Three Essays on Happiness Economics:

Insights from China

Youxing Zhang

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I confirm that the work submitted is my own, except where work which has formed part of jointly authored papers has been included. My contribution and the other authors to this work has been explicitly indicated below. I confirm that appropriate credit has been given within the thesis where reference has been made to the work of others.

(i) Chapter 1 is based on work from the following jointly authored paper:

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The end of the PhD journey is just a new beginning. I know the road ahead will not be easy, but I will move forward fearlessly as I know that all these lovely people will stand by my side.

Abstract

This thesis consists of three self-contained empirical papers. They address different topics, but all relate to the economics of happiness within a Chinese context. The first chapter examines whether the publicity of anti-corruption investigations affects individuals' life satisfaction. It shows that, at the population level, such information is modestly detrimental to citizens' life satisfaction. More importantly, this effect is significantly moderated by the levels of one's trust in the government. I show that it is harmful to individuals with low trust, but beneficial for those with high trust. This study thus highlights an unintended consequence of certain government policies: when political trust is low, these efforts may reduce subjective well-being by increasing citizens' awareness of the social issues, such as corruption, that the policies seek to address.

The second chapter studies the relationship between individual life satisfaction and political trust. Employing an instrumental variables method, I find that life satisfaction has a significant, strong and positive impact on the formation of political trust. I also provide evidence suggesting that political trust matters for regime stability in China. Taken together, my findings imply that, even in authoritarian regimes, governments will have an incentive to improve citizens' happiness.

The final chapter addresses whether early life adversity impacts the formation of individual resilience. Using a difference-in-difference identification strategy, I find that individuals who experienced extreme starvation in their childhood exhibit a lower reduction in mental well-being when faced with the Covid-19 pandemic. I also find a non-linear relationship between early life adversity and future resilience. Simply put, some adversity appears beneficial for resilience but too much may backfire.

Table of Contents

List of Tables

Lists of Figures

Introduction

General background

Happiness economics has experienced rapid development over the past few decades (Clark 2018). Firstly, given that maximizing people's happiness is the main goal of socio-economic development, it is not surprising to observe an explosive growth in the literature answering the question of 'what makes people happy' (e.g. Dolan et al. 2008; Luttmer 2005). This, in turn, has incurred extensive research on the measurement of happiness (Weimann et al. 2015). On the other hand, subjective well-being data has offered new approaches to addressing many economic puzzles, for example, how much citizens value public goods such as air quality (Luechinger 2009) and whether individuals effectively choose to be unemployed (Clark and Oswald 1994). Furthermore, there is an increasing literature suggesting that, apart from being a consequence of people's actions, subjective well-being could be a driving force behind many important socio-economic behaviours ranging from productivity (Oswald et al. 2015) to the decision to get divorced (Clark et al. 2008).

However, most of these advancements have been made in studies focused on developed or democratic countries, less is known when it comes to developing or non-democratic countries such as China. The unique cultural and institutional environments in those countries present a great opportunity for generating new insights into happiness economics. For instance, in non-democratic countries, there may be a larger disparity between the ruler' objectives and public interests. Can we assume that their governments, like those in democracies (e.g. Ward 2020), also have incentives to maximize citizens' happiness? We know from extant studies that corruption has detrimental impacts on individual well-being (e.g. Tay et al. 2014), but what about anti-corruption policies? Does it lead to a happier society or by highlighting the very problem it seeks to address foster unhappiness? Almost everyone has heard the famous saying of 'What does not kill you makes you stronger', but is this really true? Can we empirically examine it using happiness data? Motivated by these questions, my thesis empirically investigates three independent issues from the perspective of happiness economics, with a focus on China which offers distinct institutional and historical contexts.

Research questions

My thesis discusses three different topics, each constituting an independent chapter. Here I introduce the research questions one by one, accompanied with some additional context for each topic. The first chapter investigates how an anti-corruption campaign in China affects citizens' subjective well-being. Corruption remains one of the most concerning social issues in China, and since the new leaders took office in 2012, there has been a noticeable increase in efforts and publicity surrounding government anti-corruption movements. I am particularly interested in how the disclosure of anti-corruption investigations on high-ranking government officials influences people's life satisfaction. It seems intuitive that individuals may perceive it as a signal that the government is strengthening its efforts against corruption, leading to positive expectations and thereby an increased life satisfaction. This presumably is what the ruling party hopes. However, it can also be seen as an exposure of government corruption, leading the public to believe that corruption is more pervasive than they had initially thought. This is likely to reduce citizens' life satisfaction. It is thus empirically unclear whether such disclosures damage or promote individual life satisfaction.

The second chapter examines the impact of subjective well-being on political trust. Many studies have found that, in democracies, citizens tend to vote for incumbents when they are more satisfied with their life (Liberini et al. 2017; Ward 2020). This suggests that democratic governments have an electoral incentive to enhance citizens' happiness. A pertinent question, then, is whether the governments in authoritarian regimes, where elections is less a factor, have similar incentives. I propose that political trust could be an alternative tool to voting for incentivizing the government, considering its importance for regime stability even in autocracies (Chen et al. 2021; Fairbrother 2019). The logic is that, if the levels of citizens' political trust are conditional on their happiness, then governments in authoritarian regimes would also seek to enhance citizens' subjective well-being for the purpose of maintaining its rule. This objective of this paper thus is to establish a causal link between citizens' happiness and political trust.

In the final chapter, I study whether early life adversity influences the formation of individual resilience. The outbreak of Covid-19 pandemic has caused considerable negative impacts on people's lives, particularly when it comes to mental health (e.g. Gao et al. 2020; Song 2020). This underscores the importance of resilience, which is defined as an individual's ability of bouncing back from or adapting to adverse environments (Bonanno 2004; Rutter 2012). While most studies indicate that early life adversity could generate long-term negative impacts on many individual life outcomes, some psychological theories suggest that it may also foster resilience (e.g. Dienstbier 1989). To investigate this issue, I examine whether experiencing extreme starvation in childhood affects individuals' ability to cope with the Covid-19 pandemic, through the lens of mental well-being.

Methodologies

I employ quantitative methods to investigate the aforementioned research questions and look to establish causal relationships. For this purpose, I use different identification strategies in the three chapters. For the first chapter, I focus on the disclosures of anti-corruption investigations on senior government officials. Given China's political circumstance, such disclosures are largely exogenous to the general population. I also provide evidence that these disclosures are unrelated to many important regional socioeconomic indicators. Therefore, I rely on a fixed-effects regression to evaluate the impact of these disclosures on individual life satisfaction, coupled with a number of robustness checks.

In the second chapter, I leverage an instrumental variables (IV) method to identify the impact of life satisfaction on individuals' political trust. Specifically, I instrument life satisfaction with three mutually independent variables: one's physical attractiveness score, a short-term health shock and the closeness of within-family relationships. My key argument is that, if any one of these IVs fails to meet the exclusion condition, the resulting estimate would be biased. Consequently, one would anticipate obtaining divergent results when using different IVs. Conversely, consistent outcomes across all IVs, which is what we observe, would indicate that all IVs are valid.

Relying on two exogenous events, my third chapter employs a difference-in-difference (DID) approach to investigate how early life adversity affects the formation of individual resilience. Following an increasingly common practice in this field (e.g. Johnston et al. 2021), I measure resilience by examining the changes in one's mental health before and after the outbreak of the Covid-19 pandemic. The pandemic clearly serves as a credibly exogenous shock to individuals. On the other hand, the early life adversity I use is a self-reported extreme starvation episode (ESE) during the China's Great Famine. This is also a largely exogenous event to individuals because the famine happened about 60 years prior to the Covid-19 pandemic and was caused mainly by natural calamities and government policies failure (Li and Yang 2005; Meng et al. 2015).

I then estimate the difference in the change of life satisfaction before and during the Covid-19 pandemic between individuals with and without childhood ESE, using a DID approach.

Findings

In the first chapter, I find that, overall, the disclosure of anti-corruption investigations targeting senior government officials has a negative impact on individual life satisfaction. However, this effect exhibits significant heterogeneity and is strongly moderated by the level of one's political trust. Specifically, as the level of political trust increases, the negative effect gradually diminishes and even turns positive at high levels of political trust. My interpretation is that how individuals interpret the information of anti-corruption investigations is determined by their pre-existing beliefs about the government, in line with a pattern of 'confirmation bias' (Lord et al. 1979).

In the second chapter, the IV estimation shows that individuals' life satisfaction has a substantial and positive impact on their political trust. I also find suggestive evidence that political trust is negatively correlated with individuals' intention to participate in protest activities when they feel unfairly treated by the government. This implies that, in an authoritarian regime, the government may also have incentive to improve the happiness of the general public.

The analysis in the third chapter shows that individuals who experienced extreme starvation in childhood perform better during the Covid-19 pandemic in terms of mental health, compared to those without such experience. Their life satisfaction as well as other mental health indicators were less affected during the pandemic. I attribute this to enhanced resilience by ruling out the competing explanation that this effect is due to mortality selection (i.e. more resilient individuals surviving the famine). Additionally, I find that the relationship between early adversity and resilience is nonlinear: moderate adversity fosters resilience, but this positive effect diminishes as the level of adversity increases.

Contributions

The three China-based studies contribute to happiness economics and other important economic debates in a number of ways. The first chapter firstly makes a contribution to the study of corruption and subjective well-being. Although much is known about the direct impacts of corruption on happiness (e.g. Tay et al. 2014; Helliwell et al. 2018;

Sharma et al. 2021), little attention has been paid to the consequences of anti-corruption policies. I am the first to investigate how disclosures of anti-corruption investigations affect citizens' subjective well-being. By focusing on investigations of high-ranking government officials, which are largely exogenous to individuals, I can neatly isolate the effect I am interested in from the impacts of corruption itself. More importantly, I uncover the moderating role of political trust, aligning with the confirmation bias theory in psychology (Lord et al. 1979; Wilson 2014).

Second, the first chapter also enriches existing research on the effects of China's anti-corruption campaign over the past decade. Previous studies have mainly focused on its impact on corporate behaviours, such as company innovation, rent-seeking, and compliance with safety regulations (Chen and Kung 2019; Xu and Yano 2017; Xu et al. 2021). I add to this literature by highlighting its impact on an individual life outcome, namely happiness. Third, this study contributes to the research on the consequences of anti-corruption information disclosure, which has primarily explored its influence on citizens' voting behaviour (Ferraz and Finan 2008). From a practical perspective, my findings reveal that there may be a disincentive for governments to address certain issues if doing so makes the problem more salient to citizens.

The contributions of the second chapter are threefold. Firstly, I provide evidence that, even under authoritarian regimes, governments may have incentives to enhance citizens' subjective well-being. This adds to the 'happy voter' literature, which has primarily focused on democratic countries (Liberini et al. 2017; Ward 2020). Second, my study also contributes to the research on the origins of political trust (Abramson et al. 2022; Acemoglu et al. 2020; Yao et al. 2022), by identifying subjective well-being as a key driving force. Last but not least, this chapter speaks to an emerging literature suggesting that subjective well-being can predict many individual outcomes such as productivity (Oswald et al. 2015) and job-seeking behaviours (Krause 2013; Gielen and van Ours 2014). Specifically, I contribute to this research by highlighting its role in shaping political trust.

The final chapter makes two key contributions. First, it contributes to the emerging literature exploring the origins of individual resilience. Although resilience has been shown to have a significant impact on many individual behaviours (e.g. Law et al. 2014; Scales et al. 2006; Schuler 2017) and may yield substantial economic benefits (Asheim et al. 2020), there has been limited investigation into its formation. Using a quasi-experimental approach, I identify the causal effect of early adversity on individual resilience. Second, this chapter supplements the existing literature examining the long-term effects of childhood experiences on later life outcomes (Almond et al. 2018; Hoynes et al. 2016; Sviatschi 2022). Existing studies in this field mainly suggest that early life adversity could generate long-term negative consequences on various individual outcomes (Chetty and Hendren 2018 Kesternich et al. 2014; Kesternich et al. 2015). My study however indicates that it may also foster specific positive effects (namely enhanced resilience), in line with the ‘steeling effect’ theory in psychology. It is worth noting that I have no intention to praise hardship, instead, my study underscores human’s ability of adapting to adverse environments and such ability may be strengthened during that adaption process.

Chapter 1 'Tiger-Hunting' and Life Satisfaction: A Matter of Trust

Abstract

Through anti-corruption prosecutions, governments will often look to publicly signal their efforts in tackling corruption as a way of garnering political support. Combining data on the public disclosure of anti-corruption efforts and individual well-being in China, we show that such signals may increase the salience of the issue in question and hence diminish the life satisfaction of citizens. We show that these effects are heavily dependent on individual's political trust. The core implication of these findings is that signalling efforts may have unintended negative consequences on population well-being and thus political support, particularly when faced with low political trust.

1.1 Introduction

There is a widely circulated story in the famous Chinese classic novel *All Men Are Brothers*: a young man named *Wu Song*, by his bare hands, killed a ferocious tiger that had hurt many people which made him a well-known folk hero. ‘Hunting tigers’ has since become a symbol reflective of efforts aimed at getting rid of any evil in Chinese culture which is why the contemporary Chinese government has dubbed the corruption investigations of senior officials as ‘Tiger-Hunting’ (in what follows, TH; Jing-Schmidt and Peng 2017). Clearly eliminating an imminent danger to life would improve local residents’ well-being. The question we pose is whether hunting down corrupt senior government officials, and in particular the public disclosure of such efforts, leads to a more satisfied society?

Corruption is a widespread phenomenon particularly in developing and emerging economies and recognised as an important barrier towards economic development (Mauro 1995; Aidt 2009; Grundler and Potrafke 2019).[1] It lowers human capital accumulation, increases fiscal deficits and poverty (Dimant and Tosato 2018), discourages investment on public goods (Beekman et al. 2014), damages the effectiveness of the legal system and, in the process, reproduces itself (Herzfeld and Weiss 2003). It is not too surprising therefore to note that there is ample evidence indicating corruption itself does have a detrimental impact on people’s life satisfaction (e.g. Sharma et al. 2021). Accordingly, one would expect anti-corruption efforts to be beneficial. We argue, however, that this relationship is not clear-cut, particularly when it comes to the public disclosure of anti-corruption efforts. Our core argument is that information pertaining to anti-corruption investigations can be seen in both a positive and a negative light. On the one hand, it can signal a government’s commitment towards cracking down on corruption; on the other hand, by highlighting corruption, some citizens may come to believe that corruption is more pervasive than they had initially thought. A priori, it is therefore unclear what these different effects imply for individual well-being overall. As we discuss later, we propose this will depend on the level of political trust.

[1] Theoretically, corruption may also ‘grease the wheel’ of economic development (e.g. Lui 1985), but the empirical evidence that corruption lowers economic growth overall is overwhelming (Dimant and Tosato 2018).

The downfall of many senior provincial officials during the recent anti-corruption campaign in China provides us with an ideal setting to empirically examine this issue.[2] Except for some limited elections at the village level in rural areas (Martinez-Bravo et al. 2022), officials in the Chinese government at all levels are appointed by upper-level bureaucrats who themselves are government appointees. In correspondence with this, the anti-corruption campaign in China is principally a top-down movement, leaving the public with little to no influence on the decision about which high-ranking officials are to be investigated. Crucially for our purposes, every single TH event is officially disclosed and propagated widely in the media, ensuring the public are made aware of the government's anti-corruption efforts. Adding to that, there is a long-standing Chinese culture of 'Official-Oriented Thought'[3] (Gao 2016), which means that any information relating to senior (provincial) officials will inevitably attract attention. Although the campaign is nationwide and all provinces have had senior officials investigated, the frequency of TH varies considerably across provinces and years.

Matching data about the distribution of TH events across provinces with a longitudinal geo-referenced survey, we estimate the well-being effect of individuals' exposure to TH disclosures concerning high-ranking officials in their province. Quantitatively, this highest part of the provincial government makes for only a trivial fraction of the total number of anti-corruption investigations during the campaign, which means that the number of TH events does not represent the actual corruption level of a province.[4] Instead, we believe there is a lot of randomness in the distribution of TH events across provinces for reasons rooted in the Chinese political system (see more detailed discussion in Section 1.2).

We begin our analysis by discussing the Chinese government's efforts to widely disseminate the investigation of high-ranking officials from the perspective of signalling theory. Signalling theory captures the idea that the government or other entities (e.g. firms, individuals) may choose to send a costly signal to convince others (the receiver)

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[2] We focus on TH events of senior provincial officials as they are propagated and as these officials belong to the political elite of China. They constitute the top tier of geographical delegation of power (there are five levels below the central government). Except for a few cases, investigations of lower level officials are not publicly reported. The central government publicises only the aggregate numbers of anti-corruption investigations for each province (i.e. covering all levels of officials).

