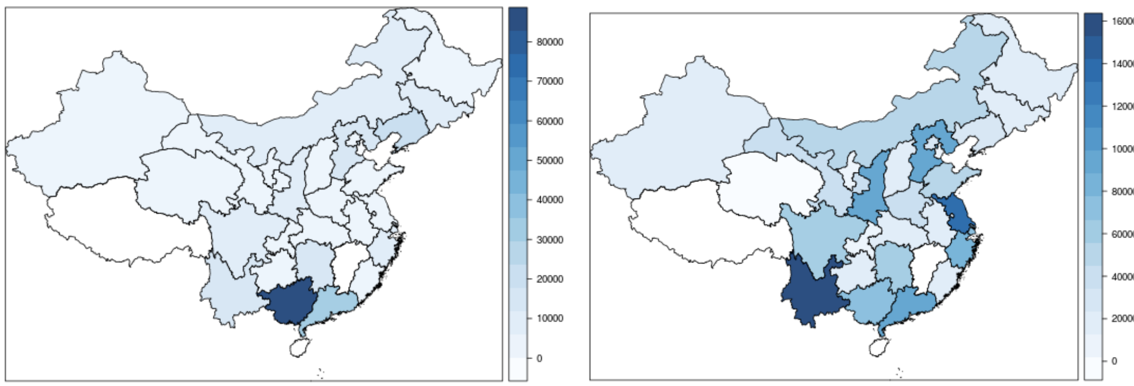
The data are currently only available as a temporally-aggregated total, by county, across the full 1966–71 period. For the baseline, the county data are further aggregated into provinces to derive the instruments used in the baseline. Figure 3 maps the distribution of the provincial-level aggregates. It is evident that the distribution of deaths is less varied than that of victims, as might be expected given the far larger number of victims overall. Even so, by either metric, political casualties were largely concentrated in the coastal southeast and northeast regions, although Yunnan (in the country's extreme southwest) also experienced a disproportionate number of victims.

It is important to recognize that the reported jurisdictions only cover a subsample of the total,[80] and are likely to be represent an incomplete count of all casualties.[81] This presents two potential problems. First, while the casualties data cover virtually all jurisdictions (2,213 of the 2,223), successful matching with with relevant covariates subsequently reduced this to a working sample of between 1,583 and 1,814. Visual inspection of the unmatched observations confirmed that there was no systematic

[79] Walder, 2014:, "Rebellion and Repression in China, 1966–1971", *op. cit.* (as in n. 1) p. 516.

[80] Walder, 2014:, "Rebellion and Repression in China, 1966–1971", *op. cit.* (as in n. 1).

[81] Walder and Su, 2003:, *op. cit.* (as in n. 3).



(a) Deaths (b) Victims

Figure 3: Distribution of political persecutions resulting in death (left panel) and counting all victims (right panel). There is much greater variation in the distribution of victims (relative to deaths), but by either metric, political casualties were concentrated in the coastal southeast and northeast regions.

(observable) pattern in the unmatched data.[82]

A second concern is the possibility of censored data. The compilers of the annals faced a tradeoff between an honest and accurate record of local events, against the risk that doing so could lead to revelations of a politically-sensitive nature. National guidelines promulgated in 1985 specified that Revolution-era accounts were to adhere to the principle of "broad strokes, not in detail,"[83] although the broad latitude exercised by local authorities in complying with the national directive meant significant variability in practice, which implies that the casualties data are almost certainly part of a larger (unobserved) population, and hence underreport total casualties.[84]

[82]Indeed, most of the discrepancy between the two stems from either subsequent mergers in jurisdictions since 1971 (the significant minority), or truly missing county-level statistics (the majority). To the extent that the former implies an undercounting of political casualties, the result will be biased against my null hypothesis of no effect. The issue of systematic differences in missing versus available county statistics is more worrisome, but is a fundamental constraint of data availability and cannot be resolved.

[83]Walder and Su, 2003:, *op. cit.* (as in n. 3) p. 80.

[84]The average length of accounts across all provinces for which data were available was 4,066 characters, and ranged from a high of 10,689 characters (in Shaanxi) to a low of 2,089 (in Zhejiang), with data only unavailable for Tibet. See Ibid. pp. 81, Table 1. Again, this underreporting works against my null hypothesis.

The other independent variables and controls I introduce are relatively standard, and the discussion of these, along with their sources, is relegated to the appendix.

IV.II Identification Strategy

The central empirical challenge in this thesis is the successful identification of the effects of human capital on output, given the endogeneity of human capital. While there is a strong theoretical basis for how human capital can give rise to output gains in either neoclassical[85] and endogenous[86] growth models, there is also the possibility of reverse causality, either through a discount rate channel,[87] or more generally through improvements in human development.[88] This simultaneity suggests that naïve attempts to retrieve the contribution of human capital will almost certainly encounter bias in their estimates. My approach toward resolving this issue appeals to the use of political casualties in the instrument set to help identify the effect of human capital.

Doing so, however, is accompanied by its own set of caveats and concerns. As noted earlier, the casualty data are likely to be censored. For my purposes, however, the fact that the instrument is measured with noise is not entirely detrimental; what is most

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[85]Robert E. Lucas, "On the Mechanics of Economic Development", *Journal of Monetary Economics*, 22: no. 1 (July 1988).

[86]Paul M. Romer, "Endogenous Technological Change", *Journal of Political Economy*, 98: no. 5 (October 1990).

[87]Mark Bils and Peter J. Klenow, "Does Schooling Cause Growth?", *American Economic Review*, 90: no. 5 (December 2000).

[88]Tavneet Suri et al., "Paths to Success: The Relationship Between Human Development and Economic Growth", *World Development*, 39: no. 4 (April 2011). The discount rate channel operates by inducing greater demand for schooling when there is anticipated growth: higher future output raises the wage profile of future earnings, thereby reducing the effective discount rate; agents therefore become willing to sacrifice current earnings by taking on more education. In contrast, the more general human development channel is based on the premise that since improved human flourishing is the ultimate goal of development, higher income levels permit the purchase of greater amounts of education.

important for instrument validity is simply that the instrument (or instrument set) is sufficiently correlated with human capital (the relevance condition), while remaining uncorrelated with the error term in the regression of income on human capital (the exclusion restriction).[89]

The underreporting of recorded deaths results from variations in province-level editorial policy as well as potential resource constraints.[90] Consequently, measurement error in this variable is unlikely to be correlated with either the true value of the measure of interest (human capital) or the error term in the human capital equation. It may, however, be correlated to the error term in the main income equation: small, resource-constrained counties may systematically report lower deaths due to limited resources or idiosyncratic provincial recordation practices, rather than politically-induced censorship.

To circumvent this issue, a full specification of the human capital equation would need to include a measure of resource constraints that could affect the accumulation of human capital,[91] and controls in the main income equation should also attempt to control for observable factors that plausibly affect output. In what follows, I do both.

That said, there is little evidence that such constraints matter, insofar as variations in political persecutions are concerned. For instance, the highest incidence of casualties occurred in Guanxi; this is a mid-sized province with average levels of GDP and other standard economic performance measures. A similar argument can be made for

[89]More formally, for a given relationship $Y = \pi_0 + \pi_1 H^* + \nu$ with a noisily-measured variable $H = H^* + \varepsilon$ of the true (unobserved) measure H^* and i.i.d. error ε, the political persecution instrument P is valid for $P = H^* + \upsilon$ (where υ is the i.i.d. error in the instrument), so long as it satisfies the the conditions that $\text{cov}\,(\upsilon, H^*) = 0$, $\text{cov}\,(\upsilon, \varepsilon) = 0$, and $\text{cov}\,(\upsilon, \nu) = 0$, along with the relevance condition, $\text{cov}\,(P, H) > 0$ and the exclusion restriction, $\text{cov}\,(P, \nu) = 0$.

[90]Walder, 2014:, "Rebellion and Repression in China, 1966–1971", *op. cit.* (as in n. 1).

[91]This reliance on resource availability in the first stage is also is the tack employed by ibid., although in that case resources are used as an exogenous source of selection into a Heckman-type model to evaluate the extent of censorship in the reporting of casualties.

Qinghai, a much less populous province, but with a comparable level of income per capita. Even the above-median persecutions in more industrialized provinces (such as Jiangsu and Shanghai) appear to be offset by similarly large casualty counts in less-industrialized ones (such as Gansu and Inner Mongolia), reinforcing the notion that political persecutions were little related to pre-Revolution measures of economic performance.[92] Finally, it is worth noting that *even if* bias is introduced due to resource constraints, the undercounting of political casualties will work *against* the null hypothesis of no effect of human capital, which is generally preferable to the opposite circumstance.

Censorship concerns aside, does the measure otherwise satisfy the conditions for relevance and exclusion? I argue that it does. For relevance, recall that a significant majority of political casualties can be tied directly to various political campaigns, the objective of which was to remove class enemies, especially educated individuals. For example, casualties attributable to the "Cleansing of Class Ranks" were often teachers, while that of the "One Strike" campaign were university students. The link between lost human capital and casualties, therefore, is evident from the historical narrative.

But one can go beyond narrative arguments to statistically verify instrument relevance. A necessary and sufficient condition for relevance is that the matrix of casualty instruments and endogenous human capital measure attains full rank (the human capital equation is not underidentified). The $rk\ LM$ statistic[93] is one such test that is valid in the presence of heterskedasticity-robust standard errors. In the results that follow, I report this underidentification test for all instances where it is applicable.

Incidentally, it is worth noting that satisfying the relevance condition also means

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[92] More formally, the pairwise correlation between casualties and different measures of per-worker income range from $[-0.027, -0.041]$, and are statistically insignificant at conventional levels. The relationship is somewhat stronger vis-à-vis government size, but still small ($\rho = 0.153$).

[93] Frank R. Kleibergen and Richard Paap, "Generalized Reduced Rank Tests Using the Singular Value Decomposition", *Journal of Econometrics*, 133: no. 1 (July 2006).

that measurement error in the *independent* variable is not an issue, and indeed an IV approach is often recommended as a resolution strategy for dealing with attenuation bias due to measurement errors of this nature. This is especially worth noting since, as mentioned in the previous subsection, the human capital variable is almost certainly measured with error.

Now, even if not underidentified, a given instrument set may be weak. A typical rule of thumb for ensuring instrument strength is to ensure that the first-stage F exceeds 10. A more formal weak identification test compares the rk Wald F to the maximum tolerable size of distortion (to the test) that may be introduced by weak instruments. To this effect, I report critical values[94] corresponding to a maximal distortion of 25 percent.

While it is impossible to definitively ascertain that the exclusion restriction is met, the historical narrative lends credence to this condition being satisfied. As discussed in Section III, the effects of Revolutionary political action operated almost exclusively through the human capital channel. Furthermore, as discussed earlier, there is little evidence that suggests that the magnitude of political persecution would be related to other aspects of economic performance.

However, there may be a legitimate concern that political casualties may affect economic outcomes through a different, institutional channel: political stability. There is some evidence in the development economics literature that conflict can exert a direct influence on economic outcomes.[95] In order to control for this possibility, the first-stage regression includes a variable commonly used as an instrument for conflict,

[94] James H. Stock and Motohiro Yogo, "Testing for Weak Instruments in Linear IV Regression", in Donald W. K. Andrews and James H. Stock (eds.), *Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg* (Cambridge, MA: Cambridge University Press, 2005).

[95] Robert J. Barro, "Economic Growth in a Cross Section of Countries", *Quarterly Journal of Economics*, 106: no. 2 (May 1991); William R. Easterly and Ross Levine, "Africa's Growth Tragedy: Policies and Ethnic Divisions", *Quarterly Journal of Economics*, 112: no. 4 (November 1997).

but plausibly exogenous to human capital decisions: climatic changes. Weather shocks have been used successfully to identify the effects of political instability on economic outcomes in a number of applications, especially in developing economies with significant rural populations.[96] In the case of China, researchers have deployed either rainfall[97] or weather-linked crop failure[98] as instruments. Since Revolution-related conflict occurred in both rural and urban areas, I rely instead on temperature as a primary instrument for weather-induced conflict, appealing to the literature in social psychology that relates haptic sensations to interpersonal hostility.[99]

Another complicating factor that could invalidate the instrument is related to migratory flows. If teachers and intellectuals chose to migrate to provinces with weaker levels of persecution, and if these provinces were in turn systematically different from others, this would violate the exclusion restriction. While this is a theoretical possibility, the likelihood that this matters in practice is small. To begin with, even with the rustication program, immigration during this period was very low:[100] in 1966, for example, average immigration flows across all provinces amounted to slightly fewer than a million people, or 4 percent of the total population (by way of contrast, immi-

[96] Melissa Dell, Benjamin F. Jones and Benjamin A. Olken, "Temperature Shocks and Economic Growth: Evidence from the Last Half Century", *American Economic Journal: Macroeconomics*, 4: no. 3 (July 2012); Edward A. Miguel, Shanker Satyanath and Ernest Sergenti, "Economic Shocks and Civil Conflict: An Instrumental Variables Approach", *Journal of Political Economy*, 112: no. 4 (August 2004).