[3] Official-Oriented Thought, or Official Rank Standard, refers to a worship of government officials, particularly senior government officials. Any news or anecdotes involving senior officials are always a hot topic of conversation among the general public.

[4] According to an official report, between December of 2012 and June of 2017, more than 1.14 million party members and cadres at town-level (and below) have been punished for corruption-related issues (Chen 2017). In contrast, only 135 provincial-level officials were investigated during our sampling period (2010 to 2018).

of certain desirable qualities (Spence 1973). In this context, anti-corruption campaigns, and in particular the public disclosure of anti-corruption investigations, can be seen as an effort by the ruling party to sustain political support (Kang and Zhu 2021). TH disclosures are a costly signal due to both the cost of TH itself and the efforts to widely promote and disseminate knowledge of these events amongst the general public. The Chinese government may intend for the public disclosure of TH events to be seen as a positive signal of its commitment to tackling a social issue of growing importance, giving rise to what we name as a 'countering effect' on what the public believe is the prevalence of corruption. Simply put, if people see high-ranking officials prosecuted on corruption charges, they may expect that corruption will reduce. In line with the assumed intention of the government, the public may receive this as good news that should enhance their life satisfaction.

Nevertheless, the public disclosure of TH events may also be interpreted negatively. For example, due to a lack of objective knowledge about the actual level of corruption, the public may learn through the disclosure of TH events that corruption is more pervasive than they previously thought. Increasing the salience of corruption through TH may thus diminish life satisfaction ('salience effect'). What is more, some citizens may see TH events as an effort by the ruling party to remove provincial high-ranking officials that have fallen into disfavour with the central government for reasons unrelated to corruption.

As a result of these considerations, we put forward political trust as key to understanding the impact of TH disclosures on life satisfaction. There is an oft-commentated upon tendency for people to interpret new information in a way that supports their own existing beliefs, known as confirmation bias (Lord et al. 1979). If individuals believe the government is trustworthy, we argue that they will likely focus on the positive aspects of the signal when exposed to TH disclosures as that is in keeping with their existing perception of the government. The opposite may be true for individuals with low political trust.

In order to estimate the impact of TH events at the provincial level for individual life satisfaction, we match information capturing the public disclosure of TH events across provinces with survey data from the China Family Panel Studies (CFPS). This enables us to use an individual-fixed effect model controlling for any time-invariant individual and provincial characteristics in the estimation of life satisfaction. One may argue that TH events are unrelated to provincial characteristics as the decision to investigate high-ranking officials is largely influenced by their personal political ties (we discuss this in more detail in Section 1.2). Additionally, high-ranking officials are regularly rotated across provinces, further breaking the link between provincial characteristics and the decision to investigate an individual on corruption charges. Nonetheless, we include a number of time-variant provincial characteristics as control variables in our empirical model. The empirical model is further augmented with a number of time-variant individual characteristics which allow us to analyse effect heterogeneity using interaction variables.

Our results demonstrate that, on average, TH disclosures are negatively associated with individual life satisfaction. We show how this main effect masks considerable heterogeneity with political trust playing an important moderating role. As an illustration, for those with high political trust, we find that TH disclosures are associated with an increase in life satisfaction, but for those with low political trust, it appears that TH disclosures have the opposite effect. We also find that the estimated effect of TH events varies significantly across socio-demographic groups with older individuals as well as those with a rural-*Hukou* status responding more positively to TH disclosures in their province.

These findings are supported by a number of additional analyses. We first underpin the assumption that TH is an exogenous event at the provincial level by showing that TH is unrelated to a series of province-level variables capturing differences in economic and social indicators across provinces. The same applies to recent trends in these indicators. Crucially, TH is also unrelated to past, current and future actual levels of corruption in a province as well as recent changes in that level. We also show that the public disclosure of anti-corruption investigations is unrelated to people's individual experience of corruption which again supports our argument that the well-being impact of TH is driven largely through signalling effects. In further support of 'signalling effects', we also demonstrate that TH disclosures has a more pronounced negative impact for those who are more likely to obtain information through the internet, which we suggest can expose people to TH disclosures to a larger extent. Finally, we confirm that our results are robust to different empirical modelling approaches and different measures of TH.

While the relationship between corruption and citizens' well-being has been widely studied (Tay et al. 2014; Wu and Zhu, 2016; Sulemana et al. 2017; Helliwell et al. 2018; Li and An 2019; Sharma et al. 2021), the well-being impact of related policy measures such as anti-corruption campaigns is largely unknown. The only study we are aware of that is close to our own is the work carried out by Sharma et al. (2021), who examine the relationship between corruption and mental health in Vietnam. Among other things, this study reported how the average change in mental health between 2016 and 2018 was more positive in high-corruption regions relative to other regions. They relate this result to Vietnam's 2016 anti-corruption campaign. Presumably, high-corruption regions were exposed more strongly to the campaign than low-corruption regions, which in turn could be the reason for the difference in the change of mental health over time. The present study substantively differs from Sharma et al. (2021) as we are able to directly measure the disclosure of anti-corruption investigations across provinces. Additionally, we are principally concerned with the impact of public dissemination of anti-corruption investigations for life satisfaction, as opposed to the impact of corruption itself. Moreover, we shed light on the importance of political trust as well as further individual-level variables in shaping the impact of observing anti-corruption events for life satisfaction.

This study also contributes to previous work on corruption as well as measures to fight it. There is an emerging literature on the effects of disclosing anti-corruption information on corruption and voting behaviour (Ferraz and Finan 2008; Cavalcanti et al. 2018; Avis et al. 2018; Colonnelli et al. 2022). Some studies have analysed the impact of the current anti-corruption campaign in China on related outcomes, such as corruption-related rent-seeking (Chen and Kung 2019), political support (Wang and Dickson 2022), civil servant recruitment (Jiang et al. 2022), firms innovation (Xu and Yano 2017), corporate philanthropy and efficiency (Hao et al. 2020) and safety compliance in coal industry (Xu et al. 2021). Others shed light on whether investigating high-ranking officials will reduce or, inversely, increase actual corruption from a purely theoretical angle (Che et al. 2019). Within this literature, we are the first to link this campaign to life satisfaction which is an important indicator of overall social progress and hence government performance (Stiglitz et al. 2009).

Overall, our results offer important new insights when it comes to understanding how the public interpret government action and more broadly the importance of political trust. We demonstrate how the high cost of signalling itself may not be enough to convince the public of 'good intentions', particularly when faced with low political trust. Ultimately, our findings also suggest that there may be a potential disincentive for governments to tackle certain problems if, by doing so, they make the problem more salient to citizens.

The remainder of this paper is organised as follows. In Section 1.2, we first introduce China's anti-corruption campaign in detail, then present signalling theory as a useful framework for understanding how the public disclosure of anti-corruption investigations could affect people's life satisfaction. Section 1.3 outlines our data sources. Section 1.4 describes the empirical strategy. In Section 1.5, we document our main findings. This is followed by Section 1.6 which presents various robustness checks and further analyses. Lastly, we conclude with a discussion of our main findings in Section 1.7.

1.2 Background and theoretical considerations

1.2.1 China's anti-corruption campaign

Corruption has become an issue of increasing concern for the general public in China (Pew Research Center 2016). President Xi Jinping has even warned that corruption is threatening the survival of the ruling Communist Party of China (CPC, Wong 2012). To help address public concern on this issue, an intensive anti-corruption campaign was initiated towards the end of 2012 and swept across the country over the following years (Wedeman 2017). A striking feature of this campaign is that a number of high-ranking officials have been investigated (see Appendices Table A1.1). There were also some prior investigations on senior officials, the scale however was much smaller.

The official ranks in Chinese government are as follows: national level, provincial-ministerial level, prefectural-bureau level, county-division level, town-section level, village-clerk level. The provincial government is hence the top level of local government in China. Despite the political centralization, officials in local governments particularly in the provincial government are endowed with considerable discretionary power in their jurisdiction (Li and Zhou 2005). The central government is in charge of provincial officials and they are directly managed by the Central Organization Department (COD), a core institution controlling the most important and powerful cadres in contemporary China (Lance 2015). Given their status, provincial officials can only be investigated by the Central Commission for Discipline Inspection (CCDI), the top agency responsible for anti-corruption in China.

The Chinese government has looked to heavily publicise its anti-corruption investigations. As an illustration, each single TH event is announced on the official

CCDI website. Following this, the news is actively publicised on all mainstream media platforms (e.g. *Sina Weibo*, the Chinese *Twitter*), using the folk-popular tag of 'Tiger-Hunting' to promote its dissemination (Sun et al. 2022). This is usually followed by a strong moral condemnation as well as *stories* describing the ousted official's road to corruption. As a result, these news items attract considerable public attention. For example, within one week the investigation of the party secretary of Hangzhou city (capital of Zhejiang province) in August 2021 attracted 700 million views on *Sina Weibo*.

Although there is heavy propaganda after the CCDI announcement of a TH event, the decision-making process underpinning these investigations is opaque. According to CCDI's interpretation of its rules for discipline inspection (Central Commission for Discipline Inspection 2020), any formal investigation should be based on a careful scrutiny of any evidence pertaining to violations of party discipline or law. This means that there is likely some 'pre-investigation' before each public disclosure which, in turn, limits the potential for the government to announce TH events in response to other provincial factors such as economic growth. It therefore seems unlikely that TH could be an effective tool for the government to arbitrarily distract the public from other social issues. In further support of this premise, we show in Section 1.6 that TH events are not correlated with a wide range of provincial level factors (e.g. provincial growth rates, actual corruption, number of civil servants etc.).

We focus on TH disclosures concerning provincial officials in local government and their impact on the life satisfaction of individuals living in the same province. This is, firstly, because Chinese people usually construct their social ties based on geographical and genealogical connections (Fei et al. 1992; Fisman et al. 2018), which means that people are likely to be far more concerned about TH in their own province than in other areas. Second, the considerable variation in the frequency of TH events across provinces (see Appendices Table A1.1) allows for meaningful regression analyses. One possible explanation for differences across provinces in the frequency of TH are different levels of actual corruption. As we document later, however, the frequency of TH events appears unrelated to actual corruption levels as well as numerous other provincial characteristics (see, again, Section 1.6).

We therefore assume that the distribution of TH among different provinces is as good as random. Three further reasons support this assumption. First, personal connections (e.g. shared hometown, college or former workplaces) with the top leaders in central government are believed to have an important influence on the career development of political elites in contemporary China (Shih et al. 2012; Jia et al. 2015). Such personal connections are in turn a crucial factor in the decision to investigate a potentially corrupt high-ranking official (Jiang et al. 2022). This means TH is more of a person-specific event than a regional event. Second, for the purpose of preventing regional political alliances and corruption, there is a convention of geographical rotation of high-ranking officials in contemporary China (Jiang and Mei 2020). This further reduces the connection between the frequency of TH events and certain provinces. Third, numerically, the numbers of TH events itself can hardly represent the levels of actual corruption across provinces. We can see from Appendices Table A1.1 that the frequency of TH events within the sample province varies considerably across years. One can never expect the level of actual corruption in any provinces to swing in such a dramatic manner (see also our additional analyses in Section 1.6.1).

1.2.2 Tiger-Hunting through the lens of signalling theory

We use signalling theory as a theoretical framework to rationalise the Chinese government's efforts to advertise anti-corruption successes and to predict the effect of disclosing these TH events on citizen well-being. In a seminal article, Spence (1973) postulated that high-quality prospective employees distinguish themselves from low-quality ones via the costly signal of higher education. The argument being that candidates are in effect seeking further education partly as a means to signal their 'quality' and reduce information asymmetries about their skills and motivation. Workers of lesser ability are unable or unwilling to mimic this 'signal' as it is harder for them to succeed in higher education.

Signalling theory has since been used to explain behaviours in a wide variety of situations, such as the public disclosure of charitable donations (Bracha and Vesterlund 2017), the use of diverse boards by firms to enhance reputation and status (Lamkin Broome and Krawiec 2008), bluffing in auctions (Horner and Sahuguet 2007) and sunk cost signalling where states look to signal their resolve to other states (Quek 2021). Each of these examples illustrates how one party undertakes costly efforts to signal a specific quality to another party. Given the information asymmetry between the government and the public (Lorentzen 2014; Wu et al. 2017), the active public disclosure of TH events can be seen as an effort by the ruling party to signal its commitment to reduce corruption

(Diallo 2021). To this end, it is seeking to distinguish itself from a 'low-quality government' that allows corruption to flourish with a view to retaining public support.

The signal meets the requirement of being costly for several reasons. First, policing corruption itself is costly (Lui 1986). Provincial officials in China possess considerable power and complex political connections and investigating them is often met with significant resistance (Xu 2016). In addition, the government goes to considerable effort in publicising these TH events and in developing supportive narratives (i.e. stories documenting their road to corruption).

While the signaller looks to 'signal' its good intentions, the receiver is free to interpret the signal in any fashion they so wish. In theoretical applications of signalling theory, the signal is usually seen as beneficial for not only the sender, but also the receiver. Going back to Spence's original example of higher education, the receiver (employer) of the signal benefits from being able to effectively discriminate between job candidates on the basis of ability. In keeping with this idea, the public could also benefit from the signal of TH disclosures by learning that corruption is being seriously tackled and should therefore become less prevalent. As a result of this 'countering effect', the well-being of citizens should improve.

In reality, however, the receiver may interpret the signal in a negative fashion, against the original intention of the signaller (Connelly et al. 2011). The lack of information about the actual level of corruption originates not only from uncertainty about the government's efforts to fight it, but more generally the true prevalence of corruption is hard to determine (e.g. Saiz and Simonsohn 2013). Due to this uncertainty, TH events could expose corruption citizens were unaware of, giving rise to a 'salience effect'. Perceiving corruption as even rifer than before, citizens may actually experience a reduction of their well-being in response to learning about another TH event. In support of this premise, a recent study suggests that anti-corruption campaigns in China actually increase the public's subjective perception of the level of corruption, on average (Wang and Dickson 2022).

A question to ask is therefore whether we can predict if the public disclosure of anti-corruption investigations will be seen in a positive or negative light at the individual level. A starting point in thinking about this issue is to consider how individuals will deal with the bad news and the good news (countering and salience) the TH signal conveys. We posit that people's trust in the government (political trust thereafter, Hetherington 1998), in connection with confirmation bias, will be key in predicting which effect dominates at the individual level.[5] A person who believes in the trustworthiness of the government will we suggest be more likely to neglect the bad news the signal carries, and focus on the good news. In other words, they will be more likely to see the TH signal as evidence of good intentions. This means that, even if the salience effect is strong for the average person, the higher the level of political trust the more likely it is that a person will benefit from observing TH disclosures in the media as to them efforts to tackle corruption will be in keeping with their existing positive perception of the government.

However, as political trust lowers, we argue it becomes more likely that the salience effect dominates, resulting in diminished life satisfaction. Someone who has no trust in the government whatsoever may, in fact, hesitate to consider any government action as good news at all. This is aided by the lack of information relating to the decision-making process underpinning TH events which may raise suspicions about the true purpose of an investigation. Simply put, TH might be seen as an attempt to get rid of a provincial official who, for whatever reason, has become a nuisance for the central government. In the process, the lack of political trust eliminates the countering effect as the person does not believe tackling corruption to be at the heart of the government's motive.

1.3 Data

Our individual-level data come from the China Family Panel Studies (CFPS, Institute of Social Science Survey, Peking University, 2015). This longitudinal nationally representative survey is conducted every two years beginning in 2010 with the last wave released in 2018. The CFPS samples over 32,000 adults (age ≥ 16 in 2010) across 25 provinces (listed in the Appendices Table A1.1).[6] These provinces cover 95% of the Chinese population. We match the individual-level data with provincial characteristics and TH events occurring between two survey waves based on the interview month as well as the province in which each respondent lives. As explained in greater detail below, we therefore use CFPS waves from 2012-2018 only. We exclude respondents who migrated across provinces while participating in the CFPS (about 1% of the full sample). This is because we cannot calculate the frequency of TH events for these respondents

[5] Confirmation bias describes the tendency of people to often seek out and interpret news in a way that nurtures their existing attitudes and beliefs (Lord et al. 1979; Rabin and Schrag 1999; Wilson 2014).