[97] Ruixue Jia, "Weather Shocks, Sweet Potatoes and Peasant Revolts in Historical China", *Economic Journal*, 124: no. 575 (March 2014).

[98] James Kai-sing Kung and Chicheng Ma, "Can Cultural Norms Reduce Conflicts? Confucianism and Peasant Rebellions in Qing China", *Journal of Development Economics*, 111: Supplement C (November 2014).

[99] Adam J. Fay and Jon K. Maner, "When Does Heat Promote Hostility? Person by Situation Interactions Shape the Psychological Effects of Haptic Sensations", *Journal of Experimental Social Psychology*, 50: no. 1 (January 2014).

[100] Immigration data are not directly available from the statistical bureau. I estimate immigration flows indirectly, by taking the difference between the permanent and registered populations.

gration flows as a share of the population were more than two times larger in 2014). Moreover, with the exception of a handful of provinces, the absolute number of migrants was also small.[101] Furthermore, the relationship between migration flows and the casualties instrument is either very weak or completely nonexistent.[102]

One final concern with regard to the exclusion restriction is related to the manner by which the casualty data are temporally aggregated (across the full 1966–71 period) to obtain the instrument. Given this aggregation, future casualties will necessarily be correlated with the contemporaneous error term, violating strict exogeneity. Fortunately, the order condition in 2SLS/3SLS only requires that the instrument matrix be at least *predetermined*, not necessarily strictly exogenous. That is, one only requires the satisfaction of sequential moment conditions, such that the error term remains uncorrelated with current and historical values of the instrument, while permitting correlations with future values.[103] Since this is indeed the case, I conclude that there is no issue with this temporal aggregation in the baseline setting, where regressions correspond to the year 1966. For subsequent years, this predeterminedness argument is no longer valid, and so the quality of the instrument set rests solely on the assumption of uncorrelatedness of persecutions with the error term in the income regression.

Although there is no direct statistical test for the exclusion restriction, I follow convention and report—for specifications where the model is overidentified—the Hansen J statistic for overidentification, which evaluates the overall coherence of the instrument

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[101]The four provinces where inflows were especially large were Guangdong, Hebei, Sichuan; coastal provinces that experienced significant political turmoil, such as Beijing, Tianjin, and Shanghai, hardly saw any migratory outflows at all.

[102]The year-by-year correlation between immigration and casualties, for the years of the Revolution, is bounded by $[0.088, 0.223]$, and is uniformly insignificant at conventional levels.

[103]More formally, using the notation introduced in footnote [89], the exclusion restriction only requires that $E(\nu_s \mid P_t) = 0 \quad \forall s \geq t$, instead of $E(\nu_s \mid P_t) = 0 \quad \forall t = 1, \ldots, T$.

set.[104]

IV.III Theoretical Setting

I motivate the empirical analysis to follow with a theoretical model based on an extension of the augmented Solow (1956) growth model of Mankiw et al. (1992). The model embeds three reproducible factors: labor, L, physical capital, K, and human capital, H. Output Y in province i at time t is represented by the production function

$$Y_{it} = K_{it}^{\alpha} H_{it}^{\beta} \left(A_{it} L_{it}\right)^{1-\alpha-\beta}, \quad 0 < \alpha, \beta < 1, \tag{1}$$

where A is the level of (exogenous) total factor productivity, and I assume decreasing returns to all forms of capital, so that $\alpha + \beta < 1$.

The microeconomic literature on the education production function[105] generally relates cognitive achievement for a given individual j to innate ability, η, along with resource inputs to schooling, R. At the individual level, human capital at time t is therefore a function $H_{it} = h\left(\eta_{ij}, R_{ij,t}; E_{it}\right)$, where E is the (exogenous) environment whereby learning takes place, and individual ability is (by assumption) time-invariant. Aggregating over all effective units of labor in a given province yields

$$\begin{aligned} H_{it} &= \int_{1}^{A_{it}L_{it}} h\left(\eta_j, R_{ij,t}; E_{it}\right) dj \\ &= R_{it}^{\gamma} \left(A_{it} L_{it}\right)^{1-\gamma} \cdot E_{it}^{\psi}, \quad 0 < \gamma < 1, \end{aligned} \tag{2}$$

[104] Tests of overidentifying restrictions are often interpreted as tests of the validity of the moment conditions, although such tests are neither necessary nor sufficient; see Paulo M.D.C. Parente and João M.C. Santos Silva, "A Cautionary Note on Tests of Overidentifying Restrictions", *Economics Letters*, 115: no. 2 (May 2012).

[105] Petra E. Todd and Kenneth I. Wolpin, "On the Specification and Estimation of the Production Function for Cognitive Achievement", *Economic Journal*, 113: no. 485 (February 2003).

where I have imposed a Cobb-Douglas form and decreasing returns to inputs with $\gamma < 1$. Note the omission of the ability term at the aggregate level; this amounts to assuming that innate ability is distributed normally both within and between provinces at the national level, with cross-provincial differences averaging to zero. Taking logarithms of (2) gives the (steady-state) amount of human capital per effective unit of labor:

$$\ln\left[\frac{H_{it}^{*}}{L_{it}}\right] = \ln A_{i0} + gt + \gamma \ln r_{it} + \psi E_{it}, \tag{3}$$

where I have followed convention and rewritten $r \equiv \frac{R}{AL}$ in intensive form, representing schooling resource inputs per unit of effective labor.

Both productivity and labor grow exogenously, at rates according to

$$A_{it} = A_{i0}e^{g_i t}, \quad L_{it} = L_{i0}e^{n_i t}.$$

The equations of motion for both forms of capital thus evolve in accordance with the ordinary differential equations

$$\dot{k}_{it} = s_{ik}y_{it} - (n_i + g_i + \delta)\, k_{it}, \tag{4a}$$

$$\dot{h}_{it} = s_{ih}y_{it} - (n_i + g_i + \delta)\, h_{it}, \tag{4b}$$

where s_k and s_h are, respectively, the investment shares of physical and human capital in output, δ is the rate of capital depreciation (uniform between different types of capital), and as before the intensive form defines $y \equiv \frac{Y}{AL}$, $k \equiv \frac{K}{AL}$, and $h \equiv \frac{H}{AL}$. In the steady state, $\dot{k}_{it} = \dot{h}_{it} = 0$, which simplifies (4) down to

$$k^{*} = \left[\frac{s_{ik}^{1-\beta}s_{ih}^{\beta}}{n_i + g_i + \delta}\right]^{\frac{1}{1-\alpha-\beta}}, \quad h^{*} = \left[\frac{s_{ik}^{\alpha}s_{ih}^{1-\alpha}}{n_i + g_i + \delta}\right]^{\frac{1}{1-\alpha-\beta}}.$$

Returning the above expressions into (1), taking logarithms, and re-substituting the steady-state share of human capital back into the resulting equation gives us the steady-state income per worker

$$\ln\left[\frac{Y}{L}\right] = \ln A_{i0} + g_i t + \frac{\alpha}{1-\alpha}\ln s_{ik} + \frac{\beta}{1-\alpha}\ln h_i^* - \frac{\alpha}{1-\alpha}\ln\left(n_i + g_i + \delta\right). \qquad (5)$$

IV.IV Empirical Model

The empirical model is based on estimating system of equations summarized by (3) and (5):

$$\ln\left[\frac{H_{it}^*}{L_{it}}\right] = \mu_i + \theta_0 + \mathbf{E}_{it}'\boldsymbol{\Theta}_1 + \ln\left[\frac{\mathbf{R}_{it}}{L_{it}}\right]'\boldsymbol{\Theta}_2 + \epsilon_{it}, \qquad (6)$$

$$\ln\left[\frac{Y_{it}}{L_{it}}\right] = \rho_i + \pi_0 + \pi_1\ln s_{k,it} + \pi_2\ln\left[\frac{H_{it}^*}{L_{it}}\right] - \pi_3\ln\left(n+g+\delta\right) + \mathbf{X}_{it}'\boldsymbol{\Pi}_4 + \nu_{it}. \qquad (7)$$

where human capital is proxied by educational enrollment, labor—which normalizes all variables—is measured as the total number of employed persons, output is given by GPP, and the capital share is represented as the fixed investment share of GPP. For the net rate of depreciation of effective units of labor, $(n+g+\delta)$, I follow Mankiw et al. (1992) and assume that g and δ are constant (across provinces), so that their sum is approximated by calibrated estimates of 0.02 and 0.03, respectively.

In general, the vector of resource inputs $\mathbf{R}_{it}$ is comprised of financial resources (captured by educational expenditure), but may also entail human resources (the teacher-student ratio). The vector of additional controls, $\mathbf{X}_{it}$, include factors such as government expenditure or trade openness, which are included in robustness checks. μ_i and ρ_i are time-invariant provincial fixed effects (in panel specifications), and $\nu_{it} \sim N\left(0, \sigma_\nu^2\right)$ and $\epsilon_{it} \sim N\left(0, \sigma_\epsilon^2\right)$ are disturbance terms.[106] The theoretical prior for the

[106]Recall, following the discussion in subsection IV.II concerning measurement error in the instrument, ϵ in (6) would be equivalent to the sum of innovations ε and measurement error υ.

main coefficient of interest, π_2, is positive.

In the prior stage, H^*_{it} is instrumented by populating the external environment vector $\mathbf{E}$ with political persecutions, P_{it}, as well as (in some specifications) exogenous weather shocks W_{it} (usually proxied by temperature), so that $\mathbf{E}_{it} = [P_{it}\ W_{it}]$. In other specifications, I allow persecutions to directly depend on weather shocks:

$$\ln P_{it} = \phi_0 + \ln \mathbf{W}'_{it}\mathbf{\Phi} + \zeta_{it}, \tag{8}$$

where $\mathbf{W}_{it}$ is vector of climatic shocks (which may include precipitation in addition to temperature), and $\zeta_{it} \sim N\left(0, \sigma^2_\zeta\right)$ is a disturbance term.

I estimate the system (6)–(7) with either IV or 2SLS, adjusted for heteroskedasticity-robust standard errors with small-sample corrections. The system that includes (8) is estimated either by treating both auxiliary equations separately—and estimating a general IV specification with two endogenous variables—or via 3SLS, with general method of moments (GMM) to ensure consistent estimation.[107] While the assumptions underpinning 3SLS are admittedly more restrictive than 2SLS, it has the distinct advantage that it explicitly specifies the relationship between weather and persecutions, along with the generally-recognized benefit of improved asymptotic efficiency.

In secondary panel specifications, estimation is performed either by controlling solely for unobserved heterogeneity (with fixed or random effects, FE and RE respectively), or by also allowing for cross-sectional dependence. In the latter case, I consider panel-corrected standard error estimates (PCSE) with Prais-Winsten parameters that account for first-order AR(1) panel autocorrelation, and the family of

[107]This approach permits a slight relaxation of the requirement in traditional 3SLS that all instruments be uncorrelated with all errors. In particular, GMM permits consistent estimation in the event that the instrument is not exogenous to the persecutions equation (8) (as it is in our case), so long as errors remain homoskedastic. See Jeffrey M. Wooldridge, *Econometric Analysis of Cross Section and Panel Data*, 2nd edition (Cambridge, MA: MIT Press, 2010).

heterogeneous-panel[108] common correlated effects (CCE) models,[109] estimated either via GMM without a trend, or with a group-specific linear trend and robustness to outliers.[110]

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[108]Such models are generally preferable to Arellano and Bover (1995) and Blundell and Bond (1998)-type system GMM estimators, owing to the significantly longer time periods (relative to provincial coverage) of the panel ($T \gg N$).

[109]M. Hashem Pesaran, "Estimation and Inference in Large Heterogeneous Panels with a Multi-factor Error Structure", *Econometrica*, 74: no. 4 (July 2006).

[110]There are additional issues associated with the practical estimation of the augmented Solow model, many of which have been raised before by other authors; see Robert E. Hall and Charles I. Jones, "Why Do Some Countries Produce So Much More Output Per Worker Than Others?", *Quarterly Journal of Economics*, 114: no. 1 (February 1999) and Steve Dowrick and Mark Rogers, "Classical and Technological Convergence: Beyond the Solow-Swan Growth Model", *Oxford Economic Papers*, 54: no. 3 (July 2002). These include, *inter alia*, assumptions of homogeneous cross-province technology and a failure to distinguish between the effects of diminishing returns and technology transfer. Resolving these additional issues here would go far beyond the scope of this thesis, but I would reiterate that the focus here is on resolving endogeneity issues in estimating the effects of human capital on economic outcomes, not on testing the Solow model.

V.

Results

V.I Baseline Results

Table 1 presents the province-level baseline results for 1966, the first year of the Cultural Revolution. The top panel reports the estimates corresponding to the main equation of interest (7), while the middle panel includes coefficients for the auxiliary equation(s). The first column offers OLS estimates, with bootstrapped errors to accommodate the small sample size, as a simple benchmark. This is followed by a basic IV specification, where human capital is instrumented solely with political casualties. The next two columns report 2SLS results that include educational expenditure as a resource input, alongside casualties as the sole exogenous instrument (column 3), or with both casualties and weather (column 4). Column 5 then treats both human capital and persecutions as endogenous (determined, respectively, by casualties and weather), while the final column estimates the 3-equation system via 3SLS.

By and large, the effect of human capital on per-worker income is positive and statistically significant. Significant point estimates, excluding OLS, range $[6.59, 7.41]$, and are typically five times to a full order of magnitude larger than the contribution from physical capital or labor supply growth. The instrumented specifications all result in a larger coefficient, relative to OLS.[111] However, the coefficient on human capital for the pure IV specification (column 2) is very imprecisely estimated, which is attributable to the weakness of the casualty instrument when used solely as a determinant of human capital. The richer specifications, however, perform much better: the

[111] Since the IV specification is an estimate of the local average treatment effect (LATE) induced by the political casualties instrument, this suggests that the average treatment effect (ATE), as captured by OLS, tends to be weaker than the treated subsample. To the extent that this treatment selected for the intellectual class (as argued in Section III), it is entirely reasonable that the magnitude of the LATE is greater than the ATE.

Table 1: Baseline results for effects of human capital on output, 1966†

	(1)	(2)	(3)	(4)	(5)	(6)
				Main		
Human capital	3.920	24.354	7.412	7.260	6.586	7.089
per worker	(1.273)***	(73.627)	(2.387)***	(2.256)***	(1.306)***	(1.655)***
Capital share	0.872	9.077	1.310	1.240	-0.491	0.166
	(1.158)	(33.875)	(1.011)	(0.979)	(0.553)	(1.193)
Net depreciation	0.309	5.021	1.068	1.033	-0.118	0.170
	(0.454)	(17.090)	(0.602)*	(0.569)*	(0.217)	(0.462)
				Auxiliary		
Resources			0.791	0.734	0.913	0.947
per worker			(0.262)***	(0.280)**	(0.197)***	(0.199)***
Persecutions		-0.005	0.015	0.017	0.011	0.053
		(0.017)	(0.016)	(0.016)	(0.018)	(0.030)*
Weather				-0.021	0.820	0.637
shock				(0.032)	(0.332)**	(0.308)**
Estimator	OLS	IV	2SLS	2SLS	2V-IV	GMM-3SLS
R^2/χ^2	0.444	-13.138	0.041	0.077	27.945	0.0853
Underid. p		0.768	0.017**	0.033**		
Overid. p			0.295	0.250		0.077*
Weak in. F		1.83	27.51***	21.23***		
Weak id. F (crit.)		0.08 (5.53)	4.71 (7.25)	3.79 (7.80)		
Obs.	30	29	29	29	29	29
Obs. unit	Province	Province	Province	Province	Province	Province

† The dependent variable in the main equation is the natural logarithm of income per worker, and for the auxiliary equations are either human capital per worker (IV/2SLS) or persecutions (2V-IV/3SLS). All independent variables are expressed in natural logarithms, unless otherwise indicated. A constant term was included in the regressions, but not reported. Bootstrapped (OLS) or heteroskedasticity-robust (IV, 2SLS, and 3SLS) standard errors with small-sample correction are given in parentheses. Goodness-of-fit measures report the adjusted R^2 (OLS, IV/2SLS), main-equation pseudo R^2 (3SLS), or the Wald χ^2 (2V-IV). The underidentification test reports the p-value associated with the Kleinbergen-Paap *rk LM* statistic, the overidentification test the p-value associated with the Hansen J statistic (where applicable), and the weak instrument tests are the Anderson-Rubin Wald F and Kleinbergen-Paap *rk F*, with Stock-Yogo critical value corresponding to 25% maximal IV size in parentheses. * indicates significance at the 10 percent level, ** significance at the 5 percent level, and *** significance at the 1 percent level.

combined instrument set generally satisfies the relevance condition, although the $rk\ F$ suggests some possibility of weak-instrument bias. Furthermore, in all overidentified specifications, the instrument set remains coherent, at least at conventional levels.

On the basis of this set of results, I conclude that human capital exerts a positive causal effect on per worker incomes, of around seven percent for each percentage change in human capital. Put another way, a negative shock to human capital of around a third of a percent—the maximum two-year decline over the course of the decade-long Revolution—would have led to a reduction in incomes of around 2.3 percent. To place this in context, the average 2-year growth rate since 2000 was a quarter of a percent. Viewed this way, the potential losses from human capital destruction during a particularly severe 2-year Revolution period would be almost 10 times as large, or have taken around 20 years to recover from.

The other results are worth commenting on briefly. As expected *a priori*, greater schooling resources are associated with higher levels of human capital attainment. Although not always statistically significant, political persecution is also positively related to human capital, corroborating the historical narrative that persecutions were more severe in provinces that had greater levels of education, on average.[112] Finally, the coefficient on weather shocks is also positive, corroborating the idea that political instability resulting in more casualties is heightened when temperatures rise.

To gain a better handle on the evolution of human capital effects over the course of the Revolution, Figure 4 plots the coefficients, along with associated 95 percent confidence bands, from repeated cross-sectional estimates of the 2SLS specification in column (4). It is clear that through most of the ordeal, the shock to human capital from remained significant. Only at the end of the Revolution—all years after 1972—

[112] Note that the initially more intuitive notion that a larger casualty count should be associated with *lower* human capital is not applicable here, since the model is estimated on levels. The coefficient for casualties with *changes* in human capital (considered in the robustness checks to follow) is indeed negative, although as is the case here, these are indistinguishable from zero.

does the effect of human capital fall out of statistical significance,[113] with the point estimate falling to the lowest magnitude in the final year.[114]

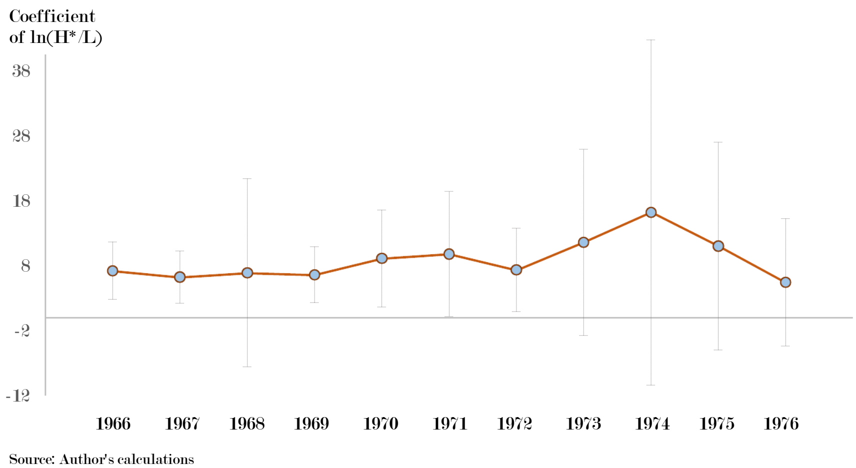

Figure 4: Coefficients (blue markers) with associated 95 percent confidence bands (gray capped lines) for human capital, repeated cross-sections, 1966–76. Estimates are obtained using 2SLS, including both persecutions and surface temperature as instruments. Point estimates in the earlier years of the Cultural Revolution, through till 1972, are typically precisely estimated and significantly positive. Point estimates between 1973 and 1976 are estimated much less precision (and indeed are indistinguishable from zero), and begin to decline in magnitude in the latest years.

V.II Robustness of the Baseline

I consider a suite of robustness checks on the baseline in Table 2. These checks can be classified into several classes: (a) alternative choices of the main dependent variable; (b) alternative choices for the key independent variables; (c) changes to the instrument set; and (d) additional controls for the main and auxiliary equations. In the appendix, I also consider an alternative data source for the weather instruments, and a host of

[113]Part of this is likely attributable to the fact that the political persecution instrument only records casualties through till 1971; with the signal from the instrument tapering to zero, it is also unsurprising that the instrumented coefficients become increasingly imprecisely estimated.

[114]This general pattern is not unique to the choice of the specification. In the appendix, I replicate the result for the 3SLS estimator, and the qualitative message is essentially identical.

additional controls for the main regression (including a specification that includes all the additional controls). For all checks, I report two specifications: the 2SLS variant with both persecution and weather instruments (column 4 of the baseline), for the first year of the Revolution (1966), and the 3SLS version corresponding to the 3-equation system (column 6 of the baseline), for the midpoint of the Revolution (1970).[115]

The first two columns swap gross provincial product, which is measured using the production method in the baseline, with GPP computed using the expenditure method.[116] Despite the slightly smaller sample size, the coefficients remain positive and highly significant.

The subsequent eight columns alter the key independent variables in the baseline.

Columns 3 and 4 utilize gross investment in place of fixed asset investment, under the notion that inventory accumulation during a period of social upheaval may have been exaggerated (thereby raising gross investment). While the human cpaital coefficient in the 3SLS specification does in fact fall significantly (and out of significance), that in the 2SLS setting actually rises; nevertheless, in either case, the coefficient on capital share remains statistically indistinguishable from zero.

Columns 5–8 replace total enrollment with enrollment at the primary level (columns 5 and 6)—to assess the importance of basic education—and the stock of teachers at the primary and secondary levels (columns 7 and 8), chosen to examine whether a stock concept of human capital may be critical.[117] In both cases, the significance of

[115]Recall, this was also the year where the "One Strike, Three Anti" campaign was underway, precipitating a renewed wave of casualties, and marked a new nadir in the Revolution's impact.

[116]I also considered specifications where the dependent variable was consumption instead of income per worker. While the sample size falls slightly (to 25 observations), the coefficient on human capital remains qualitatively unchanged. These results are available on request.

[117]In unreported regressions, I also consider enrollment at the tertiary level only. Although the coefficients in this instance are not directly comparable with those at the primary level—I revisit this comparison later with standardized coefficients for that very purpose—the human capital effect remains significant.