[6] People from the five relatively large provinces of Henan, Gansu, Guangdong, Shanghai and Liaoning are oversampled in the CFPS. It is therefore reassuring to note that we would obtain the same results based on a sample excluding these five provinces (see Appendices Table A1.10).

as we do not know exactly when they moved in or out of a province (i.e., in which province they lived when a TH event happened). Our final sample consists of a balanced panel of 41,724 observations, after observations with missing values on individual characteristics used in the empirical analyses were excluded. [7]

We measure life satisfaction based on a survey item in the CFPS that requires respondents to answer the following question on a 5-point scale: '*Are you satisfied with your life*'. A score of 1 corresponds to the lowest level of satisfaction, 5 the highest. The measure of political trust is based on a survey item where respondents are presented with an 11-point scale and asked to answer the following question '*How much do you trust the cadres (officials in local county-level government)*', with 0 indicating least trust and 10 highest trust. While the item refers to the 'local county-level' we assume that it is inextricably linked with the trust in the central government because all levels of government are controlled at the central level (see Section 1.2). This measure has been widely used before to capture political trust (e.g. Cai et al. 2020).

Further individual-level information that are used as control variables include age (squared), education (six levels from being illiterate to having a university degree), residence (urban or rural area), political status (member of the CPC) and family per capita income. *Hukou* status denotes if a person possesses a permanent urban residency permit and hence has access to wide-ranging public services and social security, such as in the areas of education, healthcare and pensions. Note that simply working and living in an urban area does not mean a person has urban *Hukou* status (Deng and Schöb 2022).

Our starting point in capturing TH events is a list of all investigated provincial officials in local governments since December 2012 obtained from *China Economic Net* (Economic Daily 2020). This source records the detailed position of the investigated official and the official disclosure date of the investigation. While the large-scale investigations of senior officials started at the end of 2012, there were already a few investigations of provincial officials in previous years (see Appendices Table A1.1). These were also widely publicised by state media. We were able to obtain information pertaining to these anti-corruption investigations from the CCDI annual work report

[7] We also conduct a sensitivity check where we replicate all our main results using an unbalanced panel. These results (see Appendices Table A1.3) are quantitatively indistinguishable from our preferred setting.

(Central Commission for Discipline Inspection 2011, 2012, 2013).[8] We then simply assign each individual in the CFPS the number of TH events that have happened in their province between the dates of the previous survey interview and the current interview (roughly, two years).[9] As there are no pre-2010 TH data at our disposal, the first CFPS wave is not part of the study.

Our analyses also include various socio-economic indicators at the provincial level. These data are obtained from the China Statistical Yearbook (National Bureau of Statistics, 2011-2018) and the Procuratorial Yearbook of China (Supreme People's Procuratorate, 2011-2018) and due to spatial identifiers we can match these data sources with the CFPS.[10] The provincial indicators are designed to capture differences in the levels of economic and social development across provinces, such as per capita GDP and the GDP growth rate. We also include a measure of the actual level of corruption, albeit we concede that corruption is challenging to measure precisely, as it is typically concealed (e.g. Olken 2009). Following a common measure of corruption in studies on China (e.g. Huang et al. 2017), we use the annual number of filed corruption investigation cases at all government levels within a province (from the Procuratorial Yearbook of China) per 10,000 people as a proxy for actual corruption. Note that, unlike TH events, these data are not actively publicised by the government and hence less known to the public. To take the local government scale into account, we include government fiscal size, which is measured by the ratio of government expenditure and GDP of each province, and the ratio of civil servants, which is measured by the number of civil servants per 100 people.

For an overview of summary statistics, we refer the reader to Appendices Table A1.2. The average frequency of TH events a person experiences between two waves of the CFPS is 1.2. In general, the frequency of TH events during the sampling period varies markedly, which is advantageous for estimation purposes. The average life satisfaction score is 3.7. The mean value of political trust is 5.1.

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[8] We conduct a robustness check by excluding the data that is manually collected based on CCDI work reports, i.e., focusing on the sample period after 2012. The results are quantitatively the same to our main setting (see Appendices Table A1.4).

[9] For TH events in 2010 and 2011 we matched this with life satisfaction data recorded in 2012 and so on.

[10] Statistical yearbook data concern the respective previous year, so our provincial characteristics cover 2010 to 2017. In some years yearbooks lack data from certain provinces. We supplement those data by manually collecting information from the People's Procuratorate's annual work report in provincial governments.

1.4 Empirical strategy

We employ an individual-fixed effects model to evaluate the effect of TH disclosures on individual life satisfaction. Our baseline empirical specification is as follows:

$$LS_{ijt} = \alpha + \beta \cdot QTH_{ijt} + \varphi \cdot PT_{ijt} + \boldsymbol{Pro}_{jt} \cdot \boldsymbol{\delta} + \boldsymbol{Ind}_{ijt} \cdot \boldsymbol{\lambda} + \phi \cdot LoI_{ijt} + \theta_k + \eta_i + \varepsilon_{ijt} \quad (1.1)$$

LS_{ijt} represents life satisfaction of individual i in province j and year t. η_i is the individual-fixed effect. As migrants between provinces are excluded, the individual-fixed effects also absorb stable provincial characteristics. QTH_{ijt} is the key independent variable denoting the frequency of TH disclosures that individual *i* in province *j* has experienced between the previous and the current wave of the CFPS, i.e., from year *t-2* to the current interview year *t*. As an illustration, assume individual *i* in CFPS was interviewed in July 2012 and December 2014 respectively, then $QTH_{ij,2014}$ equals to the total number of investigated provincial officials between July 2012 (not including) and December 2014 (including) in province j.[11] In Section 1.5, we also test the sensitivity of our results to alternative means of measuring TH events. Note that, throughout, we cluster standard errors at the province level.[12]

As the individual-fixed effect captures the influence of all stable characteristics of a person, our identification of the effect of TH disclosures on life satisfaction rests on the assumption that these disclosures are exogenous to other changes in the lives of the respondents between survey interviews. The fact that corruption investigations are initiated by a central government agency makes it clear that changes in the lives of individual respondents do not trigger TH events. Given that senior officials' personal political ties play a role in the decision to investigate an individual for corruption and also that they are rotated across provinces, changes in provincial characteristics are unlikely to trigger TH events. This is supported by our finding that the actual level of corruption as well as numerous provincial characteristics are unrelated to the frequency of TH events at the provincial level (see Section 1.6.1). The same applies to recent trends in the level of corruption and socio-economic indicators.

[11] If a provincial official was investigated at the end of December 2014 while individual *i* was interviewed at the beginning of the month, they actually were not 'treated' by this TH event. We conduct a sensitivity check by measuring TH events until the last month before the interview month (November 2014 in the above example). The estimated effect of TH is very similar to that of our main baseline estimates presented later in Table 2 when we exclude these observations. See details in Appendices Table A1.5.

[12] This might make our estimation overly conservative in terms of statistical inference, particularly given that the number of provinces is relatively small compared to the scale of our total observations (Abadie et al. 2022).

Furthermore, respondents (even in the same province) in the CFPS were interviewed in different months of the respective survey year and the interview month varies within individuals across the different waves of the survey.[13] Even observations from the same province and survey year therefore experience different frequencies of TH disclosures. This increases the randomness of the frequency of TH events that each respondent experienced during the CFPS intervals.

To further alleviate concerns about the exogeneity of TH events, vector $\boldsymbol{Pro}_{jt}$ covers time-variant provincial characteristics. This includes the contextual (actual) level of corruption, the government fiscal size, the ratio of civil servants, the GDP growth rate and per-capita GDP. Similarly to our approach in calculating QTH, we use the two-year average value between each survey wave of the CFPS for all provincial variables. Considering the decision to investigate a public official for corruption could happen significantly before the public disclosure, we also use provincial controls lagged for additional years and confirm that the results are not quantitatively different.

PT_{ijt} represents the political trust of individual *i* in province *j* and year *t*. $\boldsymbol{Ind}_{ijt}$ is a vector of time-variant control variables at the individual level. This includes age squared (divided by 100)[14], education level, residence (urban or rural area), *Hukou* (household registration), CPC member, family per capita income (in logarithm).

To capture time trends, we add the time-fixed effects θ_k, a series of binary variables indicating the specific CFPS interview month while also capturing year effects (e.g. October 2014 and October 2016 are represented by different variables). The length of the interval (in months) between two waves of CFPS at the individual level is also controlled for (LoI_{ijt}). The reason is that longer intervals will result in larger *QTH*, which could create an endogeneity issue if the length of the interval was also correlated with changes in life satisfaction. Note, however, that our findings do not depend on controlling for LoI_{ijt}.

To aid the presentation of our results, we treat life satisfaction as a cardinal variable running an individual-fixed effects OLS model. We also conduct a robustness check employing an individual-fixed effects ordered logit model (Baetschmann et al. 2020). The results are in keeping with our baseline estimates. In addition, a random effects

[13] A fraction (4.7%) of the full sample were interviewed between January and June of the second year of each wave CFPS, e.g. some observations of CFPS 2012 were interviewed in March 2013. We exclude this part of the sample because our sample is a balanced panel neatly spanned across 2012, 2014, 2016 and 2018, and all provincial controls are matched on the first year of each wave of the CFPS. The results would not differ quantitatively if the excluded observations were kept in the sample.

[14] We do not add age itself to our model, as its effect is completely absorbed by the time variables.

model that considers both within-person and between-person variation as well as a hierarchical linear modelling approach are employed as further robustness checks. The estimates of *QTH* in all these additional modelling approaches are similar to our baseline estimates presented in the following section. Results from these other modelling approaches are presented in Appendices Table A1.6.

1.5. Empirical results

1.5.1 The average effect of TH disclosures on life satisfaction

We first look at the direct relationship between TH disclosures and individual life satisfaction before considering the moderating role of political trust. The results are presented in Table 1.1. In column 1, a parsimonious model is estimated controlling for individual and time-fixed effects only. We can see here that *QTH* attracts a negative yet insignificant coefficient ($\beta = -0.008$, $p = 0.286$). As we are interested in the signalling effect of information of anti-corruption investigations, we add our detailed set of provincial and individual controls in Specification 2, the *QTH* estimate gains precision and becomes statistically significant, albeit at the 10% level only ($\beta = -0.011$, $p = 0.084$). Overall, this provides some weak evidence to suggest that TH disclosures are associated with a net cost when it comes to individual life satisfaction.

Next, we examine how sensitive our results are to alternative ways of measuring TH disclosures. Specifically, we measure TH disclosures in the following two different ways: (1) per capita number of TH disclosures (per 10 million people) and (2) per civil servant number of TH disclosures (per 0.1 million civil servants). These measures take account of the fact that the same number of TH events may affect people differently depending on the total population in each province or the total number of government officials in each province. The results are presented in columns 3 and 4 of Table 1.1, respectively. Both these alternative measures of TH yield statistically significant coefficients ($p<0.05$). In sum, whether we use the raw aggregate numbers pertaining to frequency of TH events across provinces or per capita measures, we obtain the same qualitative findings.

Table 1.1 Baseline results

	Dependent variable: Life satisfaction				
VARIABLES	(1)	(2)	(3)	(4)	(5)
QTH	-0.008	-0.011*			-0.012*
	(0.008)	(0.006)			(0.006)
Per capita QTH			-0.064**		
			(0.028)		
Per civil servant QTH				-0.073**	
				(0.035)	
Political trust					0.037***
					(0.002)
Provincial controls					
Contextual corruption		-0.520**	-0.539**	-0.541**	-0.494**
		(0.208)	(0.201)	(0.202)	(0.203)
Log per capita GDP		-0.033	-0.007	-0.020	-0.064
		(0.200)	(0.188)	(0.187)	(0.191)
GDP growth rate		0.002	-0.000	0.001	0.002
		(0.007)	(0.007)	(0.007)	(0.008)
Government fiscal size		-0.727	-0.552	-0.636	-0.710
		(0.635)	(0.618)	(0.622)	(0.680)
Ratio of public servants		0.248	0.317	0.264	0.303
		(0.285)	(0.278)	(0.284)	(0.288)
Individual controls					
Age squared/100		0.036***	0.036***	0.036***	0.032**
		(0.012)	(0.012)	(0.012)	(0.011)
Residence		0.021	0.023	0.023	0.025
		(0.027)	(0.028)	(0.028)	(0.028)
Hukou (rural=1)		-0.102**	-0.103**	-0.103**	-0.106**
		(0.047)	(0.047)	(0.047)	(0.046)
Log per capital family income		0.010	0.010	0.010	0.010
		(0.007)	(0.007)	(0.007)	(0.006)
CPC member		0.054	0.054	0.054	0.044
		(0.050)	(0.050)	(0.050)	(0.048)
CFPS Interval		0.002	0.002	0.002	0.002
		(0.004)	(0.004)	(0.004)	(0.004)
Education level		Yes	Yes	Yes	Yes
Month effect	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Observations	41,724	41,724	41,724	41,724	41,724
Number of persons	10,431	10,431	10,431	10,431	10,431
Within R-squared	0.122	0.124	0.124	0.124	0.131

Note. Robust standard errors clustered at province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The measurements of all provincial control variables and individual control variables are displayed in Table A1.2, except for family per capita income and per capita GDP, which have been log-linearised.

For ease of presentation, we focus the following discussion on effect sizes based on the results presented in column 2 relating to the estimated impact of TH events as opposed to the per capita measures presented in column 3 and 4. Numerically, our estimates in column 2 suggest that investigating one additional provincial official would reduce local residents' life satisfaction by an average of 0.011 points on a five-point scale. To help contextualise this overall estimated effect size, we compare it with the estimated impact of a rural *Hukou* status on individual life satisfaction. The *Hukou* system is a discriminating institution designed to control population movements in contemporary China. It deprives the rural population as well as rural-to-urban migrants of access to education, healthcare services, retirement benefits as well as other public services enjoyed by the original urban population (Afridi et al. 2015). Rural hukou has been shown to have a significant and substantive negative estimated impact on individual life satisfaction (e.g. see Tani 2016). This can also be observed in our own results where having a rural *Hukou* status attracts a significantly negative coefficient ($\beta = -0.102$, $p = 0.040$). Our estimates suggest that the disutility suffered from observing the investigation of one additional provincial official is equivalent to 11% of the individual well-being cost of having a rural *Hukou* status.

Our measure of contextual corruption is also negatively correlated with individual life satisfaction. The effect size is considerable, a reduction of one standard deviation in the contextual corruption level is equivalent to 60% of the impact of having a rural *Hukou* status. This is in keeping with much of the existing research based on Chinese data and beyond (see Section 1.1).

Given our later analysis of the moderating role of political trust it is worth highlighting the direct relationship between this variable and individual life satisfaction. We consider it in Specification 5. Its coefficient is positive and statistically significant ($\beta = 0.037$, $p = 0.000$). The estimated effect size also appears substantial. A one-point reduction in political trust is equivalent to 36% of the individual well-being cost of having a rural *Hukou* status.

1.5.2 The moderating role of political trust

Next, we examine the moderating role of political trust. Here we simply add an interaction term ($QTH \times PT$) to our baseline model. As illustrated in column 1 of Table 1.2, this interaction term attracts a statistically significant coefficient ($\beta = 0.006$, $p = 0.000$). This indicates that the signalling effect of TH disclosures on life satisfaction depends on the individual's level of political trust. Numerically, people with the lowest level of political trust are predicted to score 0.043 points lower on the life satisfaction scale as they witness an additional TH disclosure. This would be equivalent to 43 % of the estimated impact of a rural *Hukou* status. By contrast, those with the strongest political trust enjoy an estimated increase of 0.021 in life satisfaction (20% of the rural *Hukou* impact). Overall, our estimates suggest that for those with a self-reported political trust score lower than 7 on the 11-point scale (73% of the population, see Appendices Figure A1.1), a TH event is estimated to diminish their life satisfaction.

One concern when it comes to identifying the moderating role of political trust is that people's political trust itself might be influenced by the disclosure of TH. We think this should not be a serious issue in our case. For one, political trust itself is controlled for in our model, which means that it is unlikely that this finding is driven by a mediating effect of political trust (e.g. TH disclosures affects political trust and in turn political trust affects life satisfaction). Second, given that the marginal impact of TH disclosures turns from negative to positive as political trust increases, it is hard to imagine that any changes in political trust due to TH disclosures (if they truly exist) can drive such a substantive moderating effect.

To further alleviate such concerns, we directly regress political trust on TH disclosures (*QTH)* to check whether there is a direct link between these two variables. The results in columns 2 and 3 of Table 1.2 show that there is no significant correlation between TH disclosures and political trust. Additionally, we also use one-period lagged political trust as the moderator (*QTH* × *lagged political trust*). In addition to lagged political trust, here we also control for current political trust to rule out any realistic possibility of a mediating effect of political trust. The results in column 4 are similar to that in column 1, which again suggests that our finding is not driven by changes in political trust over time in response to TH.

One could also argue that the moderating effect could be driven by other individual characteristics correlated with political trust. Although it is hard to ultimately rule out this possibility, we provide evidence to suggest that this should not be a serious issue in our analysis. Specifically, we interact *QTH* with all other individual control variables in our model as well as with gender, age, residence and education level. None of these interaction terms attracts a statistically significant coefficient. On the other hand, we can see in column 5 of Table 1.2 that the coefficient of the interaction term of *QTH* and political trust still closely resembles that of column 1 and is statistically significant. This would suggest that the moderating effect of political trust is very unlikely to be driven by other individual characteristics.

Table 1.2 The moderating role of political trust

VARIABLES	(1) Life satisfaction	(2) Political trust	(3) Political trust	(4) Life satisfaction	(5) Life satisfaction
QTH	-0.043***	0.010	0.011	-0.042***	-0.099
	(0.010)	(0.023)	(0.024)	(0.013)	(0.064)
QTH × political trust	0.006***				0.006***
	(0.001)				(0.001)
QTH × lagged political trust				0.005**	
				(0.002)	
Political trust	0.029***			0.035***	0.030***
	(0.003)			(0.003)	(0.003)
Lagged political trust				-0.011**	
				(0.004)	
QTH × Individual characteristics					Yes
Provincial controls	Yes		Yes	Yes	Yes
Individual controls	Yes		Yes	Yes	Yes
Month effect	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Observations	41,724	41,724	41,724	31,293	41,724
Number of persons	10,431	10,431	10,431	10,431	10,431
Within R-squared	0.132	0.004	0.006	0.074	0.133

Note. Robust standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Provincial control variables and individual control variables are the same as in Table 1.1 and are defined according to Table A1.2. Family per capita income and per capita GDP have been log-linearised.

1.5.3 Heterogeneity analysis

Having documented the population level impact of TH disclosures, and also the moderating role of political trust, we now examine descriptively whether the estimated well-being effect of TH disclosures varies across different socio-demographic subgroups. To this end, we separately interact *QTH* with *female*, *age*, *rural Hukou*, *family per capita income (FPCI)* and *CPC member*, respectively. The results are presented in Table 1.3.

We first look at whether gender plays a role in predicting the degree to which people are affected by TH events in their province. The results in column 1 show that there is no significant difference between males and females when it comes to the impact of TH disclosures for life satisfaction. In column 2, we can see that the interaction of *QTH* with *age* produces a positive and statistically significant coefficient ($\beta = 0.001$, $p = 0.000$). This indicates that comparatively older individuals are more likely to view TH disclosures as a positive signal. A possible explanation is that older people in contemporary China have long been exposed to government propaganda and hence trust the government more. It might also be that older people are relatively more reliant for news on state-controlled television, which conveys TH news unambiguously as a story of government success, unlike social media which allows for a greater plurality of opinions, even in China. Note, however, that these explanations as well as all following suggestions for reasons for subgroup differences are speculative and derived ex post. We note that the age-*QTH* interaction effect is no longer statistically significant in a model including additional interactions of *QTH* with political trust and other variables (results of column 4 in Table 1.2, not presented in detail). This means that while it is descriptively interesting to note that the impact of TH on life satisfaction varies across age groups, age itself is unlikely the root cause of this result.

Next, we examine whether the well-being effect of TH disclosures diverges between people with different types of *Hukou* status. The results in column 3 show that the interaction effect of TH disclosures and a rural *Hukou* status is positive and statistically significant ($\beta = 0.020$, $p = 0.006$). This suggests that people with a rural *Hukou* status experience a smaller estimated reduction in their life satisfaction when exposed to TH disclosures in their province. Similar to the elderly, most rural *Hukou* holders are also more reliant on state-controlled television than the urban population as they live in rural areas. Furthermore, they are usually in an unfavourable socio-economic position and hence more likely to have suffered the consequences of corruption which is why they might welcome anti-corruption investigations particularly. As with age, the interaction effect of rural *Hukou* and *QTH* does not survive controlling for other interaction effects of *QTH* (other individual characteristics, political trust) which means that the *Hukou* status itself is unlikely to be the reason why rural *Hukou* holders respond differently to observing TH events.

The interaction term of *family per capita income (FPCI)* and *QTH* attracts a negative coefficient suggesting that poorer individuals suffer to a lesser extent from TH disclosures. Similar to the finding in relation to *Hukou* status, disadvantaged groups may be more likely to see the anti-corruption campaign as a positive signal. However, this is not a statistically significant difference ($\beta = -0.005$, $p = 0.115$).

Last, we check whether the well-being effect of TH disclosures is different for CPC members. The motivation here is that CPC members are the main target of anti-corruption policies in China (Li et al. 2007) and so they might fear anti-corruption actions. However, in column 5, we can see that the coefficient of the interaction term is statistically insignificant.

Table 1.3 Heterogeneity analysis

VARIABLES	(1) A= Female	(2) A= Age	(3) A= rural *Hukou*	(4) A= FPCI	(5) A= CPC member
QTH	-0.016**	-0.082***	-0.027***	0.034	-0.009
	(0.006)	(0.022)	(0.009)	(0.028)	(0.006)
QTH × A	0.008	0.001**	0.020***	-0.005	-0.026
	(0.007)	(0.000)	(0.007)	(0.003)	(0.016)
Provincial controls	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes
Month effect	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Observations	41,724	41,724	41,724	41,724	41,724
Number of persons	10,431	10,431	10,431	10,431	10,431
Within R-squared	0.124	0.124	0.124	0.124	0.124

Note. Robust standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Provincial control variables and individual control variables are the same as in Table 1.1 and are defined according to Table A1.2. Age squared is not controlled for in column 2. Family per capita income and per capita GDP have been log-linearised.

1.6 Robustness checks and further analyses

In this section, we discuss the results from a series of additional analyses designed to test the robustness of our main findings from our baseline analysis (column 2 in Table 1.1). We also repeat all the additional analyses described below when it comes to our examination of the moderating effect of political trust (see column 1 of Table 1.2). For parsimony, these results in relation to trust are not discussed below but are presented in Appendices Table A1.9.

1.6.1 Do province characteristics predict Tiger-Hunting?

Notwithstanding the many reasons for the arbitrariness (and hence randomness) of TH across provinces described above (Section 1.1, 1.2 and 1.4), as well as our detailed set of control variables, there might still be a concern that an unobserved confounder predicts the provincial frequency of TH and life satisfaction. We follow a common approach in demonstrating exogeneity by showing that the frequency of TH is unrelated to a series of important provincial level variables (e.g. Colonnelli and Prem 2022). We do so by regressing the provincial TH (number of TH events in each province) which we label as *PQTH*[15] on a number of provincial characteristics and their recent change.

Beyond the provincial controls included in our baseline estimation of life satisfaction above, we add the recent yearly change of these variables as well as levels

[15] Note that this is somewhat different from our key variable *QTH*, which differs even across individuals from the same province interviewed in the same year dependent on the date of the previous and the current CFPS interview. *PQTH* is the exact frequency of TH in each province in a certain year.

and changes of total population (log-linearised), average years of schooling, economic openness (measured by the logarithm of the total volume of imports and exports), money supply (measured by logarithm of loans balance of financial institutions) and the quality of the institutional environment (measured by marketisation index). All data come from the China Statistical Yearbook except for the marketisation index, which is obtained from Wang et al. (2018). The descriptive statistics of these variables are presented in Appendices Table A1.7. All explanatory variables (EV) are lagged by one year considering TH could happen in the early months of a year and the investigating process also takes some time before it is publicly disclosed.

Table 1.4 Provincial characteristics and TH events

	Dependent variable: provincial frequency of TH								
VARIABLES	(1) FE	(2) FE	(3) Tobit	(4) FE	(5) FE	(6) Tobit	(7) FE	(8) FE	(9) Tobit
Contextual corruption level									
L1	0.222	-0.056	-0.271				0.755	0.981	-0.297
	(0.970)	(1.187)	(1.311)				(1.097)	(1.337)	(1.403)
L2				1.033	1.157	0.497			
				(0.958)	(1.142)	(1.322)			
Trend (L2 – L1)							-1.668	-2.144	-3.636
							(1.237)	(1.415)	(3.552)
Ratio of civil servants									
L1		1.609	-0.282					0.559	-0.277
		(1.530)	(0.688)					(1.595)	(0.720)
L2					0.247	-0.525			
					(0.966)	(0.691)			
Trend (L2 – L1)								0.372	6.056
								(1.782)	(4.597)
Government fiscal size									
L1		-3.767	0.060					-3.936	2.749
		(6.839)	(5.006)					(6.417)	(5.447)
L2					-1.496	2.279			
					(2.847)	(5.006)			
Trend (L2 – L1)								-2.216	-13.910
								(6.763)	(12.491)
Log per capita GDP									
L1		0.297	0.761					1.754	1.527
		(1.202)	(1.508)					(2.021)	(1.581)
L2					2.358	1.359			
					(1.696)	(1.501)			
Trend (L2 – L1)								-3.092	-6.717
								(2.472)	(5.101)
Log population									
L1		2.177	0.707					2.471	1.129
		(2.994)	(0.911)					(2.887)	(0.997)
L2					3.607	1.073			
					(2.677)	(0.896)			
Trend (L2 – L1)								-8.959	-17.018
								(6.033)	(16.647)
Log money supply									
L1		-0.250	-0.189					-0.537	-0.583
		(0.697)	(0.969)					(0.901)	(1.012)
L2					-0.505	-0.064			
					(0.975)	(0.907)			
Trend (L2 – L1)								0.063	1.251
								(1.094)	(2.984)
Marketization index									
L1		-0.124	-0.165					-0.067	-0.121
		(0.144)	(0.249)					(0.178)	(0.283)
L2					-0.052	-0.032			
					(0.154)	(0.244)			
Trend (L2 – L1)								-0.025	-0.222
								(0.200)	(0.514)
Log trade volume									
L1		-0.100	-0.038					-0.240	-0.011
		(0.339)	(0.373)					(0.389)	(0.395)
L2					-0.372	-0.324			
					(0.302)	(0.358)			
Trend (L2 – L1)								0.853**	2.270**
								(0.354)	(0.995)
Average schooling years									
L1		0.017	0.095					0.048	0.167
		(0.065)	(0.111)					(0.080)	(0.144)
L2					0.011	0.060			
					(0.062)	(0.116)			
Trend (L2 – L1)								-0.015	-0.064
								(0.062)	(0.122)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	225	225	225	225	225	225	225	225	225
Number of provinces	25	25	25	25	25	25	25	25	25
R-squared	0.181	0.195	-	0.183	0.203	-	0.185	0.219	-
log likelihood	-	-	-25	-	-	-224	-	-	-220

Note. Robust standard errors in parentheses in column 1, 2, 4, 5, 7, 8, standard errors in column 3, 6, 9. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In column 1 to 3, all provincial explanatory variables lagged for one-year, and they are lagged for two-years in columns 4 to 6. In column 7 to 9, we include the one-year lagged explanatory variables and the changes in these variables (the two-years lagged value minus the one-year lagged value).

The results are presented in Table 1.4. In column 1, we regress provincial TH (*PQTH*) on our contextual corruption variable. There is no significant relationship between the two variables which suggests that anti-corruption investigations of high-ranking provincial officials are largely independent of the actual corruption level in a certain province. We include all other provincial factors in column 2. None of these variables are significantly related to *PQTH* which again suggests that TH is an exogenous event.

Considering *PQTH* is truncated at 0, we also use a Tobit model for our regression analysis. The results are presented in column 3. Again, we observe no significant relationship between the provincial variables and PQTH. Additionally, we further lag all explanatory variables for two years, as decisions about whether to investigate provincial officials may have begun significantly before they were disclosed to the public. We still do not see any significant correlations between these macro factors and *PQTH* (see columns 4-6). Finally, we add the yearly changes in these variables to the model, and this (see columns 7-9) again does not reveal any clear explanations for the frequency of TH in a province. Only the recent trend in the trade volume attracts a statistically significant coefficient, which is however positive and thus hard to reconcile with TH as a punishment. After having tested 18 variables in the model, it might simply be an occurrence of type 1 error. In line with this conjecture, if we slightly adjust the model to trends over a two-year period ('L3-L1'), none of the changes including in trade volume is significantly associated with the number of provincial TH events.

To sum up, we find that TH is unrelated to a variety of important provincial variables. Coupled with the randomness associated with the CFPS interview timing, this supports the assumption of TH events being exogenous to provincial characteristics and their trends over time.

1.6.2 Importance of province

We now examine whether the fact that the political status of provinces varies affects our estimates. The political status of a province is reflected in the political ranking of the top leader of the provincial government. Among the 25 provinces covered in this study, Beijing, Shanghai, Guangdong, Tianjin and Chongqing possess the highest political status, with the top leader (party secretary) of these five provinces being a member of the politburo, the most powerful body in the Chinese government. Although such differences in political status do not necessarily correlate with the frequency of TH events, investigating senior officials in these provinces could serve as a stronger signal and attract more media attention such that the marginal effect of the TH signal on life satisfaction is relatively strong. In order to check if our results relating to the impact of TH for life satisfaction are driven by these areas, we dropped observations from these five provinces. The results are presented in column 2 in Appendices Table A1.8. The corresponding estimates are statistically indistinguishable from our baseline results (full sample) in column 1. This suggests that our findings are not driven by high-political status regions.

1.6.3 Neighbouring Tiger-Hunting

Our baseline specification is designed to capture the effect of TH disclosures on residents in the corresponding provinces. However, as a result of geographical proximity, one might expect that TH in neighbouring provinces may also impact life satisfaction given that these events are publicised nationally. Such spill-over effects should not bias our estimates of the impact of provincial disclosures given the randomness of TH events across provinces as demonstrated earlier. Nevertheless, if TH events in neighbouring provinces also matter, then the frequency of TH exposures and consequential changes in well-being are not limited to TH events occurring in the same province. This would also support the explanation of a signalling mechanism, as TH events in other provinces are less likely to influence people's life satisfaction through their real-life experience than TH events in their own province. To examine this possibility, we manually calculated the frequency of TH disclosures in neighbouring provinces[16] and estimated its relationship with life satisfaction. The results are presented in column 3 in Appendices Table A1.8. Here we can see that TH disclosures in neighbouring provinces also appear to influence life satisfaction, albeit to a lesser extent than disclosures in one's own provinces.

1.6.4 Does Tiger-Hunting affect well-being over the long term?

In our baseline analysis, we looked at the direct relationship between TH disclosures and individual life satisfaction. One may also be interested in whether the estimated well-being impacts associated with TH events continue to impact individuals over the longer term. Given our short panel of four waves, we are unable to analyse the effect of recent TH events over a long-time horizon. But we can provide some suggestive evidence by regressing individual life satisfaction to the one-period lagged *QTH*, i.e.,

[16] We define two provinces as neighbouring provinces once they are directly bordered.

we can capture the effect of TH disclosures that happened roughly 2 to 4 years ago. The corresponding results are presented in Appendices Table A1.8 (column 4). Lagged *QTH* attracts a statistically insignificant coefficient and one that is close to zero in absolute terms. This suggests that what principally matters for people's life satisfaction are current or recent TH disclosures.

1.6.5 Is there a deterring effect?

Up to now, we have focused on the 'signal' effect when it comes to the relationship between TH disclosures and life satisfaction. Another possible channel of influence is through an actual deterrence of corruption. If the public disclosure of TH events in provinces led to a reduction in actual corruption in those provinces, then this could benefit the life satisfaction of the local population. To examine this idea, we look at the direct correlation between TH and our measure of contextual corruption at the provincial level. The results are presented in Appendices Table A1.11 and show that there is no significant relationship between *PQTH* and contemporaneous or future (up to 4-years later) contextual corruption. This provides some initial evidence to suggest that TH disclosures are not leading to a change in actual corruption.

Next, we relate TH to data on actual individual experience of corruption. We do this by taking advantage of two questions in the CFPS (available for 2012, 2014 and 2016): Respondents are asked (1) Whether they experienced unreasonable delay or stalling at a government agency in the past year; (2) Whether they experienced unreasonable charges paid to a government agency in the past year. These two experiences are coded as *Stalling* and *Charge*, respectively, with '1' indicating having had a corruption experience and '0' otherwise.

The descriptive statistics of these variables are presented in Appendices Table A1.7. We employ a probit model to evaluate the relationship between TH disclosures and the probability of an individual experiencing *Stalling* or *Charge,* respectively. The results are presented in Appendices Table A1.12. The effect of *QTH* is insignificant which again supports the premise that variation across provinces in the public disclosure of these anti-corruption investigations is not having a significant impact on the actual prevalence of corruption. Importantly, this shows again that TH disclosures impact the life satisfaction of citizens through signalling effects, rather than any actual change of their corruption experience and/or living conditions.