Table 2: Robustness results for effects of human capital on output, 1966 and 1970†

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Human capital	7.008	10.173	8.376	4.508	8.875	12.191	93.045	158.186	6.275	10.521
per worker	(2.093)***	(3.712)***	(2.727)***	(7.052)	(3.082)***	(4.289)***	(33.392)***	(46.708)***	(2.063)***	(3.366)***
Capital share	1.121	-1.323	2.865	-4.405	1.050	-1.174	0.568	0.165	0.837	0.867
	(1.093)	(2.445)	(1.890)	(3.179)	(0.946)	(2.031)	(1.191)	(1.986)	(0.978)	(2.083)
Net depreciation	0.845	0.032	0.978	-0.011	1.268	0.026	0.456	0.007	-5.575	-4.298
	(0.483)*	(0.150)	(0.633)	(0.189)	(0.670)*	(0.147)	(0.380)	(0.150)	(11.897)	(8.337)
Variation:										
Alt. dep?	Yes	Yes	No	No	No	No	No	No	No	No
Alt. indep?	No	No	Capital share	Capital share	Human capital	Human capital	Human capital	Human capital	Depreciation	Depreciation
Year	1966	1970	1966	1970	1966	1970	1966	1970	1966	1970
Estimator	2SLS	GMM-3SLS	2SLS	GMM-3SLS	2SLS	GMM-3SLS	2SLS	GMM-3SLS	2SLS	GMM-3SLS
R^2	0.145	-1.121	-0.070	0.110	-0.178	-0.778	0.253	-0.157	0.166	-0.913
Obs.	26	26	25	25	29	29	29	29	29	29
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Human capital	7.272	10.869	6.730	12.156	5.004	7.467	6.272	5.620	6.882	7.943
per worker	(2.259)***	(3.702)***	(2.043)***	(3.751)***	(1.483)***	(1.566)***	(2.551)**	(0.766)***	(2.079)***	(2.696)***
Capital share	1.245	-0.213	0.997	2.059	1.340	0.652	0.329	-0.923	1.066	-0.583
	(0.980)	(2.463)	(0.912)	(2.596)	(0.763)*	(1.278)	(1.349)	(0.689)	(0.876)	(1.879)
Net depreciation	1.035	0.004	0.909	-0.010	0.457	-0.041	0.540	-0.045	0.944	0.009
	(0.570)*	(0.153)	(0.508)*	(0.174)	(0.380)	(0.119)	(0.586)	(0.099)	(0.522)*	(0.153)
Variation:										
Alt. instr?	Yes	Yes	No	No	No	No	No	No	No	No
Add. instr?	No	No	Yes	Yes	No	No	No	No	No	No
Add. ctrl?	No	No	No	No	Government	Government	Trade	Trade	Resources	Resources
Year	1966	1970	1966	1970	1966	1970	1966	1970	1966	1970
Estimator	2SLS	GMM-3SLS	2SLS	GMM-3SLS	2SLS	GMM-3SLS	2SLS	GMM-3SLS	2SLS	GMM-3SLS
R^2	0.074	-1.204	0.189	-1.286	0.622	0.172	0.229	0.545	0.159	-0.198
Obs.	29	29	29	29	28	28	24	28	29	29

† The dependent variable is the natural logarithm of income per worker. All independent variables are expressed in natural logarithms, unless otherwise indicated. A constant term was included in the regressions, but not reported. Heteroskedasticity-robust standard errors with small-sample correction are given in parentheses. Goodness-of-fit measures report the adjusted R^2 (2SLS) or main-equation pseudo R^2 (3SLS). * indicates significance at the 10 percent level, ** significance at the 5 percent level, and *** significance at the 1 percent level.

the coefficient on human capital survives this change in proxy for the main regressor.

Finally, columns 9 and 10 replace the growth rate of the labor force with the provincial population growth rate. Since the Cultural Revolution saw the displacement of many skilled workers from the official labor force, this alternative may better capture shifts in growth-enhancing demographic dynamics. Unfortunately, there is little change to the insignificance of the coefficient on net depreciation (although these do turn more negative in magnitude), while that on human capital is largely unaltered.

Columns 11–14 explore changes in the instrument set. Given the wider variation in political victims—as opposed to deaths—it is worthwhile checking if using only victims data may improve the quality of the instruments (in particular, if there are improvements in the $rk\ F$ in the weak identification test). As it turns out, both coefficient estimates and postestimation results are virtually identical, as can be seen by comparing column 11 to column 4 of Table 1.[118] Columns 13 and 14 then include an additional weather instrument, average precipitation, to the instrument set. While there is some increase in the human capital coefficient on the 3SLS estimate, that on the 2SLS specification declines marginally, and postestimation tests (not reported) are likewise largely unaltered.[119]

Columns 15 through 18 add two other controls into the income equation that may be important. I choose total trade—defined as the sum of imports and exports in GPP—and total net government expenditure, because these two are components of the provincial account identity.[120] Introducing these changes do not bring about any

[118]Interestingly, this is also largely the case when using only political deaths, which I do not report, but are available on request.

[119]To allay concerns about the confounding effect of migration on the political persecutions instrument, I also run regressions that include immigration flows in human capital equation. These additional checks do not alter the qualitative findings here, and these results are available on request.

[120]In the appendix, I consider a large number of additional covariates, including those related to health capital, infrastructure, and financial development. By and large, the human capital coefficient survives the introduction of these controls, although in some cases the sample size is significantly

remarkable changes to the coefficient estimates vis-à-vis the baseline.

The final two columns of Table 2 adds an additional covariate to the human capital equation: the teacher-student ratio. As opposed to educational expenditure, which captures financial resources, the teacher-student ratio gauges invested human resources, which could be relevant since some authors[121] have made the case that class size is a key determinant of schooling outcomes. Although this measure of resources does feature in the human capital equation with a statistically significant coefficient and with a correct sign,[122] the results in the income equation are qualitatively unaffected.

While not strictly in line with the empirical specification given by (6), (7), and (8), it is useful to further explore whether the baseline results are robust to growth instead of level effects. Accordingly, I take first differences in the estimating equations and rerun the specifications as changes over three different period lengths: the two-year period that captures the onset of the revolution (1965–67), the six-years span between the start and the zenith of the Revolution (1965–71), and the full eleven-year change from 1965 to 1976. These are reported in Table 3, with the same two specifications as before.

A rather interesting message emerges from this exercise. While the coefficient on human capital for the initial two-year period is positive and significant, the magnitude of the human capital effect falls as the period considered lengthens. Equivalent coefficient magnitudes for the six-year period are around half that of the two-year

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compromised (owing to missing data in certain provinces), which justifies their exclusion from the checks considered here.

[121] Alan B. Krueger, "Economic Considerations and Class Size", *Economic Journal*, 113: no. 485 (February 2003).

[122] The coefficient is negative and significant in the 2SLS regression at the 1 percent level. However, the coefficient estimate on human capital is largely unaltered, and furthermore, there is little gain by way of weak-instrument statistics.

Table 3: Robustness results for effects of growth in human capital on growth in output, 1965–76 (varying period length)†

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta t = 2$		$\Delta t = 6$		$\Delta t = 11$	
Human capital	4.891	4.641	2.826	2.041	2.103	1.585
growth	(0.670)***	(0.808)***	(1.375)**	(1.211)*	(1.470)	(0.994)
Capital share	0.825	-0.183	-1.642	-0.058	-0.075	0.181
growth	(1.648)	(1.178)	(1.103)	(0.842)	(0.958)	(0.640)
Net depreciation	0.584	0.082	-0.405	-0.506	-0.035	-0.295
growth	(0.424)	(0.260)	(0.288)	(0.202)**	(0.379)	(0.231)
Period	1965–67	1965–67	1965–71	1965–71	1965–76	1965–76
Estimator	2SLS	GMM-3SLS	2SLS	GMM-3SLS	2SLS	GMM-3SLS
R^2	0.416	0.443	0.468	0.593	0.190	0.383
Underid. p	0.585		0.104		0.574	
Overid. p	0.633	0.064*	0.689	0.106	0.507	0.091*
Weak in. F	32.74***		4.826		1.158	
Weak id. F	12.38 (7.80)		4.88 (7.80)		1.38 (7.80)	
Obs.	29	29	29	29	29	29

† The dependent variable is the growth rate of income per worker. All independent variables are expressed percentage changes, unless otherwise indicated. A constant term was included in the regressions, but not reported. Heteroskedasticity-robust standard errors with small-sample correction are given in parentheses. Goodness-of-fit measures report the adjusted (2SLS) or unadjusted main-equation (3SLS) R^2s. The underidentification test reports the p-value associated with the Kleinbergen-Paap *rk LM* statistic, the overidentification test the p-value associated with the Hansen J statistic (where applicable), and the weak instrument tests are the Anderson-Rubin Wald F and Kleinbergen-Paap *rk F*, with Stock-Yogo critical value corresponding to 25% maximal IV size in parentheses. * indicates significance at the 10 percent level, ** significance at the 5 percent level, and *** significance at the 1 percent level.

change, with weaker statistical significance; for the full period, changes in human capital are no longer associated with income changes at all. This is likely due to the loss of instrument validity, as the degree of changes in human capital that can credibly be attributed to the Revolution shock diminishes, and is corroborated by the decline in the weak-instrument and weak-identification F statistics. Overall, the results in Table 3 attest to the importance of accounting for the endogeneity in human capital effects using our identification strategy requires careful attention to the conditions under which the quasi-experimental is applicable.

VI.

Discussion

VI.I Overall Effects of Human Capital on Output

The changing magnitude of the human capital effect, especially toward the end of the Cultural Revolution period, is a stark reminder of how the overall influence of human capital may well depend on the temporal frame in question. Since provincial-level data are available for a much longer time frame, I can now turn to an examination of full-panel effects. The purpose here is not so much to analyze human capital on income over the Revolution—the casualties instrument is, after all, only available as a cross-section—but rather to offer some context as to how important human capital is when considered in terms of within-province variation over time.

To this end, the first three columns of Table 4 reports results for the unbalanced panel between 1951 and 2014 from three alternative panel data models. These models accommodate only unobserved heterogeneity, and the specifications incrementally allow more variability in the residual structure. Column 1 are within estimates from a fixed effects model, column 2 the random effects model,[123] and column 3 is a FE model that embeds an autocorrelated error term.

Perhaps somewhat surprisingly, the overall relationship between human capital and income is actually *negative*: the coefficients are tightly bound between -1.27 and -1.37, and are at least marginally significant. The point estimate on the RE model is very close the FE versions. Since the RE model is simply a matrix-weighted version of the between and within estimators, this implies that this negative result is essentially due to within-province variation. It is also evident that the contribution of capital accumulation is both significant and much greater in the broader panel, and even the

[123] A simple Hausman test on coefficient differences between the two is insignificant, thereby lending support to the use of a more-efficient random effects estimator.

Table 4: Overall effects of human capital on output, 1951–2014 (unbalanced panel)[†]

	(1)	(2)	(3)	(4)	(5)	(6)
	Unobserved heterogeneity			*Cross-sectional dependence*		
Human capital	-1.370	-1.268	-1.370	1.220	1.860	1.739
per worker	(0.742)*	(0.706)*	(0.675)**	(0.326)***	(0.218)***	(0.304)***
Capital share	8.975	8.847	8.975	1.808	0.144	-0.089
	(1.884)***	(1.917)***	(0.827)***	(0.312)***	(0.165)	(0.192)
Net depreciation	-0.619	-0.608	-0.619	-0.263	-0.119	-0.255
	(0.261)**	(0.258)**	(0.206)***	(0.048)***	(0.106)	(0.097)***
Estimator	FE	RE	AR(1) FE	PCSE	CCE	GMM-CCE
R^2/χ^2	0.526	0.526	0.526	512.702	86.891	39.936
Provinces (Periods)	31 (55)	31 (55)	31 (55)	31 (55)	31 (55)	31 (55)
Obs.	1,716	1,716	1,716	1,716	1,716	1,716

[†] The dependent variable is the natural logarithm of income per worker. All independent variables are expressed in natural logarithms, unless otherwise indicated. A constant term was included in the regressions, but not reported. Standard errors clustered at the provincial level and corrected for AR(1) autocorrelation (columns 3, 4) and/or spatial dependence (columns 4–6) are given in parentheses. Goodness-of-fit measures report the within R^2 (columns 1–3) or Wald χ^2 (columns 4–6). * indicates significance at the 10 percent level, ** significance at the 5 percent level, and *** significance at the 1 percent level.

coefficient on net depreciation enters with its theory-consistent sign (recall, the dependent variable is per-worker income, and so faster growth in the employed workforce raises the denominator). On the basis of these set of regressions alone, one is tempted to conclude that human capital is either relatively unimportant or, worse, detrimental to economic performance.