1.6.6 Does the extent of exposure to TH matter?

If as we speculate TH disclosures negatively impacts life satisfaction through 'signalling' effects, one would expect that those most exposed to the signal should witness larger losses in life satisfaction. The CFPS offers us a unique opportunity to test this proposition as it records both whether individuals have access to the internet as well as the importance they place on it as a source of information.[17] Considering that the propaganda of TH is heavily conducted over the internet (see Section 1.2), we can assume that those who rely more on the internet for information will be exposed to TH disclosures to a larger extent. We then interact both measures of internet usage with *QTH* in our model respectively to capture the extent to which individuals are exposed to TH disclosures. The results are presented in Appendices Table A1.13. Both interaction terms attract a significant and negative coefficient, suggesting that individuals indeed experience a larger decrease in life satisfaction when exposed more heavily to TH disclosures.

1.7 Conclusion

We examined whether the public disclosure of anti-corruption investigations in China affects individuals' self-reported life satisfaction. We examined this through the prism of signalling theory, wherein the Chinese government is looking to signal 'good intentions' through the public disclosure and dissemination of anti-corruption investigations.

Overall, our results demonstrate that the public disclosure of anti-corruption investigations modestly reduces population-level life satisfaction. We show that the estimated impact of public disclosures depends, however, to a large degree on a person's political trust. For those with low political trust, the disclosure of anti-corruption investigations is associated with declining satisfaction, but for those with high political trust, it appears well-being enhancing. Our proposed explanation is that anti-corruption disclosures could produce both a countering and a salience effect when it comes to an individual's perception of the actual corruption level. In keeping with confirmation bias, we suggest that whether people will focus on the 'good' or 'bad' aspects of the signal

[17] Between 2014 and 2018, CFPS includes two questions about internet usage. Respondents are asked to answer '*yes*' or '*no*' to the question, '*Do you use the internet (through computer or mobile terminals)?'*, which we convert in our binary indicator 'internet usage'. They also answer on a five-point scale, *'How important is the Internet as an information channel to you?'*. Their answers serve us as indicator of 'internet importance'.

will depend on their existing perception of the trustworthiness of the government. The lower the level of political trust the more strongly the government's true intentions may be questioned which ultimately could eliminate the countering effect.

One implication of these findings is that when signalling good intentions, such as disclosing policy actions aimed at addressing issues of public concern, a government may also draw attention to the problem in question. In the process, the well-being effect of its action may well be negative, which potentially reduces public support (Liberini et al. 2017; Herrin et al. 2018). This raises the question whether the Chinese government is well-advised to widely publicise anti-corruption campaigns, i.e., whether a rational policy-maker would use 'Tiger-Hunting' signals as a way to consolidate political support.

On a more general note, our findings imply that a policy action may have unintended side effects for governments if the issue they seek to tackle becomes more salient in the process, in particular in the absence of political trust. An interesting avenue for future work would be to assess whether similar findings are observable when it comes to additional efforts by governments both in China and elsewhere to tackle economic or social issues of public concern. If tackling a specific problem (e.g. tax avoidance, income inequality) serves to highlight its prevalence, then in the presence of low political trust such efforts may not necessarily increase support for the government or serve to improve self-reported well-being.

Appendices

Table A1.1 Distribution of TH across different provinces in China (2010-2018)

Province	2010	2011	2012	2013	2014	2015	2016	2017	2018	Total
Panel A: 25 Provinces covered in this study (in line with CFPS)										
Beijing						1			1	2
Tianjin					1		2	1	1	5
Hebei					1	2	1	2	1	7
Shanxi					7	1				8
Liaoning					1		4	2		7
Jilin		1				2		1	1	5
Heilongjiang					2	1				3
Shanghai						1		1		2
Jiangsu				1	1	1	1	1	1	6
Zhejiang	1					1	1			3
Anhui				1	1		2	1		5
Fujian						2				2
Jiangxi	1			1	2	1	1		1	7
Shandong		1			1	1	1		1	5
Henan					1		2		2	5
Hubei				2		1	1	1		5
Hunan				1	1		1			3
Guangdong			1		2		2		1	6
Guangxi				1		1	1		1	4
Chongqing			1		1			3		5
Sichuan		1	1	2	1	1	2			8
Guizhou				1			1		2	4
Yunnan					2	1				3
Shaanxi					1	1		1	2	5
Gansu						1		2	1	4
Panel B: Provinces not covered in this study										
Inner Mongolia	1			1	1	2			2	7
Hainan					2		1			3
Tibet						1				1
Qinghai					1					1
Ningxia						1				1
Xinjiang						1	1	1		3
Total	3	3	3	11	30	25	25	17	18	135

Note. We display TH events in all provinces in mainland China in this table, however only 25 provinces (represent 95% the Chinese population) are covered in CFPS and thus only these provinces are included in our empirical study. The numbers in the table denote how many provincial officials have been investigated in each province during a certain year. As illustrated in the table, there are also some high-ranking officials investigated before 2012 but on a far smaller scale than thereafter. Investigations that happened before 2012 were also widely publicised by state media.

Table A1.2 Descriptive statistics

Variables	Mean					SD	Min	Max
	All	2012	2014	2016	2018			
Observations: 41,724								
Panel A. key variables								
Frequency of TH (QTH)	1.188	0.208	0.895	2.233	1.416	1.273	0	7
Per capita QTH (per 10 million people)	0.241	0.030	0.184	0.431	0.319	0.297	0	2.565
Per civil servant QTH (per 0.1 million civil servant)	0.200	0.030	0.151	0.364	0.256	0.229	0	2.292
LS	3.745	3.352	3.849	3.680	4.097	1.046	1	5
Political trust	5.060	4.925	5.031	5.086	5.197	2.622	0	10
Panel B. individual controls								
Age	51.35	48.35	50.35	52.35	54.35	13.37	18	93
Female (female=1)	0.517	0.517	0.517	0.517	0.517	/	0	1
Per Capital Family income (10k/Yuan)	1.494	1.101	1.349	1.502	2.025	1.959	0	84.7
CPC member	0.101	0.094	0.093	0.103	0.116	/	0	1
Residence (urban=1)	0.476	0.450	0.472	0.483	0.497	/	0	1
Hukou (rural=1)	0.693	0.698	0.694	0.691	0.690	/	0	1
Education level								
Illiteracy	0.267	0.271	0.271	0.271	0.255	/	0	1
Primary school	0.237	0.238	0.239	0.238	0.234	/	0	1
Middle school	0.287	0.289	0.284	0.282	0.292	/	0	1
High school	0.137	0.138	0.139	0.136	0.137	/	0	1
Occupational college	0.044	0.041	0.042	0.045	0.048	/	0	1
University	0.028	0.023	0.026	0.028	0.034	/	0	1
CFPS interval (in months at individual level)	24.221	25.861	23.053	23.925	24.048	2.042	16	32
Panel C. provincial controls								
Contextual (actual) corruption	0.388	0.361	0.400	0.427	0.366	0.120	0.170	0.839
Government fiscal size	0.220	0.213	0.213	0.219	0.237	0.081	0.112	0.437
Ratio of civil servants	1.181	1.116	1.176	1.200	1.232	0.232	0.786	2.182
Per capita GDP (10k/Yuan)	4.635	3.414	4.443	5.123	5.560	2.208	1.40	11.96
GDP growth rate	9.086	12.45	9.894	7.480	6.522	2.897	0.85	16.90

Note. The frequency of TH events as well as all provincial variables over the past two years are matched to each wave of CFPS (see Sections 1.3 and 1.4).

Table A1.3 Robustness check: Estimation based on unbalanced panel

	Dependent variable: Life satisfaction			
VARIABLES	(1)	(2)	(3)	(4)
QTH	-0.008			-0.033***
	(0.006)			(0.011)
Per capita QTH		-0.051**		
		(0.022)		
Per civil servant QTH			-0.061**	
			(0.029)	
QTH × political trust				0.005**
				(0.002)
Provincial controls	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Month effect	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	80,296	80,296	80,296	80,296
Number of persons	26,025	26,025	26,025	26,025
Within R-squared	0.116	0.116	0.116	0.125

Note. Robust standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Provincial control variables and individual control variables are the same as in Table 1.1 and are defined according to Table A1.2. Family per capita income and per capita GDP have been log-linearised.

Table A1.4 Robustness check: Excluding pre-2012 data of TH disclosure (drop CFPS 2012)

	Dependent variable: Life satisfaction	
VARIABLES	(1)	(2)
QTH	-0.017**	-0.052***
	(0.008)	(0.014)
QTH × political trust		0.007***
		(0.002)
Provincial controls	Yes	Yes
Individual controls	Yes	Yes
Month effect	Yes	Yes
Individual FE	Yes	Yes
Observations	31,293	31,293
Number of persons	10,431	10,431
Within R-squared	0.124	0.132

Note. Robust standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Provincial control variables and individual control variables are the same as in Table 1.1 and are defined according to Table A1.2. Family per capita income and per capita GDP have been log-linearised.

Table A1.5 Robustness check: Measurement error of QTH

	Dependent variable: Life satisfaction	
VARIABLES	(1)	(2)
QTH	-0.015*	-0.054***
	(0.008)	(0.011)
QTH × political trust		0.007***
		(0.001)
Provincial controls	Yes	Yes
Individual controls	Yes	Yes
Month effect	Yes	Yes
Individual FE	Yes	Yes
Observations	41,724	41,724
Number of persons	10,431	10,431
Within R-squared	0.124	0.132

Note. Robust standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Provincial control variables and individual control variables are the same as in Table 1.1 and are defined according to Table A1.2. Family per capita income and per capita GDP have been log-linearised.

Table A1.6 Robustness check: Different econometric models

	Dependent variable: Life satisfaction					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	FE-Ologit		Random effect		HLM	
QTH	0.975*	0.904***	-0.012*	-	-0.010*	-
	(0.014)	(0.024)	(0.007)	(0.011)	(0.005)	(0.009)
QTH × political trust		1.015***		0.006***		0.005**
		(0.004)		(0.002)		(0.001)
Political trust		1.067***		0.044***		0.055**
		(0.008)		(0.003)		(0.003)
Provincial controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Month effect	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	41,724	41,724	41,724	41,724	41,724	41,724
Number of persons	10,431	10,431	10,431	10,431	10,431	10,431
Overall R-squared	-	-	0.041	0.088	-	-
Log likelihood	-22222	-22006	-	-	-58972	-58441

Note. We report the odds ratio rather than coefficients in columns 1 and 2. Robust standard errors clustered at province in parentheses in column 1 to 4. Standard errors in column 5 and 6. *** p<0.01, ** p<0.05, * p<0.1. Provincial controls and individual controls are the same as in Table 1.1 and are defined according to Table A1.2. Family per capita income and per capita GDP have been log-linearised.

Table A1.7 Descriptive statistics of additional variables in robustness checks

Variables	Observations	Mean	SD	Min	Max
Panel A Variables used in Table 1.4					
PQTH	225	0.529	0.840	0	7
Contextual corruption level	225	0.362	0.124	0.165	0.886
Per capital GDP (10k/Yuan)	225	4.706	2.460	1.122	12.62
GDP growth rate	225	9.941	2.933	-2.500	17.40
Government fiscal size	225	0.215	0.073	0.094	0.443
Ratio of civil servants	225	1.161	0.293	0.753	2.202
Population (10k)	225	5,135	2,501	1,228	11,169
Monetary supply (billion/Yuan)	225	28,60	21,55	3,65	12,603
Marketization index (10-points scale)	225	6.729	1.670	3.260	10
Trade volume (billion/Yuan) (Total volume of export and import)	225	927	1,513	186	7,938
Average schooling years	225	8.838	1.203	4.222	12.39
Panel B Variables used in Table A1.12					
Stalling	30,864	0.169	/	0	1
Charge	30,891	0.094	/	0	1
Panel C Variables used in Table A1.13					
Internet access	31,293	0.303	/	0	1
Internet importance	31,269	2.119	1.502	1	5

Table A1.8 Robustness check and further analyses

VARIABLES	Dependent variable: Life satisfaction			
	(1)	(2)	(3)	(4)
	Baseline	Excluding provinces with higher political status	Neighbouring effect	Long term effect
QTH	-0.011*	-0.012**		-0.023**
	(0.006)	(0.006)		(0.009)
Neighbouring QTH			-0.005*	
			(0.003)	
Lagged QTH				-0.004
				(0.007)
Provincial controls	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Month effect	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	41,724	34,852	41,724	31,293
Number of persons	10,431	8,713	10,431	10,431
Within R2	0.124	0.125	0.124	0.065

Note. Robust standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Provincial control variables and individual control variables are the same as in Table 1.1 and are defined according to Table A1.2. Family per capita income and per capita GDP have been log-linearised. All provincial control variables in column 4 are lagged for one period, in line with the key variable of lagged *QTH*.

Table A1.9 Robustness check and further analysis: the moderating role of political

VARIABLES	Dependent variable: Life satisfaction			
	(1)	(2)	(3)	(4)
	Baseline	Excluding provinces with higher political	Neighbouring effect	Long term effect
QTH	-0.043***	-0.046***		-0.023***
	(0.010)	(0.010)		(0.008)
QTH × PT	0.006***	0.006***		
	(0.001)	(0.002)		
Neighbouring QTH			-0.014***	
			(0.004)	
Neighbouring QTH × PT			0.002***	
			(0.000)	
Lagged QTH				0.002
				(0.016)
Lagged QTH × political trust				-0.001
				(0.002)
Provincial controls	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Month effect	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	41,724	34,852	41,724	31,293
Number of persons	10,431	8,713	10,431	10,431
Within R-squared	0.124	0.125	0.124	0.065

Note. Robust standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Provincial control variables and individual control variables are the same as in Table 1.1 and are defined according to Table A1.2. Family per capita income and per capita GDP have been log-linearised.

Table A1.10 Robustness check: Excluding five provinces that are over-sampled

	Dependent variable: Life satisfaction	
VARIABLES	(1)	(2)
QTH	-0.013*	-0.042***
	(0.006)	(0.012)
QTH × political trust		0.006***
		(0.002)
Provincial controls	Yes	Yes
Individual controls	Yes	Yes
Month effect	Yes	Yes
Individual FE	Yes	Yes
Observations	21,120	21,120
Number of persons	5,280	5,280
Within R-squared	0.124	0.134

Note. The five oversampled provinces are Henan, Gansu, Guangdong, Shanghai and Liaoning. Robust standard errors clustered at province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Provincial control variables, individual control variables are the same as in Table 1.1 and are defined according to Table A1.2. Family per capita income and per capita GDP have been log-linearised.

Table A1.11 Check on deterring effect: Evidence at provincial level

	Dependent variable: Contextual (Provincial) corruption level				
	(1)	(2)	(3)	(4)	(5)
The number of lagged years	Current year	One year	Two years	Three years	Four years
PQTH	-0.006	-0.003	0.006	0.007	0.005
	(0.006)	(0.005)	(0.004)	(0.006)	(0.010)
Year effect	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Observations	200	175	150	125	100
Number of provinces	25	25	25	25	25
Overall R-squared	0.262	0.270	0.264	0.287	0.365

Note. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. As the Procuratorial Yearbook of China is no longer available after 2018, our sample period is 2010 to 2017 in this table although we have data of TH in 2018. To avoid cutting off any potential transmission channels, we do not include any additional controls in this estimation.

Table A1.12 Check on deterring effect: Evidence at individual level

	Dependent Variable (DV): Individual direct corruption experience			
	Current DV		One period lagged DV	
	(1)	(2)	(3)	(4)
VARIABLES	Stalling	Charge	Stalling	Charge
QTH	0.002	0.001	0.006	0.004
	(0.002)	(0.001)	(0.005)	(0.003)
Provincial controls	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Month effect	Yes	Yes	Yes	Yes
Observations	30,864	30,891	20,576	20,594
Number of persons	10,288	10,297	10,288	10,297
Log pseudo likelihood	-12791	-8741	-13991	-9133

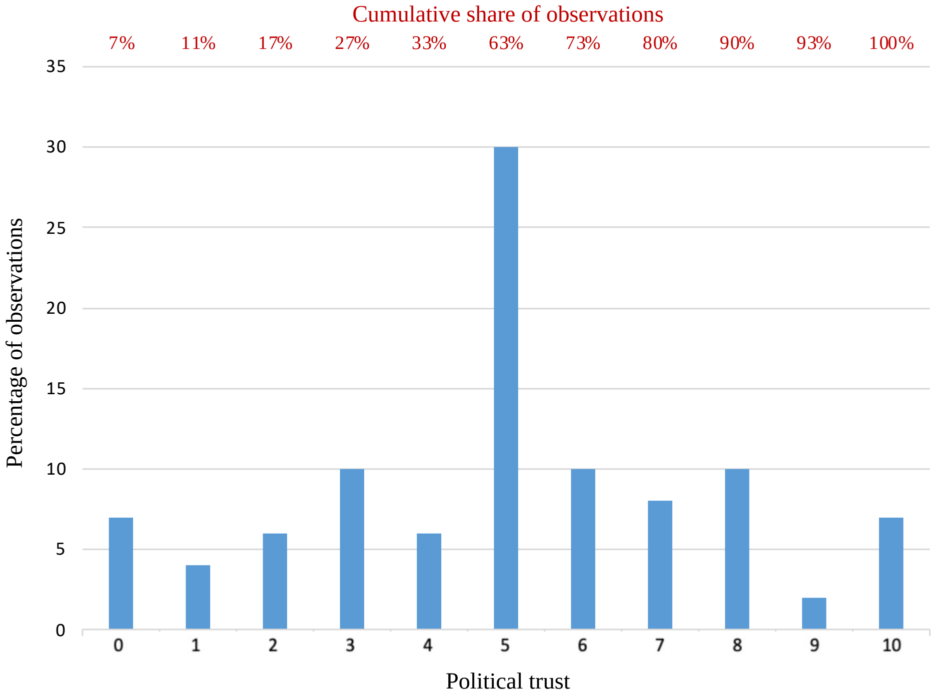
Note. The coefficients reflect marginal effects. Robust standard errors clustered at province level in parentheses. *** p<0.01, **p<0.05, * p<0.1. Provincial control variables and individual control variables are the same as in Table 1.1 and are described in Table A1.2. Family per capita income and per capita GDP have been log-linearised.

Table A1.13 The extent of exposure to TH disclosures does matter

	Dependent variable: Life satisfaction	
VARIABLES	(1)	(2)
QTH	-0.011	-0.001
	(0.007)	(0.007)
QTH × Internet access	-0.023**	
	(0.009)	
QTH × Internet importance		-0.008***
		(0.003)
Internet access	0.043**	
	(0.019)	
Internet importance		0.031***
		(0.008)
Provincial controls	Yes	Yes
Individual controls	Yes	Yes
Month effect	Yes	Yes
Individual FE	Yes	Yes
Observations	31,293	31,269
Number of persons	10,431	10,431
Within R-squared	0.066	0.066

Note. Robust standard errors clustered at province level in parentheses. Provincial control variables, individual control variables are the same as in Table 1.1 and are defined according to Table A1.2. Family per capita income and per capita GDP have been log-linearised.

Figure A1.1 Distribution of different levels of political trust



Note. These numbers are calculated based on our balanced panel sample covering CFPS 2012 to 2018.

Chapter 2 Happy Citizens Trust Their Rulers

Abstract

Using Chinese panel data, we examine whether citizen well-being impacts the formation of political trust, which is key to regime stability. Through a quasi-experimental method, we demonstrate how an improvement in subjective well-being directly leads to increased political trust. In a supplementary analysis, we also demonstrate how low political trust is predictive of actions that undermine regime stability. These findings suggest that any government, even an authoritarian one, has an incentive to foster the happiness of its citizens.

2.1 Introduction

Elections are generally regarded as an effective mechanism for holding governments accountable to the public interest (Downs 1957; Nordhaus 1975; Persson et al. 1997). An extensive literature in the field of retrospective voting has, for instance, shown that voters tend to reward incumbents in times of economic prosperity and punish them when times are hard (e.g. see Kramer 1971; Fair 1978; Fiorina 1978; Lewis-Beck and Stegmaier 2000). This helps to incentivize governments to act in the collective interest and limits rent-seeking (Ferraz and Finan 2011). Some recent research has looked to examine if there is an electoral incentive for governments to focus on citizen well-being beyond per capita GDP (Martínez Bravo 2016; Ng et al. 2017; Ward et al. 2021; Ng et al. 2022). While undoubtedly correlated, there can be a disconnect between economic prosperity and broader measures of subjective well-being, as GDP does not cover major determinants of individual welfare (Layard 2011).

Raising the flag of "happy voters", recent research by both Liberini et al. (2017) and Ward (2020) suggest that governments in democracies have an electoral incentive to improve their citizens' happiness. [1] Liberini et al. (2017) demonstrated how individuals who are more satisfied with their life were more likely to vote for the incumbent party in the United Kingdom. Ward (2020) reinforced this finding by showing that subjective well-being has been a significant predictor of real-stakes electoral outcomes in 15 European countries over four decades above and beyond objectively measurable indicators of economic prosperity.

While these results point to an electoral advantage for governments who manage to promote societal well-being, it is unclear whether a similar incentive exists in an authoritarian regime where elections are absent. Citizens in China still cannot directly decide the fate of government officials except for some limited elections at the village (community) level (Martinez-Bravo et al. 2022).[2] In a pyramid-like bureaucratic system, officials at all levels in China are appointed and evaluated by higher levels of government (Francois et al. 2023). An important question to start with is whether, in lieu of elections, there are other political objectives which an authoritarian regime likely cares about. We put forward political trust, namely people's trust in government

[1] Note that we are using the terms happiness and (subjective) well-being interchangeably as umbrella terms for subjective indicators of quality of life.

[2] In fact, these village or community committees are not a branch of the government but self-management organizations of the residents. In other words, they have no real administrative power.

officials (Hetherington 1998; Levi and Stoker 2000) as a plausible candidate, as political trust is key to the survival of any regime in the long run, even an authoritarian one. In doing so, we draw inspiration from a large body of empirical work which has previously highlighted the crucial role of political trust in effective governance (Braithwaite and Levi 1998). For example, it significantly underpins citizens' advocacy of government policies (Fairbrother 2019; Macdonald and Cornacchione 2023) and compliance to laws or regulations (Tyler 2006; Marien and Hooghe 2011; Bargain and Aminjonov 2020). Crucially, in the Chinese context, political trust has previously been shown to matter for regime stability (Zhong and Hwang 2016; Chen et al. 2021).[3]

Recognizing the importance of political trust for any regime, including authoritarian systems, we estimate the causal effect of subjective well-being on political trust in China. In other words, we ask whether happier citizens are more likely to exhibit trust in the government. If true, this could be seen as an incentive, even for an authoritarian government, to enhance the happiness of the general public. We establish the theoretical foundation more formally in a conceptual framework derived from social contract theory (Hobbes 1651; Locke 1689; Rousseau 1762). It asserts that, in any stable governmental system, there is naturally an intangible agreement between the ruler and the ruled in which both commit to certain mutually beneficial rules and obligations. Citizens realise that coordinated rules serve their welfare better than a state of anarchy, which is why they give up freedoms and delegate decisions to a trusted institution that imposes universally accepted rules, i.e., a government. Drawing from this framework we posit that people's political trust in the ruling government will be conditional on their well-being. In effect, to retain trust, governments must serve the public interest by delivering policies to enhance societal well-being.

Our empirical analysis is carried out using the China Family Panel Studies (CFPS, 2012-2018) dataset which is a nationally representative longitudinal survey. The dataset records various metrics of subjective well-being such as respondents' self-reported life satisfaction as well as their self-reported trust in local county-level government officials (hereafter referred to as political trust).[4] The difficulty in establishing a causal effect of life satisfaction on political trust is that an Ordinary Least Squares (OLS) estimation will likely be biased due to various endogeneity issues such as omitted variables and bi-

[3] In support of this, we show in a supplementary analysis how political trust is predictive of whether individuals intend to involve themselves in actions that may result in social unrest.
[4] We explain why this serves as a good measure of political trust in the Chinese context when introducing the data in Section 2.3.

directional causality. An example of the former would be policies enacted by the government that simultaneously affect subjective well-being and political trust. When it comes to bi-directional causality, there is research suggesting that political trust promotes happiness (Helliwell et al. 2020). Unlike this study, most of this research does not employ quasi-experimental methods to disentangle the two conceivable causal relationships between subjective well-being and political trust.[5]

A further source of endogeneity bias relates to measurement error. While commonly overlooked in social science research, measurement error is unavoidable in surveys because of a variety of reporting biases and other issues. This is likely to be particularly important when dealing with survey questions aimed at ascertaining how people think or feel as opposed to more objective criteria. The consequence of measurement error is that it causes one to underestimate the impact of the affected explanatory variable on an outcome variable, commonly referred to as attenuation bias. As an illustration of the consequences of measurement error, a number of studies on the effect of individual income (as captured through surveys) on life satisfaction have found that after correcting for measurement error using an instrumental variables (IV) method, estimated effect sizes are 2-5 times larger than conventional OLS estimates (Luttmer 2005: Powdthavee 2010; Howley 2017). Similarly, Berger and Spieß (2011) in a study of the effect of maternal life satisfaction on child outcomes such as verbal skills, found that effect sizes were more than 3 times larger after adjusting for attenuation bias using an IV method.

We combine two strategies to overcome these potential problems. First, given the longitudinal nature of the CFPS, we use an individual-fixed effects analysis which means we can account for any time-invariant omitted variables such as personality traits and family background. This approach is augmented by an IV approach to alleviate any remaining endogeneity issues. As with any analysis which relies on IVs, one might argue that the exclusion restriction is hard to prove. We address this concern by employing three separate IVs and argue that the consistency of our estimates across these three IVs points to the reliability of our estimates. It is highly improbable that all

[5] A notable exception is the work by Fu (2018) who employs internal instrumental variables (heteroskedasticity based instruments – see Lewbel 2012) to identify the effect of political trust on subjective well-being. This approach is useful when credible external instrumental variables are unavailable. We argue that our external instruments overcome endogeneity issues enabling us to identify the effect of life satisfaction on political trust.

instrumental variables would be biased in precisely the same manner, even more so here given that all IVs vary significantly in their nature.

Our first IV is respondents' physical attractiveness (PA) evaluated by CFPS interviewers. The intuition behind the selection of this instrumental variable rests on the fact that being physically attractive can lead to more positive evaluations from others in everyday life (e.g. see Lorenzo et al. 2010), and this in turn benefits individual happiness (Hamermesh and Abrevaya 2013). While likely strongly correlated with life satisfaction, we argue that there is no a priori reason to expect that evaluations of physical attractiveness by a third party, namely the CFPS interviewers, will be directly related with respondent's level of political trust. It is worth noting that, in this framework, any stable traits (personality, optimism) that might be simultaneously correlated with one's physical attractiveness and political trust will be absorbed by the individual fixed effects.

Our second IV indicates whether a respondent experienced physical discomfort in the two weeks just before the next CFPS interview. While a minor health shock (e.g. a fever or cough) is likely to temporarily impact self-reported indicators of well-being, it is, we would argue, very unlikely to change one's political trust. The third IV captures the closeness of the relationship of parents with their children. Intuitively, these kinds of family connections are unlikely to be directly related to political trust, all the more so in China since there is a longstanding custom in Chinese society where the government avoids involvement in family matters (Ocko 1991). On the other hand, the closeness of the parent-child relationship is significantly related with life satisfaction (Lowenstein et al 2007; Peng et al. 2019).

Our results show that life satisfaction has a significant and positive impact on the formation of political trust. Notably, our estimates remain remarkably similar to a battery of alternative specifications wherein we deploy these IVs individually or in various combinations. This strongly supports the suggestion that we are capturing the impact of life satisfaction on the formation of political trust, as opposed to the reverse, or that the relationship is due to another factor.

Our findings contribute to existing research in a number of ways. First, we add to the emerging "happy voters" literature which seeks to establish if governments have an electoral incentive to focus on measures of subjective well-being (Martínez Bravo 2016; Ng et al. 2017; Liberini et al. 2017; Ward 2020; Ward et al. 2021; Ng et al. 2022). To date, this research has focused on democracies. Our study can be seen as the first to consider this aspect in the context of an authoritarian regime where real elections are absent. Apart from the focus on democracies, one limitation with existing work is that, while it has done much to enhance our understanding of the importance of subjective well-being in the political sphere, findings may be affected by endogeneity concerns. As an illustration, when looking at simple associations, it can be difficult to establish the degree to which subjective well-being influences support for incumbents from the fact that people are likely to be happier when their political party has won. To the best of our knowledge, Liberini et al. (2017) provide the only study addressing this problem. They construct a difference-in-difference model with the death of one's spouse as an exogenous shock to happiness under the assumption that this life event does not impact on voting behaviour via other channels. Here, we employ three novel instrumental variables in establishing a causal link between subjective well-being and political trust.

We also contribute to research concerned with factors affecting the formation of political trust (Blanco and Ruiz 2013; Sangnier and Zylberberg 2017; Acemoglu et al. 2020; Abramson et al. 2022; Yao et al. 2022). Two approaches dominate the existing research in this area (Mishler and Rose 2001). The first one, an institutional approach, posits that government performance (particularly economic performance) is the determinant of political trust (e.g. see Hetherington and Rudolph 2008; Stevenson and Wolfers 2011; Van Erkel and Van Der Meer 2016). The second one, a cultural approach, argues that political trust originates from social norms and values rooted in the history of a society (e.g. see Chen et al. 2020; Nunn and Wantchekon 2011). We add to the institutional approach with a nascent measure of government performance, namely subjective well-being. A potential problem with the traditional institutional approach, particularly in authoritarian regimes, is that official statistics are susceptible to government manipulation (e.g. GDP, see Henderson et al. 2012). Using subjective well-being as a metric of government performance can alleviate this issue, as people's true feelings expressed in surveys are more difficult to manipulate than governments' own statistics.

Finally, there has been a wealth of existing research highlighting factors that go beyond income and wealth in making people happy. Much less is known about the reverse. Rather than being a consequence of people's actions, equally happiness could be a causal force in shaping how people feel and act. The nascent literature that does exist in this area has shown that happiness can predict productivity (Böckerman and Ilmakunnas 2012; Oswald et al. 2015; Bellet et al. 2023), future divorce or separation (Clark et al. 2008; Guven et al. 2012), labour market choices (Krause 2013; Gielen and van Ours 2014) and, as discussed earlier, voting behaviour (Liberini et al. 2017; Ward 2020). This paper adds to this body of work by examining the role that happiness plays in shaping political trust.

The remainder of this paper is organised as follows. In Section 2.2, we present social contract theory as a useful framework through which we develop our hypothesis in relation to the importance of subjective well-being for the formation of political trust. Section 2.3 outlines our data sources. Section 2.4 describes the empirical strategy. In Section 2.5, we document our main findings and heterogeneity analysis. This is followed by Section 2.6 which presents various robustness checks and further analyses. Lastly, we conclude with a discussion of our main findings in Section 2.7.

2.2 Conceptual framework

Beginning with the innate drive for individuals to maximize their own well-being, social contract theory posits that the relationship between citizens and the state is fundamentally defined by a reciprocal obligation (Hobbes 1651; Locke 1689; Rousseau 1762). Specifically, it posits that the ruling authority must commit to serving the interests of the citizens of the state, and in return citizens will consent to their authority for their own benefit. Although social contract theory has received considerable criticism over the last centuries (Loewe et al. 2020), the basic idea of mutual obligations and reciprocity remains influential in contemporary political economy (Besley 2020). For instance, it offers a useful framework for understanding contemporary state-citizen relations such as the link between tax compliance and the quality of public services delivery (Feld and Frey 2007; Levi and Sacks 2009).

The phenomenon of retrospective voting in modern democracies also aligns with social contract theory: rewarding well-performing incumbents while punishing incompetent ones is an act of reciprocity. Retrospective voting demonstrates that people who constitute the state do actively seek to ensure the public power serves their interest. One can expect that citizens in authoritarian regimes will also aspire to make their governments accountable to the public interest. The question is, however, how can they incentivize those in power to promote their well-being in the absence of elections? We suggest that when the ruling elite does not serve the interests of their citizens, they are perceived to be in breach of the implicit social contract and in turn political trust is damaged. This could potentially threaten the survival of the nomenklatura as political trust is essential to regime stability even in an autocracy where elections are absent. For instance, looking specifically at China, political trust has been shown to be important in predicting political protests (Zhong and Hwang 2016; Chen et al. 2021).

Beyond just mere survival, political trust is vital for the general efficacy of (authoritarian) governments. As an illustration, previous work has shown how low political trust makes tax evasion more likely and, more broadly, damages social trust and cooperation (Fukuyama 1996; Putnam 2000). Political trust has also been shown to be important in predicting the acceptability of government initiatives in China such as long-term compensation as opposed to a one-cash deal for land expropriation (Cai et al. 2020) and the implementation of emission trading schemes (Gao et al, 2022). Research by Zhang et al. (2022) illustrated how the public disclosure of anti-corruption investigations in China, which are aimed at convincing the public of the government's intention to crack down on corruption, negatively impacted the life satisfaction of those with low political trust, but had the opposite effect for those with high trust. This suggest that political trust may moderate the effectiveness of a governments messaging efforts.

In summary, the importance of political trust renders it a plausible alternative to voting for Chinese citizens when it comes to incentivizing their government to work in their interests. Indeed, perhaps due to the recognition of the importance placed on political trust by the central government, the likelihood of a county governor in China being promoted correlates positively with the level of political trust among local residents (Yao et al. 2022). We thus posit that, in lieu of voting, Chinese citizens offer their political trust in exchange for actions which enhance their individual well-being. In essence, in the absence of elections, political trust is what the Chinese people can offer in the social contract of reciprocity with their government.