But the results obtained in the previous section suggest caution is warranted before arriving at this conclusion. After all, the severe period-specific shocks over the course of the Cultural Revolution was experienced—albeit heterogeneously—in common across China. This calls for panel models that can account for not just unobserved heterogeneity, but also cross-sectional dependence. Columns 4–6 of Table 4 reports the results from three such regressions. Column 4 reproduces the AR(1) residual structure of the previous column with Prais-Winsten parameters, but applies panel-corrected standard errors that also reflect spatial correlation. The final two columns rely on a common correlated effects (CCE) models, estimated with a group-specific trend (column 5) or without (column 6).

Once I do so, the effect of human capital reverses signs, and returns to a high level of statistical significance.[124] Although the magnitudes are smaller than for the cross-sectional analyses, this is to be expected, since these estimates rely on (less volatile) within-province variation, and moreover encompass a much longer time frame, the majority of which did not involve any large human capital shock. Still, the positive effects—on average, a one percent increase in human capital is accompanied by a 1.6 percent increase in income—completely dominate those of the other two factors in two of the three specifications.

VI.II A Comparison to Other Historical Episodes

Given the sensitivity of the estimates of the human capital effect to the period under examination, the next natural question that arises is to ask whether the instrument set is likewise dependent on the period. In theory, this should be the case: the instruments capture a specific type of human capital shock during the Cultural Revolution, and in the absence of a shock of this nature, the instruments should no longer be applicable. The most straightforward approach to verify this conjecture is to deploy a falsification test, where we rerun the regressions for a period *outside* of the natural experiment.

As should already be evident from Figure 4, the precision of the human capital point estimates does indeed decline the further one moves from the start of the Cultural Revolution. To verify that this fading effect is not only due to noise, I rerun the 2SLS and 3SLS specifications for the first post-Revolution year, 1977. These are reported in the first two columns of Table 5. In both cases, the coefficient is not statistically

[124]Since the coverage is unchanged (31 provinces averaging 55 years each), this changed result must emanate from explicitly allowing for spatial dependency. In effect, the dynamic heterogeneous panel models are accounting from the types of common negative shocks to human capital that would otherwise throw off a standard fixed effects result.

significant, confirming the need to confine the analysis to the Revolution period.[125]

In contrast to the post-Revolution period, a comparable falsification test for the period *prior* to the Cultural Revolution is considerably more tricky. For starters, while the Revolution officially dates back to May 1966, a number of events were already in motion in the years prior. The Socialist Education movement was in full force between 1963 and 1964, ostensibly to root out corruption and ideological backsliding, but in reality set the stage for the Revolution to come.[126] By the eve of the Revolution in 1965, public struggle sessions involving attacks against academics—such as the campaign against Tsinghua history professor Wu Han—had already begun.[127] Perhaps the most severe complication (from an analytical perspective) is the fact that the Great Leap Forward (1958–62)—which also saw widespread persecution of intellectuals, alongside socioeconomic disruption and mass starvation—preceded the Cultural Revolution. The most pernicious effects of the famine only receded at the end of 1962, and (coincidentally) many of the provinces with the highest excess mortality—such as Guangxi, Hunan, and Sichuan—also experienced a disproportionate amount of Revolution-related casualties. All these factors leave little scope for clean identification for the pre-Revolution period.[128] An in-depth comparison of the two episodes is beyond the scope of this thesis. Instead, I consider a pair of tests that attempt to tease out whether the Revolution may have given rise to a greater negative human capital shock than otherwise.

[125] Although not reported, this is also the case for the years beyond. In fact, the human capital effect occasionally turns negative (although still statistically insignificant) for certain years after 1978. These results are available on request.

[126] Dikötter, 2010:, *op. cit.* (as in n. 2).

[127] MacFarquhar and Schoenhals, 2008:, *op. cit.* (as in n. 4).

[128] Although the casualty instrument dates between 1966 and 1971, it is unclear to what extent political persecutions did not follow a spatial distribution similar to that of the Great Leap Forward. Furthermore, climactic conditions during the Great Leap Forward would render the temperature instruments equally relevant during that time.

The first strategy is to repeat the 2SLS and 3SLS regressions for a pre-Revolution year. I choose 1963 both because this is the first year after the events of the Great Leap Forward, and because—as discussed above—later years are more likely to be contaminated by pre-Revolution persecution of intellectuals. These results are reported in columns 3 and 4. The estimates turn out to be significant, which justifies the concern regarding contamination. But they are significantly smaller in magnitude compared to those pertaining to the Revolution years (except for 1976). Just as tellingly, the postestimation statistics do not lend much support to the instruments. For example, the 2SLS results for 1977 fails the relevance condition, while the 3SLS results for 1963 suggests that the instrument set is not coherent.

The second way I tackle the ambiguity in the pre-Revolution results is by running rolling panel regressions at the provincial level (as opposed to the repeated cross-sections shown in Figure 4), corresponding to 10-year windows between 1951 and 1969. This allows me to examine whether the changing magnitude of the coefficients are indeed reflective of a more systemic shift in human capital effects, or whether any given change is due largely to noise. Figure 5 plots the results. There are three noticeable dips in coefficient estimates in the chart. The first occurs in 1955, the second in 1963, and the third in 1969. The final decline corroborates the findings earlier, about a diminishing effect of human capital as the Revolution wore on. The first dip is followed by a sharp rise in the human capital coefficient, beginning with the Hundred Flowers Campaign in 1956 and peaking at the height of the Great Famine in 1960. This then steadily falls till it attains a nadir in 1963. Notably, most coefficients fall on the low end of comparable within estimates reported in Table 4. Taken together, the rolling regressions support the notion that the human capital effect was not particularly large prior to the enforced shock from the Cultural Revolution.

Table 5: Falsification tests for effects of human capital on output, 1977 and 1963†

	(1)	(2)	(3)	(4)
	Post-Revolution		*Pre-Revolution*	
Human capital	21.325	8.927	5.132	5.724
per worker	(32.786)	(7.254)	(1.047)***	(0.774)***
Capital share	-5.870	-2.717	-0.477	-1.345
	(11.788)	(2.102)	(2.117)	(2.084)
Net depreciation	3.785	0.745	0.412	-0.126
	(7.526)	(1.759)	(0.222)*	(0.348)
Year	1977	1977	1963	1963
Estimator	2SLS	GMM-3SLS	2SLS	GMM-3SLS
R^2	-12.951	-1.735	0.492	-14.715
Underid. p	0.928		0.014**	
Overid. p	0.838	0.057*	0.178	0.003***
Weak in. F	50.99***		21.65***	
Weak id. F	0.15 (7.80)		17.36 (7.80)	
Obs.	29	29	28	28

† The dependent variable is the natural logarithm of income per worker. All independent variables are expressed in natural logarithms, unless otherwise indicated. A constant term was included in the regressions, but not reported. Heteroskedasticity-robust standard errors with small-sample correction are given in parentheses. Goodness-of-fit measures report the adjusted R^2 (2SLS) or main-equation pseudo R^2 (3SLS). The underidentification test reports the p-value associated with the Kleinbergen-Paap *rk LM* statistic, the overidentification test the p-value associated with the Hansen J statistic (where applicable), and the weak instrument tests are the Anderson-Rubin Wald F and Kleinbergen-Paap *rk F*, with Stock-Yogo critical value corresponding to 25% maximal IV size in parentheses. * indicates significance at the 10 percent level, ** significance at the 5 percent level, and *** significance at the 1 percent level.

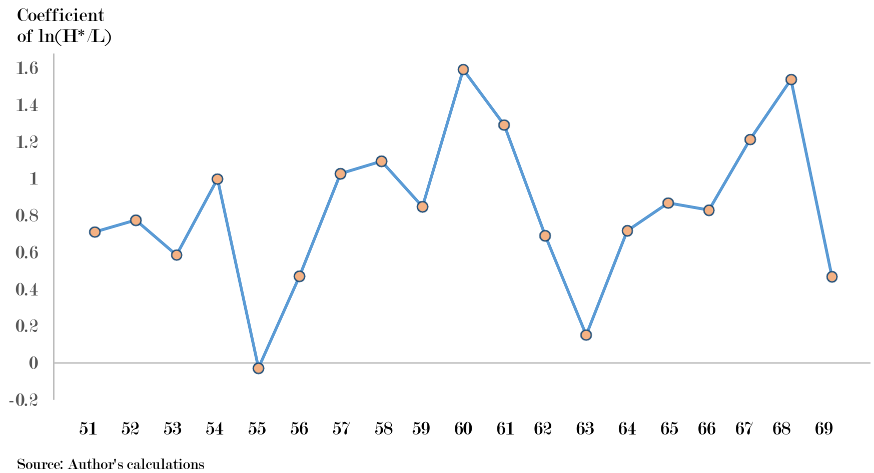

Figure 5: Coefficients (orange markers) for human capital, 10-year rolling panel regressions, 1951–69. Estimates are obtained using CCE. Point estimates several years prior to the Great Leap Forward (1958–62) and the Cultural Revolution (1966–76) demonstrate a noticeable dip in magnitude (as does the end of the period, which encompasses years after the Revolution).

VI.III What Were the Channels of Transmission for Human Capital?

To better understand the manner by which human capital affects growth, it is useful to inquire whether shocks to different levels of schooling have different effects. To do so, I decompose total enrollment into its components, and replace the measure with enrollment at either the primary, secondary, or tertiary level. All variables are then standardized to allow direct comparison of the coefficient estimates, and the 2SLS and 3SLS specifications are rerun. Columns 1–2, 4–5, and 7–8 of Table 6 report these results for the respective levels.

While in virtually all cases the coefficients are positive and significant, the standardized coefficient on tertiary education is larger by around a third, and that on secondary almost twice as large. This result implies that one-standard deviation increases in secondary (tertiary) education have a 30 (80) percent larger effect on per worker incomes than equivalent one-standard deviation increases in primary schooling. This is somewhat surprising: for the period considered, Mincerian returns to

Table 6: Transmission effects of different forms of human capital on output, 1966†

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Primary			*Secondary*			*Tertiary*	
Human capital per worker	0.480	0.484	0.520	0.912	0.860	0.224	0.632	0.638	0.546
	(0.167)***	(0.130)***	(0.262)**	(0.409)**	(0.233)***	(0.312)	(0.335)*	(0.132)***	(0.508)
Primary ed. growth			-0.271						
			(0.637)						
Hum. cap. × Pri. growth			0.038						
			(0.022)*						
Secondary ed. growth						-0.766			
						(1.680)			
Hum. cap. × Sec. growth						-0.044			
						(0.033)			
Tertiary ed. growth									-3.369
									(3.100)
Hum. cap. × Ter. growth									0.020
									(0.054)
Capital share	0.101	0.007	-0.026	0.187	-0.022	-0.047	-0.100	-0.106	-0.039
	(0.091)	(0.131)	(0.055)	(0.162)	(0.132)	(0.049)	(0.061)	(0.084)	(0.048)
Net depreciation	0.119	0.022	-0.002	0.006	-0.020	-0.039	-0.035	-0.032	-0.052
	(0.063)*	(0.056)	(0.028)	(0.045)	(0.027)	(0.021)*	(0.019)*	(0.022)	(0.026)**
Estimator	2SLS	GMM-3SLS	2V-IV	2SLS	GMM-3SLS	2V-IV	2SLS	GMM-3SLS	2V-IV
R^2/χ^2	-0.178	0.130	17.470	-0.427	-0.117	12.280	-0.033	-121.122	6.235
Underid. p	0.035*			0.230			0.233		
Overid. p	0.211	0.065*		0.980	0.265		0.090*	0.376	
Weak in. F	21.226***			21.226***			21.226***		
Weak id. F (crit.)	3.51 (7.80)			1.75 (7.80)			1.43 (7.80)		
Obs.	29	29	29	29	29	29	29	29	29

† The dependent variable is the standardized natural logarithm of income per worker. The human capital measure is the standardized natural logarithm of the human capital channel listed in the top row, except for columns 3 and 6, where it is the standardized baseline measure. The remaining independent variables are in analogous standardized natural logarithm form. A constant term was included in the regressions, but not reported. Heteroskedasticity-robust standard errors with small-sample correction are given in parentheses. Goodness-of-fit measures report the adjusted R^2 (2SLS) or main-equation pseudo R^2 (3SLS). The underidentification test reports the p-value associated with the Kleinbergen-Paap $rk\ LM$ statistic, the overidentification test the p-value associated with the Hansen J statistic (where applicable), and the weak instrument tests are the Anderson-Rubin Wald F and Kleinbergen-Paap $rk\ F$, with Stock-Yogo critical value corresponding to 25% maximal IV size in parentheses. * indicates significance at the 10 percent level, ** significance at the 5 percent level, and *** significance at the 1 percent level.

education are typically higher at the primary relative to tertiary levels.[129] However, more recent estimates do suggest that returns at the tertiary level are indeed slightly higher at the tertiary level,[130] consistent with the results here. This anomaly is even more pronounced at the secondary level, where returns found in the literature tend to be lower than either the primary or tertiary levels. That said, the outcome is consistent with findings that stress the spillover effects of university-level education on productivity[131] and growth, more generally.