If one accepts the premise that political trust is important for the ruling party, the question becomes how individuals judge whether the government is committed to the contract and hence worthy of their trust. We argue that much like subjective well-being is important in predicting incumbent support, it will be key in shaping the degree to which citizens trust their rulers. This is simply because of the aforementioned innate human drive for being happy, and the idea that the government may be able to contribute to one's happiness.

Traditionally, economic performance has been put forward as the most important criterion by which citizens assess government performance (e.g. see Kramer 1971; Markus 1988; Chappell 1990; Lewis-Beck and Stegmaier 2000; De Benedictis-Kessner and Warshaw 2020). However, voters oftentimes do not appear to vote in accordance with their country's (or indeed individual) economic interest. This suggests that income is only one of many aspects people take into account when evaluating government performance. Indeed, one reason why we observe the strong relationship between macro variables such as economic growth and the probability of incumbents being re-elected may simply be that voters' happiness is, by and large, enhanced by a strong economy. In support of this, the emerging literature on "happy voters" indicates that measures of subjective well-being are able to explain more of the variance in governing party vote share than standard macroeconomic indicators (Ward 2020). This is essentially in line with the literature discussed in the introduction claiming that government performance is a determinant of political trust, while we argue that a main mediator in this relationship would be subjective well-being. Put simply, government performance will affect subjective well-being, and in turn causes changes in political trust.

Note, however, that we do not argue that citizens clearly discern the sources of their well-being and in turn only link the variations caused by the government to their political trust. Previous studies in this field have shown that citizens' support of the government is affected by life events that are separate from politics, but influence their personal well-being. Examples include the death of a spouse and local college football wins (Healy et al. 2010; Liberini et al. 2017; Esaiasson et al. 2020).

One possible reason for this is when people are feeling unhappy, unhappiness can be partly alleviated by searching for someone or something to blame (Schwarz and Clore 1983) and so individuals may incorrectly attribute their dissatisfaction and negative emotions to others, particularly salient others like the government. This is not to dissuade governments from looking to maximise the happiness of its citizens. It has been shown previously that there is a clear electoral dividend from doing so, rather it highlights how we are not very good at correctly attributing the source of our happiness.

2.3 Data

Our data come from the China Family Panel Studies (CFPS, Institute of Social Science Survey, Peking University 2015). This longitudinal nationally representative survey has been conducted every two years since 2010 with the last fully released wave in 2018. The CFPS samples approximately 32,000 adults (age ≥ 16 in each wave) across 25 provinces which represent 95% of the total population in China.[6] We use four waves

[6] The 31 provinces in mainland China are covered by CFPS except Xinjiang, Tibet, Qinghai, Inner Mongolia, Ningxia and Hainan.

(2012, 2014, 2016 and 2018) of the CFPS in our analysis as our measure of political trust only became available with the 2012 wave. Our measure of political trust is based on answers on an eleven-point scale to the following question: "*How much do you trust the cadres (officials in local county-level government)*". 0 indicates least trust and 10 highest trust. This question has been used as a measure of political trust in various previous studies (e.g. Cai et al. 2020; Yao et al. 2022; Zhang et al. 2022; Sha 2023).

Two main reasons make this measure of political trust suitable for our study. First, although politically centralized, Chinese local governments are granted considerable autonomy in developing the local economy and providing public goods (Qian and Roland 1998; Jin et al. 2005). Second, local governments are also responsible for executing policies enacted by both local and central authorities, which means that citizens interact more directly with local officials than with representatives at higher government levels.[7]

Our main measure of subjective well-being is life satisfaction, which is based on a question consistently asked across all waves of the CFPS: "*Are you satisfied with your life*?" The scale ranges from 1, least satisfied, to 5, most satisfied. The CFPS also records a measure of self-reported happiness, which we employ as an alternative indicator of subjective well-being in a sensitivity check. This measure is available in two waves (2014 and 2018) and respondents are presented with a 10-point scale and asked: "*Are you happy overall?*", with 1 indicating least happy and 10 most happy.

The vast majority of respondents in the CFPS have not migrated across counties during our study period. Approximately 10% have migrated between counties across CFPS waves and we exclude these respondents in our baseline analysis as our measure of political trust relates to trust in local county government officials. Migrating might be associated with changes of satisfaction and lead to changes in the trust in local government officials simply because the county changes and with it the officials respondents refer to when indicating their level of political trust. Hence, if migrants were included, this could artificially produce a correlation between changes of satisfaction and changes of political trust.[8]

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[7] The Chinese political system includes six levels of bureaucracy: national level, provincial-ministerial level, prefectural-bureau level, county-division level, town-section level, village-community level (informal government organization). There are 2,843 county-level administrative districts in mainland China.

[8] We conduct a robustness check by including migrants and get qualitatively the same results, see Appendices Table A2.4.

One may be concerned that there is a self-censorship issue when it comes to our measure of political trust given the political sensitivity of the question in the Chinese context. For example, respondents who do not trust the government may be particularly reluctant to answer this question or simply give a politically expedient answer (e.g. a score that is much higher than their actual level of trust). Yao et al. (2022), who use the same dataset (CFPS 2012 to 2018) as ours when looking at how air pollution affects people's political trust, have conducted a detailed examination of this issue. Their results suggest that self-censorship is highly unlikely. Rather than repeating their analysis, we summarize their findings as follows. First, the response rates are quite similar between trust in government officials and trust in other groups (e.g. parents and strangers) where political sensitivity is less of an issue. Second, political trust in the CFPS has high internal validity. For instance, those who had unhappy experiences with government officials (e.g. unreasonable charges and stalling) in the past 12 months before the CFPS interview report significantly lower political trust. Third, political trust in the CFPS is normally distributed, resembling the distribution of political trust as measured by the Asian Barometer Survey[9] which is conducted anonymously online. Despite these findings, we conduct our own additional robustness checks to further alleviate any concerns surrounding self-censorship.[10] This discussion is included in supplementary analyses A of our online Appendices. It shows that our results are very unlikely to suffer bias from self-censorship.

Instrumental variables

An advantage of using the CFPS is that we are able to obtain instrumental variables (IV) within the dataset to help alleviate endogeneity concerns. Reverse causality, measurement error and, despite our use of individual-fixed effects, time-variant omitted variables are potential sources of bias. This means a quasi-experimental approach is useful to reliably identify the effect of subjective well-being on political trust. Here we

[9] The Asian Barometer Survey is a cross-national comparative survey focusing on socioeconomic modernization, regime transition, democratization and changes in political values across the East-Asian region. It is conducted anonymously online so that the data is highly reliable when it comes to political attitudes.

[10] A simple method to check this issue could be to include answers of "don't know" and "refused to respond" in the analysis (see, e.g. Otrachshenko et al. 2023). However, in our dataset the share of such answers is very small (<0.6%), which means that including them into any category of the normal answers (0 to 10 points) does not yield meaningful changes in our results. We thus turn to other approaches which are detailed in the supplementary analyses A.

employ an instrumental variables approach (see Section 2.4 for further details of the empirical strategy).

Our first instrumental variable for individual life satisfaction is the physical attractiveness (PA) score of the respondent. CFPS interviewers are asked to rate the physical attractiveness of respondents on a seven-point scale. The question comes at the end of the face-to-face interview, with 1 indicating least attractive and 7 most beautiful. The rationale underpinning this instrumental variable is simply that attractiveness should be positively correlated with life satisfaction, but how one's attractiveness is rated by a third party should not be related to their political trust.

Our second IV is based on a survey question which asks whether a respondent experienced physical discomfort (PD) in the 2 weeks before the interview. Specifically, between 2012 and 2018, respondents are consistently asked to answer "yes" or "no" to the following question: "*During the past two weeks, have you felt any physical discomfort?*". Approximately 30% of respondents report PD in each survey wave. While likely to impact how people will rate their subjective well-being, we argue that it is very unlikely that these short-term health shocks (e.g. a fever or a cough[11]) would be directly related with political trust.

Our third IV comes from a survey item capturing the closeness of familial relationships, reported by a subset of respondents. In 2012, 2016 and 2018, the CFPS asked respondents aged over 60 to report their relationship with each one of their children. Specifically, they are presented with five options (*1. Not close at all 2. Not very close 3. Fair 4. Close 5. Very close*) and asked: "*In the past 6 months, how was the relationship between you and your child (child name)?*".[12] For ease of description, we refer to this variable as parent-child relationships (PCR). Intuitively, within-family relationships should have a significant impact on people's life satisfaction, while being very unlikely to be directly associated with their political trust. To maximise sample size, we assign this PCR score to each family member of the respondents who report

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[11] Not all waves of CFPS survey record detailed types of physical discomfort. This information is however collected in waves 2014 and 2016, where respondents are asked to not just report whether they experienced physical discomfort (asked in all waves) but also the type of physical discomfort amongst the following options options: *1. Fever 2. Pain 3. Diarrhea 4. Cough 5. Difficulty in breathing 6. Cannot focus attention 7. Difficulty in walking 8. Palpitation*. There is also an additional choice of "*9. Other*".

[12] We use the mean value of the relationship scores with each child to represent one's parent-child relationship.

PCR.[13] The rationale is that closeness of an interpersonal relationship such as this would not only influence the reporters themselves, but also their children and other family members as it could affect general family harmony.

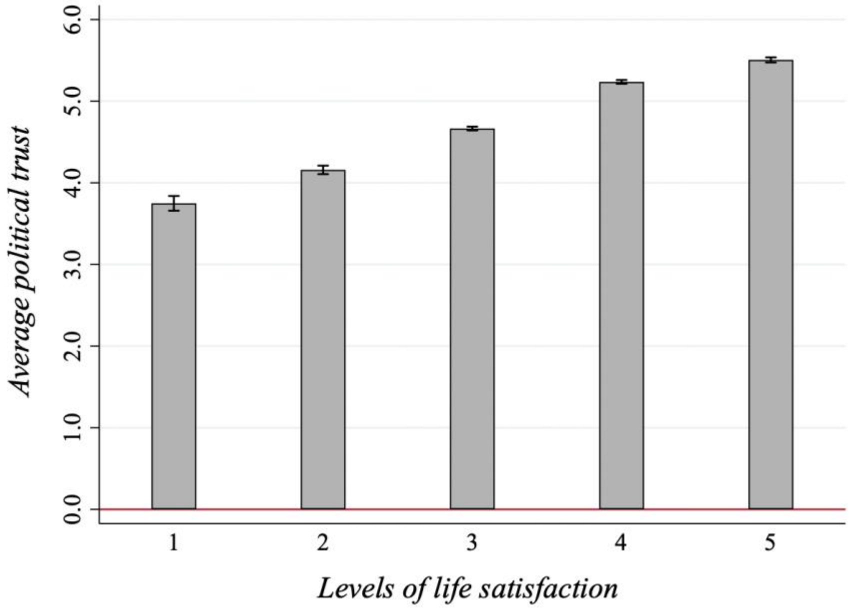
The CFPS captures rich demographic information, enabling us to control for individual characteristics in our empirical model.[14] Specifically, our main specification includes age, marital status (single, married, divorced and widowed), level of education (six levels from being illiterate to having a university degree), residence (urban or rural area), *Hukou* (permanent urban residency permit)[15], political status (member of the Communist Party of China, CPC), occupation status (10 types of status, see details in Appendices Table A2.1), family per capita income and household size.

Our final sample consists of an unbalanced panel of 77,891 observations. A concern with unbalanced panel datasets is that sample attrition may lead to selection bias. We examine this issue using the method proposed by Verbeek and Nijman (1992). In the first step, we generate an indicator of attrition (dummy variable, 0 denotes missing value, otherwise 1), then add this lagged attrition indicator to the original empirical model as a control variable. An insignificant coefficient of the lagged attrition indicator would suggest that attrition does not cause selection bias in the estimation.[16] This is the case in our study, as we show in Appendices Table A2.6.

For an overview of summary statistics, we refer the reader to Appendices Table A2.1. The mean values of life satisfaction and political trust are 3.723 and 5.045, respectively. As a starting point in our analysis of the impact of life satisfaction on political trust, we plot the average political trust over different levels of life satisfaction. Figure 2.1 shows that the average political trust increases substantially with life satisfaction, which lends some initial support for our hypothesis that happier citizens are more likely to have trust in government officials.

[13] In the CFPS, each individual is associated with a unique family code. When an individual reports PCR, we assign this score to all individuals sharing the same family code. If there are more than one person aged over 60 in a family, we use the mean value of the PCR reported by these family members.
[14] Employing a more parsimonious model by excluding these individual control variables does not change our results in any meaningful fashion.
[15] Note that simply working and living in an urban area does not mean a person has urban *Hukou*.
[16] For the details of the method, we refer readers to Verbeek and Nijman (1992) or Wooldridge (2010).

Figure 2.1 Average political trust at different levels of life satisfaction



Note. The calculations are based on the CFPS samples used in our empirical analysis. Whiskers denote 95% confidence intervals.

2.4 Identification strategy

To identify the causal effect of life satisfaction on political trust, we employ a dynamic instrumental variables (IV) method. To this end, both stages of regressions are implemented via an individual-fixed effects (FE) model. In the process, any time-invariant confounders between life satisfaction and political trust are removed as well as any stable linkages between the IV and political trust. Several instrumental variables sourced from different survey questions in the dataset are used. Consequently, the exclusion restriction can be more convincingly verified: if these distinct IVs produce comparable results, it becomes highly improbable that any potential violations of the exclusion restriction would bias the results in the exact same way.

We start our analysis with a simple FE model as shown in the following equation:

$$PT_{it} = \alpha + \beta \cdot LS_{it} + \boldsymbol{Control}_{it} \cdot \boldsymbol{\lambda} + \vartheta_t + \eta_i + \varepsilon_{it} \quad (2.1)$$

PT_{it} is the political trust of individual i in wave t, LS_{it} denotes life satisfaction. $\boldsymbol{Control}_{it}$ is a vector of time-varying control variables at the individual level. The wave (time) fixed effects and individual-fixed effects are captured by ϑ_t and η_i, respectively. As respondents who have migrated across counties are excluded from the sample, any province as well as county-fixed effects will be absorbed by the individual-fixed effects. The standard errors are clustered at both individual and county levels throughout the paper.[17] ε_{it} is the error term. β is our coefficient of interest and captures the effect of life satisfaction on political trust.

A causal identification of the relationship between life satisfaction (LS_{it}) and political trust by equation (2.1) relies on the assumption that LS_{it} is uncorrelated with the error term ε_{it}. Although the fixed-effects model removes the influence of all time-invariant confounders, the assumption of strict exogeneity is possibly violated. First, there could still be some time-varying confounders omitted from our model, for example, some government policies in certain periods may improve (or harm) people's life satisfaction and political trust simultaneously. Second, previous studies have found that political trust could be a source of individual happiness (Helliwell et al. 2020), and therefore reverse causality could also lead to a correlation between LS_{it} and ε_{it}. Third, when capturing a subjective construct such as one's own happiness through a questionnaire survey, measurement error is inevitable at least to some degree (Weimann et al. 2015) which often biases estimates towards zero (e.g. see Swaffield 2001).

To address these endogeneity concerns, we implement a two stage least squares (TSLS) approach in which we use instrumental variables (IV) to capture exogenous variations in respondents' life satisfaction. The first stage regression is given by Equation (2.2), in which life satisfaction is regressed on the IV (Z_{it}) as well as all other covariates of Equation (2.1). We then obtain a predicted indicator of life satisfaction ($\widehat{LS}_{it}$) which should be strictly exogenous (i.e. purged of any endogeneity bias) and can be used to estimate the causal effect of life satisfaction on political trust in the second stage regression (Equation 2.3).

$$LS_{it} = \epsilon + \emptyset \cdot Z_{it} + \boldsymbol{Control}_{it} \cdot \boldsymbol{\gamma} + \theta_t + \sigma_i + \mu_{it} \quad (2.2)$$

$$PT_{it} = \alpha + \beta \cdot \widehat{LS}_{it} + \boldsymbol{Control}_{it} \cdot \boldsymbol{\lambda} + \vartheta_t + \eta_i + \varepsilon_{it} \quad (2.3)$$

Our first IV is respondents' Physical Attractiveness (PA) score as reported by the CFPS interviewers. The intuition behind the selection of this instrument is that PA is found to have a strong and direct impact on subjective well-being (Hamermesh and Abrevaya 2013), as attractiveness is positively correlated with the responses people get from

[17] It is worth mentioning that this might make our estimation overly conservative when it comes to statistical inference (Abadie et al. 2022). We also show alternative approaches in Appendices Table A2.7, where we cluster standard errors (one way) at individual level, family level, county level and CFPS interviewer level respectively, and cluster standard errors (two way) at both family and county levels, and, both county and CFPS interviewer levels. We also report heteroscedasticity robust standard errors. Our results are consistent across all of these approaches. To utilize a larger sample size, we use the combination of physical attractiveness (PA) and physical discomfort (PD) as the IV set in this check.

others in everyday life (Lorenzo et al. 2010). On the other hand, one has no reason to expect that citizens will change their political trust based on how interviewers privately rate their looks, i.e., without sharing their rating with the interviewee.