In order to verify the robustness of this result, therefore, I consider an alternative strategy for eliciting the educational level effect: I take first differences of the standardized primary, secondary, or tertiary share of labor, and interact this with the aggregate human capital measure.[132] This approach allows me to examine the effect that a *change* in educational level has on income, conditional on a given level of aggregate human capital. These results are reported in columns 3, 6 and 9.

Unfortunately, the results are still not definitive. Only the coefficient on the interaction term for changes in primary is positive and (marginally) significant, while that on the other two interaction terms are insignificant, with differing signs. On balance, the results from applying this alternative strategy do not offer a sufficiently strong

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[129] George Psacharopoulos and Harry Anthony Patrinos, "Returns to Investment in Education: A Further Update", *Education Economics*, 12: no. 2 (August 2004).

[130] Claudio E. Montenegro and Harry Anthony Patrinos, "Comparable Estimates of Returns to Schooling Around the World", World Bank Policy Research Working Paper 7020 (Washington, DC: The World Bank, 2014).

[131] Ching-Fu Chang, Ping Wang and Jin-Tan Liu, "Knowledge Spillovers, Human Capital and Productivity", *Journal of Macroeconomics*, 47: No. B (March 2016); Enrico Moretti, "Workers' Education, Spillovers, And Productivity: Evidence From Plant-Level Production Functions", *American Economic Review*, 94: no. 3 (June 2004).

[132] Taking differences is necessary to avoid introducing multicollinearity issues, given the strong correlation between the total and subcomponent series. Interacting two endogenous variables brings its own set of econometric considerations, which requires generating additional instruments from the interaction of the endogenous variables with the instruments. The extended linear model is then estimated via maximum likelihood.

reason to overturn the original finding that the transmission effects of human capital are more likely to be larger due to either secondary or tertiary education.

Next, I explore which element of output is most affected by human capital. Table 7 repeats the 2SLS and 3SLS specifications but replaces the measure of income with provincial product in either the agricultural (columns 1–2), manufacturing (columns 3–4), or services (columns 5–6) sector. Since the comparison is not between different independent variables, I retain all variables in their baseline logarithm transform.

Table 7: Transmission effects of human capital on different forms of output, 1966†

	(1)	(2)	(3)	(4)	(5)	(6)
	Agriculture		*Manufacturing*		*Services*	
Human capital	0.827	0.963	7.967	7.501	8.667	8.443
per worker	(1.104)	(0.914)	(3.134)**	(1.949)***	(2.491)***	(2.037)***
Capital share	0.099	0.208	2.507	0.580	1.647	0.218
	(0.580)	(0.798)	(1.315)*	(1.565)	(1.306)	(1.472)
Net depreciation	-0.789	-0.623	2.023	1.339	1.313	0.235
	(0.470)*	(0.416)	(0.592)***	(0.635)**	(0.630)**	(0.570)
Estimator	2SLS	GMM-3SLS	2SLS	GMM-3SLS	2SLS	GMM-3SLS
R^2	0.250	0.106	-0.283	-0.077	-0.019	0.098
Underid. p	0.049**		0.103		0.033	
Overid. p	0.176	0.008*	0.960	0.083*	0.357	0.058*
Weak in. F	4.518**		26.581***		28.069	
Weak id. F (crit.)	3.38 (7.80)		3.91 (7.80)		3.79 (7.80)	
Obs.	23	23	23	23	29	29

† The dependent variable is the natural logarithm of income per worker in the economic sector listed in the top row. All independent variables are expressed in natural logarithms, unless otherwise indicated. A constant term was included in the regressions, but not reported. Heteroskedasticity-robust standard errors with small-sample correction are given in parentheses. Goodness-of-fit measures report the adjusted R^2 (2SLS) or main-equation pseudo R^2 (3SLS). The underidentification test reports the p-value associated with the Kleinbergen-Paap *rk LM* statistic, the overidentification test the p-value associated with the Hansen J statistic (where applicable), and the weak instrument tests are the Anderson-Rubin Wald F and Kleinbergen-Paap *rk F*, with Stock-Yogo critical value corresponding to 25% maximal IV size in parentheses. * indicates significance at the 10 percent level, ** significance at the 5 percent level, and *** significance at the 1 percent level.

The takeaway from this set of results is clear: as one progresses up the value-added chain, the effects of human capital become increasingly larger. In agriculture, the effects are essentially nil. Human capital yields coefficients very similar to the baseline range in terms of manufacturing output, and higher-than-baseline effects in the services sector. If one accepts the premise that the marginal contribution from tertiary education dominates that of primary schooling, this result is entirely in accord with that earlier finding: both manufacturing and services production are much more likely to require a skilled workforce, and hence the human capital contribution is correspondingly larger.[133]

VI.IV The Lasting Legacy of the Cultural Revolution

The first generation of papers that examined path dependent effects tended to rely on historical legacies as a means of identifying causal *effects* of interest,[134] whereas the second generation sought to illuminate the relevant *mechanisms* for persistent outcomes.[135] In this section, I offer some insight into how historical human capital shocks may be also used to identify contemporaneous causal effects.[136]

Up till now, most of the analysis has relied on repeated cross-sections at the provincial level, by necessity: while the political casualties data are available by county, a similar degree of disaggregation is not available for the other variables in (7) and (6), at least not before 1998. Here, I take advantage of the greater granularity available in more contemporaneous data and estimate the system using county-level data for the

[133]Indeed, the size of the coefficient here turns out to be an order of magnitude greater (than in agriculture), which is significantly larger than the secondary or tertiary contributions to overall output documented in Table 6. However, note that these coefficients are not directly comparable to each other, since the dependent variable is different (although the logarithmic transformation of all variables implies elasticities in either case).

[134]Djankov et al., 2003:, "The New Comparative Economics", *op. cit.* (as in n. 69).

[135]Nunn, 2009:, "The Importance of History for Economic Development", *op. cit.* (as in n. 11).

[136]The objective here is, in a sense, more akin to those of the first-generation papers—verifying a causal effect of interest—but to the extent that I rely on shocks from a specific historical event rather than broad historical differences, the approach aligns more with second-generation works.

year 2005,[137] and thereby shed some light on the possible legacy effects of the Cultural Revolution. The results are reported in Table 8, which replicates the six specifications followed in the baseline, but with counties as observations.

Table 8: Legacy effects of human capital on output, 2005†

	(1)	(2)	(3)	(4)	(5)	(6)
				Main		
Human capital	0.458	4.770	1.997	1.091	1.279	1.041
per worker	(0.110)***	(9.440)	(0.511)***	(0.218)***	(0.079)***	(0.069)***
Capital share	1.117	3.539	1.985	1.488	1.013	1.417
	(0.115)***	(5.375)	(0.338)***	(0.185)***	(0.047)***	(0.063)***
Net depreciation	-0.126	0.371	0.052	-0.053	-0.091	-0.125
	(0.066)*	(1.160)	(0.116)	(0.056)	(0.045)**	(0.048)***
				Auxiliary		
Resources			0.231	0.289	0.369	0.405
per worker			(0.068)**	(0.055)***	(0.018)***	(0.018)***
Persecutions		0.004	0.006	-0.002	0.461	1.958
		(0.008)	(0.008)	(0.007)	(0.121)***	(0.070)***
Weather				0.233	0.325	-0.193
shock				(0.037)***	(0.083)***	(0.030)***
Estimator	OLS	IV	2SLS	2SLS	2V-IV	GMM-3SLS
R^2/χ^2	0.271	-7.909	-0.774	0.105	677.197	0.129
Underid. p		0.265	0.066*	0.009***		
Overid. p			0.547	0.053*		138.947***
Weak in. F		3.45*	16.64***	14.82***		
Weak id. F (crit.)		0.25 (5.53)	5.90 (7.25)	25.53 (7.80)		
Obs.	1,814	1,664	1,664	1,583	1,583	1,583

† The dependent variable in the main equation is the natural logarithm of income per worker, and for the auxiliary equations are either human capital per worker (IV/2SLS) or persecutions (2V-IV/3SLS). All independent variables are expressed in natural logarithms, unless otherwise indicated. A constant term was included in the regressions, but not reported. Standard errors clustered at the province level are given in parentheses. Goodness-of-fit measures report the adjusted R^2 (OLS, IV/2SLS), main-equation pseudo-R^2 (3SLS), or the Wald χ^2 (2V-IV). The underidentification test reports the p-value associated with the Kleinbergen-Paap *rk LM* statistic, the overidentification test the p-value associated with the Hansen J statistic (where applicable), and the weak instrument tests are the Anderson-Rubin Wald F and Kleinbergen-Paap *rk* F, with Stock-Yogo critical value corresponding to 25% maximal IV size in parentheses. * indicates significance at the 10 percent level, ** significance at the 5 percent level, and *** significance at the 1 percent level.

The estimates suggest that there is indeed a legacy effect of the Revolution on out-

[137] Since the data are available for the period 1998–2014, I choose 2005 as the midpoint. The appendix presents results for the latest year available (2014); the results described here are essentially unchanged.

put, operating through human capital. This effect is much smaller than in the baseline, as might be expected, since there is no correspondingly large shock, which was the case during 1966. The estimates point to a more or less one-for-one increase in income per worker for every percentage point rise in schooling. These point estimates—especially in the more comprehensive specifications in columns 4–6—are all highly significant. In addition, the contribution from physical capital and labor force growth take on their theoretically-expected signs, and are also generally significant. In some ways, this finding runs against the findings by other researchers,[138] who tend to uncover *negative* legacy effects. However, it is important to understand that I am not comparing Revolution effects between realized outcomes and counterfactuals—as these other approaches typically do—but rather relying on the event as a means of identifying a causal human capital influence.

I am aware that there is some tension between the earlier finding that the human capital effect appears to diminish as the Revolution wore on (as described in Sections V.I and VI.II), and the one here where the effects appear to persist for many generations. But this seeming inconsistency can be reconciled at several levels: theoretically, empirically, and statistically. Since the positive coefficient on human capital simply means that there is a positive relationship between a given educational shock and income, it is theoretically possible that the negative shock that led to an income decline during the Cultural Revolution becomes subsequently reversed, so that a positive shock to schooling efforts later induces an increase in income. In particular, given the generational differences, one could conceive of a situation where Revolution-era workers' incomes suffer a disproportionately large decline due to forgone human capital, which then fades in the aftermath of the crisis; these workers then seek to undo the disadvantage they faced by ensuring that their children accrue more education than they might have otherwise.

[138] Meng and Zhao, 2016:, *op. cit.* (as in n. 74); Bai and Wu, 2017:, *op. cit.* (as in n. 68).

It is also important to realize that the results obtain from not just different time periods, but also different ontological units. It is empirically possible that the insignificant human capital effects from the immediate post-Revolution period obtained from *provincial* data subsequently become better captured in the much larger sample of *county*-level data (recall, the coefficient magnitudes were nontrivial, just imprecisely measured). Moreover, it is inappropriate to carry conclusions obtained from the analysis at one ontological level to another (the ecological fallacy), which provides a statistical basis for the discrepancy.

VII.

Conclusion

This thesis has relied on politically-motivated persecutions of the educated class during the Cultural Revolution as a natural experiment to identify the causal effects of human capital on income. I find that the effects are indeed large: for the period between 1966 and 1971, estimates are in the order of between 7 and 10 percent of provincial income per worker (for every percent change in education). I also find that, for China at least, the effects of human capital are greater for processes further up the value-added chain, and the human capital contribution is most pronounced at the secondary and tertiary levels.

VII.I Implications

Although couched in history, the results have clear implications for policymaking even today. The strongest takeaway is that shocks to human capital, whether unintended or—as in the case of the Cultural Revolution, a predictable side-effect—can be detrimental to future incomes. This central result thus serves as a welcome reminder to policymakers contemplating cuts to educational funding for the purposes of meeting short-term budget objectives: undermining human capital in this manner comes back to bite the economy, in the form of diminished future income growth.

The results also suggest that the strongest concerns over premature deindustrialization may in fact be misplaced, so long as policymakers simultaneously recognize the important role that human capital—especially at secondary and tertiary levels—plays in supporting economic output via the manufacturing and services sector. Examples of the importance of education in such cases abound, from the need for skilled workers in executing advanced manufacturing processes, to the recognition that modern services stress a wider range of cognitive capabilities, to the understanding that knowledge workers require creative thinking to extract the most value from advances in artificial intelligence.

The results also point support the notion that, insofar as the contribution of human capital to income is concerned, the policy focus should be at the secondary and tertiary levels. While this does not negate the importance of a broad base of primary schooling, it does suggest that critiques that university-level education only accrues to the wealthy fail to take into account the overall societal benefits that such institutions of higher learning can have for supporting average incomes.

While the detrimental effect on human capital certainly fades into insignificance in the immediate aftermath of the Revolution, re-examinations of contemporaneous county-level data in Section VI.IV suggest that, in spite of the setback from the Revolution, the contribution of educational attainment remains positive and significant today. If, as a result of the events of the Cultural Revolution, Revolution cohorts subsequently pushed their children toward greater educational attainment, then one unexpected "bonus" from the tragedy would have been a (hard) lesson learned on the importance of schooling.

VII.II Research Limitations and Future Direction

As with all studies reliant on identification through IV, the quality of the estimates ultimately depends on the quality of the instrument. In the paper, I have offered both theoretical justifications as well as empirical checks support the validity of the the relevance and exclusion conditions. I am, however, cognizant of the fact that the weak instrument test results are mixed, and that there is no ironclad test to evaluate the exclusion restriction. In the final analysis, one could remain unconvinced by the strength of the results, although I believe that the qualitative *direction* of the effects are clear, even if one remains unpersuaded by the *magnitude* of the estimated effects.

Since the study relies on a natural experiment in a very circumscribed setting—a mid-century shock to human capital for a large, communist-run economy—it is also subject to questions about external validity. However, given such broad degree of decentralization in within-province economic administration, and significant initial heterogeneity between Chinese provinces (as well as their subsequent growth trajectories), this is likely to be less of a concern. At the very least, the results are certainly relevant to contemporary China—currently the world's second-largest economy—as well as to developing economies at stages of development similar to that of China at the time. Still, questions of whether the results are more broadly applicable to modern, capitalistic economies are likely to remain. Whether the results will hold up in similar quasi-experimental setups in other economies—or even a systematic comparison between different historical episodes within China (such as the Great Leap Forward)—is left for future research. 1

VIII.
Appendix

VIII.I Details of Data Construction

The definitions of variables are given in Table 9. To maximize the sample for the cross-sectional analysis, a number of missing observations were imputed, for specific variables in certain years. These instances are documented below. In all cases, these changes were marginal and did not alter the qualitative findings (particularly since many imputations were for years that were not a focus in the baseline).

- *Total employed persons* for Beijing in 1963, Hubei in 1978, and Guangxi in 1967 were imputed by maintaining a constant urban/rural employment ratio averaged from other available years. Various missing years for Anhui, Chongqing, Inner Mongolia, Liaoning, and Sichuan were interpolated using the series from state-owned enterprise employment, while those for Hubei, Tibet, and Zhejiang were interpolated using the permanent population series.

- *Fixed asset investment* for Fujian and Tibet in various years were interpolated/extrapolated using time.

- *Total enrollment* for Guangdong in 1966–69 and 1971–74, Henan in 1966–69, Hubei in 1966–69 and 1971–74, and Sichuan in 1966–69 and 1971–74 were interpolated using time. Secondary enrollment for Liaoning in 1965 and 1970, and tertiary enrollment in 1969, tertiary enrollment for Jilin in 1969, secondary and tertiary enrollment for Hainan, primary and secondary enrollment for Chongqing, tertiary enrollment for Yunnan from 1969, and tertiary enrollment for Tibet from 1969 were imputed by maintianing a constant primary/secondary/tertiary enrollment ratio averaged from other available years.

- *Teacher count* for Heilongjiang for primary and secondary in 1966–69, and tertiary in 1966–67, and for Ningxia for primary in years prior to 1978 were extrapolated/interpolated using their respective enrollment numbers, with negative extra/interpolations replaced with the pre-extra/intrapolated sample minimum.

- *Educational expenditures* for Sichuan in years before 1973 were extrapolated using time.

- *Retail price index* for Gansu was imputed by maintaining a constant RPI/PPI ratio averaged from other available years. The assumption of no inflation (RPI = 100) was assumed for Anhui in 1967–70, Jiangxi in 1968–70, and Tibet all years prior to 1990.

For county-level data, several variables in Table 9 were not available and were substituted with proxies. These include: the use of total local *government expenditure* instead of educational expenditure for schooling resources, and the use of local *industrial production* instead of fixed asset investment for investment share. The correlation between these substitute measures and the original, at the provincial level, is very high: 0.90 ($p = 0.00$) for the former pair, and 0.94 ($p = 0.00$) for the latter. Moreover, since tertiary enrollment is not available at the county level, these were excluded in the computation of human capital.

The baseline *climate* data are available in 0.5×0.5 grid latitude-longitude segments. These were matched with geocoded provincial/county coordinates by rounding the latter to the closest 0.5 degree-coordinate. For the alternative climate data, temperature and precipitation were matched instead to those corresponding to the provincial capital city.

Table 9: Definitions and sources of variables

Variable	Definition and construction	Data source(s)[†]
	Main dependent variable and alternatives	
Real gross prov. product	Value of final goods and services, deflated by consumer price index	NBSC
Real GPP (alternative)	Real GPP measured by expenditure method	NBSC
Real consumption/worker	Real consumption expenditure, normalized by labor force	NBSC
	Main independent variables and alternatives	
Human capital (flow)	Total student enrollment at primary, secondary, and tertiary levels	NBSC
Human capital (stock)	Total number of full-time teachers in primary and secondary	NBSC
Labor force	Total number of employed persons	NBSC
Investment share	Real fixed asset investment as a share of GPP	NBSC
Investment share (alternative)	Real gross investment as a share of GPP	NBSC
Labor force growth	Rate of growth of employed persons	NBSC
Schooling resources	Real government expenditure on education	NBSC
Schooling resources (alternative)	Average teacher-student ratio for primary and secondary levels	NBSC
Political casualties	Measure of deaths and other victims of Cultural Revolution-related political persecution	Walder (2014)
Climate shock	Average annual surface temperature geolocated to regional centroid	Willmott and Matsuura (2001)
Climate shock (additional)	Average annual precipitation geolocated to regional centroid	ibid.
Climate shock (alternative)	Average annual temperature and total precipitation geolocated to capital city	Tutiempo Network
	Additional controls	
Government expenditure	Local revenue minus expenditure as share of GPP	NBSC
Infrastructure	Average highway length, normalized by provincial area	NBSC and provincial government websites
Health capital	Number of health institutions within province	NBSC
Trade openness	Total imports and exports as share of GPP	NBSC
Financial development	Total loans in financial institutions as share of GPP	NBSC
Population growth	Rate of growth of total provincial population	NBSC

[†] NBSC = National Bureau of Statistics China.

VIII.II Additional Tables

Table 10 reports standard summary statistics for the baseline year (1966) and additional robustness year (1970), along with those for the full panel (1951–2014). All data are at the provincial level (county-level statistics are available on request).

As mentioned in the main text, a number of additional robustness experiments were considered, but space considerations precluded their detailed discussion. In this appendix, I report these additional checks, accompanied with some rudimentary commentary.

The first of these checks involves replacing the source of the weather instrument, with an alternative from the *Tutiempo Network*. Tutiempo compiles historical weather-station data for many of the larger towns and cities in China. Due to coverage considerations, it is not possible to map these to the provincial centroid. However, since educated individuals are typically concentrated in capital cities, this alternative geolocation approach offers a nice alternative for matching the weather instrument with the provinces. An additional drawback of these data are that, unlike the ibid. data used in the baseline, weather-station data rely on actual observations and exclude imputations. The upshot of this constraint is that temperature data are only available from 1973 onward, and precipitation data from 1975. While this is not ideal, the fact that these rely on actual weather records is a significant positive, and again offers a useful alternative to the baseline.

Table 11 reports these results for 1973 (columns 1–3) and 1975 (columns 4–6), for the final three specifications of the baseline. Although the precision of the point estimates are less definitive than in the baseline, the human capital effect remains, by and large, positive and significant, especially for the results for 1973. Those for 1975 suffer from micronumerosity (results are based on only 11 observations) and, perhaps more importantly, reflect the diminution of the human capital effect that is consistent with that of the baseline in 1975.

Table 10: Summary statistics for main variables of interest, by selected cross-sectional years (1966 and 1970) and full panel (1951–2014)†

Variable	**N**	**Mean**	**Std Dev**	**Min**	**Max**
		1966			
Output per worker	30	1.808	0.571	0.807	3.332
Human capital per worker	31	0.344	0.117	0.105	0.652
Capital share	30	0.170	0.170	0.037	0.927
Net depreciation	31	0.077	0.227	-0.662	0.704
Resources per worker	30	0.169	0.088	0.000	0.349
Persecutions	29	12.602	1.136	9.784	14.251
Weather shock	31	2.801	0.576	0.677	3.428
		1970			
Output per worker	30	1.882	0.576	1.081	3.370
Human capital per worker	30	0.341	0.100	0.198	0.565
Capital share	30	0.200	0.190	0.045	1.083
Net depreciation	31	-0.026	0.465	-2.270	0.291
Resources per worker	30	0.150	0.076	0.009	0.363
Persecutions	29	12.602	1.136	9.784	14.251
Weather shock	31	2.765	0.601	0.378	3.400
		1951–2014			
Output per worker	1,856	3.219	1.755	0.121	7.859
Human capital per worker	1,810	0.314	0.103	0.012	0.906
Capital share	1,944	0.256	0.168	0.000	1.097
Net depreciation	1,917	0.063	0.165	-2.803	1.002
Resources per worker	1,686	0.678	0.747	0.000	3.601
Persecutions	2,019	12.608	1.120	9.784	14.251
Weather shock	465	2.789	0.570	0.010	3.428

† Top and middle panels correspond to cross-sections at the provincial level, and bottom panel to full unbalanced panel at the provincial level. Actual statistics may vary depending on the available sample for a given specification.

Table 12 adds a wide range of structural determinants as an additional robustness check. These are largely drawn from the cross-country growth literature. These

Table 11: Additional robustness results for effects of human capital on output (with alternative temperature instruments), 1973 and 1975†

	(1)	(2)	(3)	(4)	(5)	(6)
				Main		
Human capital	12.056	17.032	17.137	0.928	-7.503	0.507
per worker	(7.667)	(10.283)*	(3.962)***	(3.051)	(3.455)**	(1.678)
Capital share	2.549	-2.292	3.912	6.153	2.060	6.880
	(4.313)	(0.879)***	(4.192)	(3.693)	(0.884)**	(3.704)*
Net depreciation	-4.036	-0.120	-2.646	0.166	0.281	0.219
	(2.545)	(0.922)	(2.080)	(0.090)*	(0.028)***	(0.168)
				Auxiliary		
Resources	0.251	0.368	0.600	-0.559	-0.652	-0.765
per worker	(0.412)	(0.235)	(0.241)**	(0.517)	(0.286)**	(0.455)*
Persecutions	-0.004	0.008	0.075	0.000	-0.098	-0.163
	(0.025)	(0.009)	(0.039)*	(0.034)	(0.034)***	(0.081)**
Weather (temp.)	-0.047	1.091	0.876	-0.357	-0.790	1.415
	(0.074)	(0.446)**	(0.407)**	(0.101)**	(0.377)**	(0.709)**
Weather (precip.)				0.122	0.291	0.010
				(0.078)*	(0.155)*	(0.420)
Year	1973	1973	1973	1975	1975	1975
Estimator	2SLS	2V-IV	GMM-3SLS	2SLS	2V-IV	GMM-3SLS
R^2/χ^2	-2.966	9.746	-6.314	-0.121	125.090	0.203
Underid. p	0.379			0.484		
Overid. p	0.695		0.057*	0.166		0.044**
Weak in. F	26.53***			144.32		
Weak id. F (crit.)	1.03 (7.80)			3.37 (8.31)		
Obs.	28	28	28	11	11	11

† The dependent variable in the main equation is the natural logarithm of income per worker, and for the auxiliary equations are either human capital per worker (IV/2SLS) or persecutions (2V-IV/3SLS). All independent variables are expressed in natural logarithms, unless otherwise indicated. A constant term was included in the regressions, but not reported. Bootstrapped (OLS) or heteroskedasticity-robust (IV, 2SLS, and 3SLS) standard errors with small-sample correction are given in parentheses. Goodness-of-fit measures report the adjusted R^2 (OLS, IV/2SLS), main-equation pseudo R^2 (3SLS), or the Wald χ^2 (2V-IV). The underidentification test reports the p-value associated with the Kleinbergen-Paap rk LM statistic, the overidentification test the p-value associated with the Hansen J statistic (where applicable), and the weak instrument tests are the Anderson-Rubin Wald F and Kleinbergen-Paap rk F, with Stock-Yogo critical value corresponding to 25% maximal IV size in parentheses. * indicates significance at the 10 percent level, ** significance at the 5 percent level, and *** significance at the 1 percent level.

include infrastructure capital,[139] health capital,[140] financial development,[141] and demographics.[142] Following the structure of Table 2 in the main text, each additional regressor is included and estimated via 2SLS and 3SLS. For health capital, since it is both intrinsic to both human capital formation as well as a direct influence (via externalities) on income, columns 3–6 consider alternately embedding health in either equation. The final column presents a combined specification that includes all of the above controls in the income equation.

Although the sample size occasionally falls significantly, the overall message form these further robustness checks is that the human capital effect remains robust, and the coefficient ranges are broadly comparable to those in the baseline and main-text robustness tables. It is worth noting that when the full complement of controls are included, the coefficient on human capital falls markedly, as may be expected. It nevertheless remains fairly large, and several times in magnitude relative to the other determinants in the augmented Solow model.

The main text adopted 2005 as the contemporary year to analyze legacy effects, on the basis that this is the midpoint for years for which the full set of county-level data are available. As a third robustness check, Table 13 replicates the results from Table 8 of the main text with the latest available year, 2012. The coefficient sizes are marginally smaller, but most specification retain their strong statistical significance

[139]Cesar A. Calderón, Enrique Moral-Benito and Luis Servén, "Is Infrastructure Capital Productive? A Dynamic Heterogeneous Approach", *Journal of Applied Econometrics*, 30: no. 2 (March 2015).

[140]Philippe M. Aghion, Peter W. Howitt and Fabrice Murtin, "The Relationship Between Health and Growth: When Lucas Meets Nelson-Phelps", *Review of Economics and Institutions*, 2: no. 1 (Winter 2011).

[141]Ross Levine, Norman V. Loayza and Thorsten Beck, "Financial Intermediation and Growth: Causality and Causes", *Journal of Monetary Economics*, 46: no. 1 (August 2000).

[142]Michael E. Conroy, "Population Growth, Life-Cycle Saving, and International Differences in Steady-State Optimal Saving Rates", *Demography*, 16: no. 3 (August 1979).

Table 12: Additional robustness results for effects of human capital on output (with additional controls), 1966 and 1970†

	(1)	(2)	(3)	(4)	(5)	(6)
Human capital	6.500	6.707	8.201	9.981	5.437	9.015
per worker	(1.943)***	(1.402)***	(2.219)***	(6.074)	(1.614)***	(3.077)***
Capital share	1.229	-0.610	2.178	-2.063	0.654	0.170
	(1.196)	(1.062)	(1.186)*	(3.523)	(0.792)	(2.272)
Net depreciation	0.774	0.234	0.007	-0.028	0.336	0.093
	(0.457)	(0.342)	(0.497)	(0.277)	(0.385)	(0.207)
Variation:						
Add. indep?	Infrastructure	Infrastructure	Health (inc)	Health (inc)	Health (hum)	Health (hum)
Year	1966	1970	1966	1970	1966	1970
Estimator	2SLS	GMM-3SLS	2SLS	GMM-3SLS	2SLS	GMM-3SLS
R^2	0.108	0.071	0.087	-1.211	0.382	-0.492
Obs.	26	26	23	27	23	27
	(7)	(8)	(9)	(10)	(11)	(12)
Human capital	2.525	3.925	5.339	9.263	2.029	2.995
per worker	(0.832)***	(2.035)*	(1.594)***	(2.977)***	(0.241)***	(0.588)***
Capital share	-1.353	-1.118	1.062	0.410	-1.273	-0.654
	(0.442)***	(1.207)	(0.803)	(1.757)	(0.295)***	(0.402)
Net depreciation	0.231	-0.075	0.619	-0.052	-0.204	-0.088
	(0.192)	(0.075)	(0.475)	(0.150)	(0.040)***	(0.069)
Variation:						
Add. indep?	Fin. dev.	Fin. dev.	Pop. growth	Pop. growth	All	All
Year	1966	1970	1966	1970	1966	1970
Estimator	2SLS	GMM-3SLS	2SLS	GMM-3SLS	2SLS	GMM-3SLS
R^2	0.885	0.729	0.476	-1.061	0.975	0.912
Obs.	25	27	28	27	18	25

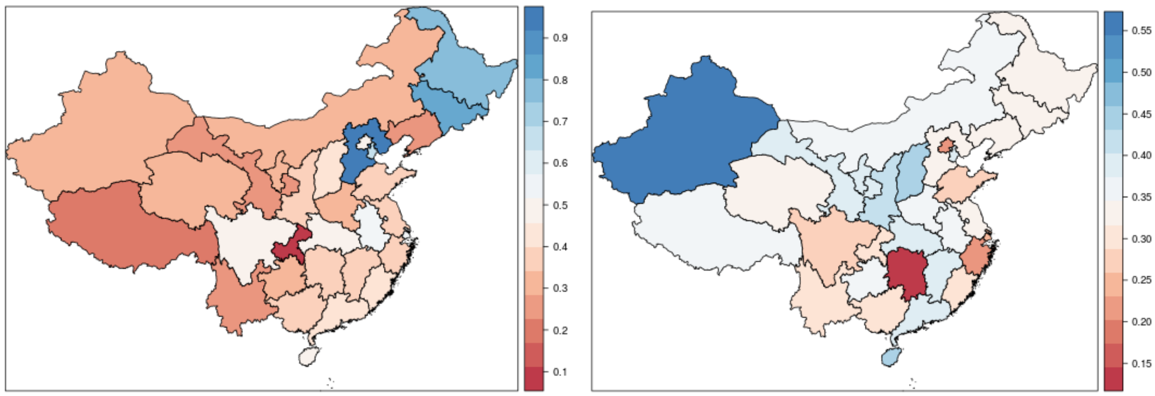
† The dependent variable is the natural logarithm of income per worker. All independent variables are expressed in natural logarithms, unless otherwise indicated. A constant term was included in the regressions, but not reported. Heteroskedasticity-robust standard errors with small-sample correction are given in parentheses. Goodness-of-fit measures report the adjusted R^2 (2SLS) or main-equation pseudo R^2 (3SLS). * indicates significance at the 10 percent level, ** significance at the 5 percent level, and *** significance at the 1 percent level.

and positive sign, and the postestimation IV tests are modestly better. The qualitative conclusions drawn in the main text are, however, unaltered.

Table 13: Legacy effects of human capital on output, 2012†

	(1)	(2)	(3)	(4)	(5)	(6)
				Main		
Human capital	0.198	-20.410	1.237	0.721	0.889	0.732
per worker	(0.097)*	(387.676)	(0.324)***	(0.174)***	(0.064)***	(0.056)***
Capital share	0.670	-5.203	0.981	0.854	0.821	0.871
	(0.093)***	(110.387)	(0.155)***	(0.140)***	(0.038)***	(0.043)***
Net depreciation	-0.452	-4.531	-0.246	-0.348	-0.341	-0.342
	(0.139)***	(76.836)	(0.191)	(0.173)**	(0.086)***	(0.086)***
				Auxiliary		
Resources			0.372	0.440	0.484	0.519
per worker			(0.063)***	(0.047)***	(0.017)***	(0.017)***
Persecutions		-0.000	0.005	-0.001	0.414	0.475
		(0.009)	(0.008)	(0.006)	(0.121)***	(0.030)
Weather				0.217	0.311	0.322
shock				(0.050)***	(0.087)	(0.038)***
Estimator	OLS	IV	2SLS	2SLS	2V-IV	GMM-3SLS
R^2/χ^2	0.179	-243.173	-0.427	0.048	564.776	0.041
Underid. p		0.265	0.002***	0.000***		
Overid. p			0.338	0.042**		168.376***
Weak in. F		1.51	27.33***	22.45***		
Weak id. F (crit.)		0.00 (5.53)	17.46 (7.25)	34.64 (7.80)		
Obs.	1,762	1,618	1,664	1,543	1,543	1,543

† The dependent variable in the main equation is the natural logarithm of income per worker, and for the auxiliary equations are either human capital per worker (IV/2SLS) or persecutions (2V-IV/3SLS). All independent variables are expressed in natural logarithms, unless otherwise indicated. A constant term was included in the regressions, but not reported. Standard errors clustered at the province level are given in parentheses. Goodness-of-fit measures report the adjusted R^2 (OLS, IV/2SLS), main-equation pseudo-R^2 (3SLS), or the Wald χ^2 (2V-IV). The underidentification test reports the p-value associated with the Kleinbergen-Paap *rk LM* statistic, the overidentification test the p-value associated with the Hansen J statistic (where applicable), and the weak instrument tests are the Anderson-Rubin Wald F and Kleinbergen-Paap *rk F*, with Stock-Yogo critical value corresponding to 25% maximal IV size in parentheses. * indicates significance at the 10 percent level, ** significance at the 5 percent level, and *** significance at the 1 percent level.



(a) 1966

(b) 2004

Figure 6: Distribution of human capital per worker, 1966 (left panel) and 2004 (right panel). In 1966, human capital was highest in the northeast, with regions in the south exhibiting moderate levels, and inland regions with the lowest. There has been a significant shift in provincial human capital over time, and by 2004, many of the formerly above-median-education provinces were no longer so. Remarkably, provinces in the northeast and southeast region—where political persecutions were significant—appear to have displayed some of the greatest retrogression over time.

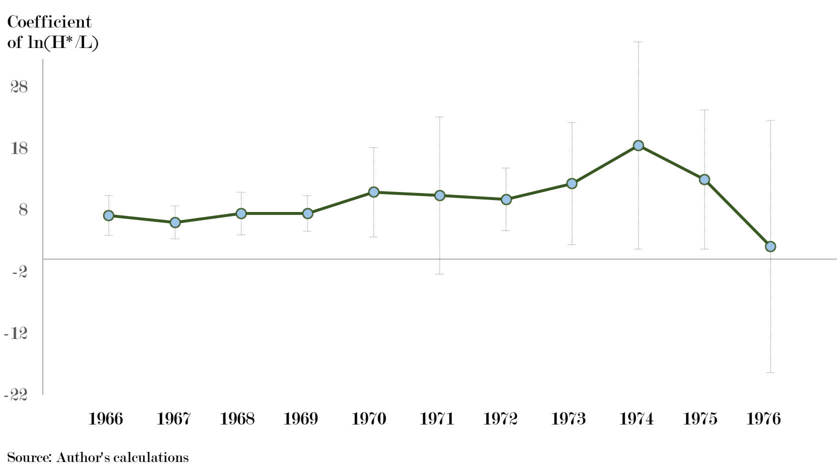


Figure 7: Coefficients (blue markers) with associated 95 percent confidence bands (gray capped lines) for human capital, repeated cross-sections, 1966–76. Estimates are obtained using 3SLS, including both persecutions and surface temperature as instruments. The evolution of point estimates over time are very similar to those obtained using 2SLS.

IX.

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