One might contend that, all things being equal, interviewers could rate the attractiveness of interviewees who report high political trust higher. In other words, these variables could be related with each other through a channel other than through life satisfaction. We find no evidence for this. Using a fixed effects model, we regressed the PA rating on political trust, controlling for life satisfaction as well as our baseline covariates, and political trust attracted a statistically insignificant coefficient and one close to zero in absolute terms (0.005). This suggests that there is no direct relationship between how interviewers rate the attractiveness of interviewees and how interviewees report their political trust.

One possible threat to the validity of using PA as an instrumental variable is the so-called "beauty premium" (Hamermesh and Biddle 1994). Specifically, previous literature has shown that PA correlates with educational achievement, attractiveness in the marriage market and labour market outcomes (Hamermesh 2011). One may thus contend that our instrument could influence one's political trust through these indirect channels. This is unlikely to be a factor here for several reasons. First, we include a detailed set of covariates reflecting success in life such as education, income, occupation and marital status. Second, our first stage regression is also a fixed-effects model in which only within-person variation is considered. This means that any time-invariant characteristics which may be related to both PA and political trust, for example personality traits, will not produce bias.[18]

It is worth noting here that most empirical studies which do support the existence of a "beauty premium" rest on between-person differences, in which PA usually correlates with other individual characteristics. Kanazawa and Still (2018) reveal that a "beauty premium" is not observed when individual differences, most of which are stable characteristics such as personality, intelligence and family background, are controlled for. This is exactly what we achieve by employing our fixed-effects model, in addition to the use of detailed control variables.

Nevertheless, we look to further alleviate these concerns, following approaches commonly used for demonstrating instrument validity in the existing literature (e.g.

[18] Between-wave changes in PA could come from, for example, changes in body shape, dressing style or make-up applied on the day of the interview.

Acemoglu et al. 2001). We first examine whether a beauty premium impacts our estimation by using additional control variables and excluding individuals from our analysis who are most likely to be impacted by a beauty premium if it exists. These findings suggest that our results are unlikely to be biased by a 'beauty premium'. The details of these checks are included in the supplementary analyses B of our online Appendices.

Our second IV is the binary indicator physical discomfort (PD) which denotes whether a respondent experienced physical discomfort in the two weeks before the CFPS interview. On the one hand, changes in physical health are obviously correlated with one's subjective well-being, implying that the instrument should be strong. On the other hand, one would not expect a minor health shock (e.g. a cough or fever) to affect someone's political trust, so the instrument should also be valid. A possible exception is if the change in health alters people's evaluation of public medical services. Here, one could argue that respondents might adjust their political trust, accordingly (Mattila and Rapeli 2018). We therefore control for respondents' evaluation of medical services (EMS) when using this instrumental variable.[19]

The third IV is a variable capturing the closeness of parent-child relationships (PCR) within the family which is available in 2012, 2016 and 2018. This should be a strongly relevant IV as previous research has shown that such relationships are important for life satisfaction (Lowenstein et al. 2007; Peng et al. 2019). On the other hand, such within-family relationships are unlikely to be directly linked to political trust, particularly so in China, as there is a long-standing tradition that the government refrains from intervening in domestic affairs (Ocko 1991). Note that here we have a smaller sample size, and the sample is also relatively older as it is drawn from households with individuals aged over 60.

A prominent advantage of having three independent IVs is that we can provide credible evidence for supporting the exclusion condition, which is hard to achieve in the case of relying on a single IV. Our intuition is simply that if any one of these IVs violates the exclusion condition (which is unlikely as we argued above), the estimate based on that IV should be biased, and one should, in turn, expect to obtain different results if using different IVs. In contrast, a similar result across all IVs would suggest that all IVs are valid and our estimates are reliable, except for an extreme possibility that all IVs are

[19] In reality, excluding/including this variable has little to no impact on our key finding, i.e., the size of the effect of life satisfaction on political trust.

invalid, yet they coincidently cause similar bias. Moreover, if the estimates derived from using these IVs combined as well as individually remain similar, one can only expect these IVs to not only cause a similar size of bias, but also through very close channels. It is challenging to justify such a scenario considering that the three IVs originate from distinctly different questions. Therefore, we implement the IV estimation by deploying these IVs both individually and in various combinations.

2.5 Empirical results

2.5.1 Main findings

We first look at the within-person correlation (individual fixed-effects (FE)) between life satisfaction and political trust. The results are presented in Table A2.2. In columns 1 and 2, we can see that life satisfaction is significantly correlated with political trust, irrespective of whether we include or exclude individual-level control variables and wave fixed-effects. As mentioned before, a strength of the FE model employed is that we do not have to worry about time-invariant omitted variables biasing our estimates, such as stable personality traits (see Allison 2009). A limitation of the FE model is that by solely relying on within-person variation, it might be overly conservative. As a sensitivity check, we therefore also report estimates using a random effects (RE) model. This takes between-person variation into account increasing the statistical power at our disposal, but the downside is that omitted variable bias is more likely in this setting. The results in column 3 show that the coefficient of life satisfaction is significant and broadly similar to that obtained when using the FE model.

Notwithstanding the fact that our FE analysis is based on within-person variation, it could still suffer from endogeneity bias as discussed in Section 2.4, for example due to bi-directional causality and measurement error. We therefore re-estimate the effect of life satisfaction on political trust using fixed effects coupled with an instrumental variable approach (FE-IV). We first use the three IVs namely physical attractiveness (PA), physical discomfort (PD) and parent-child relationship (PCR), individually. The results are presented in columns 1 to 3 of Table 2.1.

The first stage regressions indicate that all three IVs are significantly correlated with life satisfaction as the F-statistics range from 21 to 33,[20], suggesting that all of them are strong instruments. The second stage regressions show that life satisfaction has a significant and positive effect on political trust. All three estimates using separate IVs are remarkably similar, ranging from 0.896 to 1.050. This consistency supports the validity of our IVs. For our IVs to be invalid, one would have to assume that all of them are not only endogenous, but also coincidently cause a similar bias in the estimation. Such a speculation can hardly be justified given that the three IVs are all quite different in nature, namely a PA score rated by the CFPS interviewers (as opposed to by the respondents), self-reported measures capturing a short-term health shock and an assessment of the closeness of family relationships.

To further enhance the assumption on the exclusion condition, we use different combinations of these IVs as the set of instrumental variables respectively, this includes: 1) PA and PD, 2) PA and PCR, 3) PA, PD and PCR[21]. The results in column 4 to 6 remain similar to previous estimates. In addition to being robust to different combinations of instrumental variables, our instruments pass the usual validity tests, i.e., a test for over-identification restrictions (the p-value of Hensen J statistics are larger than 0.1 across all three specifications). This reaffirms that our IVs do not violate the exclusion condition. In other words, our IV estimation on the impact of life satisfaction on political trust is credible and can be interpreted as a causal relationship.

One may note that the coefficients size in our IV estimation is much larger compared to our baseline FE estimates. The larger magnitude of the FE-IV estimates relative to the FE estimates suggests that endogeneity indeed biases the latter. A possible explanation for the sizeable downward bias in the FE estimation is measurement error as political trust is self-reported and an underlying psychological construct which is hard to capture in surveys. Many studies have found that measurement error in an

[20] A rule of thumb is that instruments are not weak if the F-statistic of the first stage regression exceeds 10. This empirical rule however is not sufficient and sometimes may cause over-rejection in the second stage regression if the IV is not strong enough (Lee et al. 2022). We thus also report the results of an Anderson-Rubin Wald test which is more robust. The test result in column 5 of Table 2.1 rejects the null hypothesis that the coefficient of the endogenous regressor in the main equation (second stage) is equal to zero (p = 0.039), suggesting that the inference on the coefficient of the endogenous variable is valid even in the presence of a weak instrument.

[21] In this IV set, we find that PD does not attract a significant coefficient in the first stage regression. The reason perhaps is that the samples here consist of comparatively older people who are more likely to experience physical discomfort and have become more adaptable to such conditions. In other words, their life satisfaction is less sensitive to physical discomfort. This is why here we do not separately use the combination of PD and PCR as an IV set.

independent variable can lead to attenuation bias in the estimate, i.e., a significant downward bias (e.g. Lassen 2005; Aydemir and Borjas 2011).

According to the most conservative result (column 4) in our FE-IV estimation, an increase of one standard deviation of life satisfaction (SD, 1.056 points) would lead to an increase in political trust of 0.924 points, or 35% of a SD (2.638 points). To better gauge the significance of this finding, we compare the impact of life satisfaction on political trust to that of being a member of the Communist Party of China (CPC). As the ruling party in China, CPC members are unsurprisingly found to have significantly higher political trust. Considering the low within-person variation when it comes to CPC membership,[22] we use the results of our RE model to obtain this estimate (β = 0.504, p = 0.000). The effect on political trust of a one-SD increase in life satisfaction is almost two times as large as the estimated effect of being a CPC member (184%).

[22] This is because only a small number of respondents in our sample joined the party during the sampling period and once joined quitting is usually not allowable.

Table 2.1 The impact of life satisfaction on political trust: Results from IV estimation

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. First stage						
	Dependent variable: Life Satisfaction					
Physical attractiveness	0.028***			0.028***	0.023**	0.023**
	(0.006)			(0.006)	(0.011)	(0.011)
Physical Discomfort		-0.056***		-0.055***		-0.019
		(0.012)		(0.012)		(0.238)
Parent-Child Relationship			0.104***		0.100***	0.096***
			(0.018)		(0.018)	(0.018)
F-statistic	21.43	23.31	33.89	18.90	20.42	13.14
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	77,891	77,891	19,083	77,891	19,083	19,083
Number of persons	26,107	26,107	8,221	26,107	8,221	8,221
Panel B. Second stage						
	Dependent variable: Political Trust					
Life Satisfaction	0.896**	0.938*	1.050***	0.875***	1.240***	1.144***
	(0.457)	(0.515)	(0.396)	(0.347)	(0.360)	(0.364)
Anderson-Rubin Wald F-statistic	4.37	3.44	7.56	3.72	6.29	3.88
(p-value)	(0.038)	(0.065)	(0.007)	(0.026)	(0.002)	(0.010)
Hansen J statistic				0.020	0.685	2.432
(p-value)				(0.888)	(0.408)	(0.296)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	77,891	77,891	19,083	77,891	19,083	19,083
Number of persons	26,107	26,107	8,221	26,107	8,221	8,221

Note. Individual control variables include age (squared), marital status, education level, residence, Hukou, CPC membership, occupation status, family per capita income (log-linearised) and household size. In columns 5, 7 and 9 where the IV of physical discomfort is used, we also control for individuals' evaluation of medical services. All control variables are described in Table A2.1. Robust standard errors clustered at individual and county levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

2.5.2 Heterogeneity analysis

In this section, we explore whether the effect of life satisfaction on political trust is heterogeneous between different socio-demographic subgroups. To this end, we separately split the sample into two roughly even subgroups based on gender, residence (urban v rural), age, family per capita income and education level, respectively.[23] Here the IV estimation is carried out based on the combination of physical attractiveness (PA) and physical discomfort (PD). The results are presented graphically in Figure 2.2, while the detailed results may be obtained from Appendices Table A2.3. The estimated differences across subgroups are often large, but they are mostly statistically insignificant due to large standard errors, except for the urban-rural gap. These findings should therefore be interpreted with caution.

Figure 2.2 reveals that the estimated effect of life satisfaction on political trust is comparatively large for the subgroup with below average income. These groups are more likely to receive government aid such as unemployment subsidies and targeted poverty alleviation measures. This may lead to a greater perception that their individual well-being is dependent on the government compared to their wealthier counterparts. We note here, however, that this explanation as well as the following interpretations are somewhat speculative and derived ex post.

The figure further shows that the estimated impact of life satisfaction on political trust is much more substantive for males than females. A possible explanation is that the Chinese culture has a traditional feature of "men outside, women inside" so that men should take more responsibility when it comes to any actions involving the local government (Du et al. 2021). Under such a social structure, men may have a stronger sense that the government influences their well-being, which we suggest is a possible explanation for this gender difference.

In addition, the effect of life satisfaction on political trust is significantly more substantial for the urban as opposed to the rural subgroup. Clan culture, which still prevails in rural China, could be one reason for this difference. Clan culture encourages individuals to rely more on genealogical networks rather than the government (Cao et al. 2022). It is thus possible that rural residents who are exposed more heavily to clan

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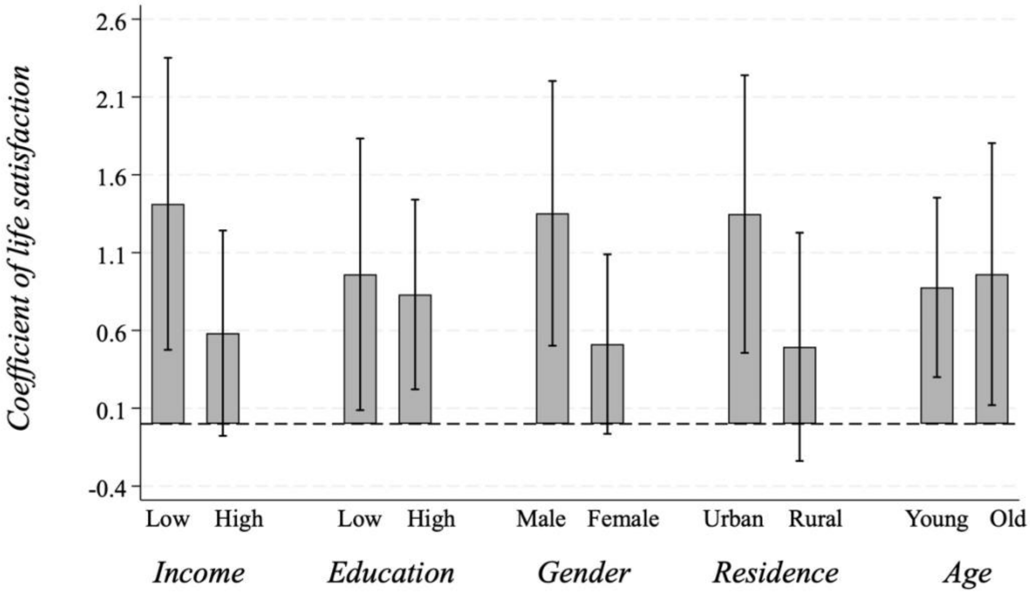
[23] In terms of age and family per capita income, we split the sample at the median point. In terms of education level, those who achieved high school or higher education are defined as the educated, otherwise less educated. As some of those individual characteristics (e.g. income) will change across waves, some observations only appear once in the subgroups and will be automatically dropped in the FE-IV estimation.

culture could be comparatively less likely to hold the government accountable for their own well-being.

Finally, there is no evidence to suggest that the relationship between life satisfaction and political trust is significantly different between the old and the young or the educated and the less educated.

Figure 2.2 Effect of life satisfaction on political trust across subgroups



Note. The results are estimated by FE-IV models with full controls, based on the IV set of physical attractiveness (PA) and physical discomfort (PD). Control variables include age (squared), marital status, education level, residence, Hukou, CPC membership, occupation status, family per capita income (log-linearised), household size and evaluation of medical services. All control variables are described in Appendices Table A2.1. Detailed results can be found in Appendices Table A2.3. Whiskers denote 90% confidence intervals, which are calculated based on standard errors clustered at individual and county levels.

2.6 Robustness checks and further analyses

2.6.1 Political trust or generalised trust?

One may be concerned that our estimates capture a pure trust effect of happiness rather than an effect specific to trust in the government. Fortunately, CFPS asks respondents about their general trust level, which enables us to control for general trust in our analysis. Specifically, respondents are asked to answer the following question: "*In general, do you think that most people are trustworthy, or it is better to take greater caution when getting along with other people?*", with 1 indicating high general trust and 0 low general trust. This or similar measures have been used widely to capture general trust in the political science literature (Sturgis and Smith 2010). The results in column 1 of Appendices Table A2.9 show that the coefficient of life satisfaction remains statistically significant and of a similar size as in our main results, if general trust is controlled for.[24] This would suggest that our results reveal an impact of happiness on political trust that goes beyond any effect of happiness on other dimensions of trust.

2.6.2 Sensitivity to alternative well-being measure

As another sensitivity check, we also test if we can replicate our main findings in Table 2.1 using an alternative measure of subjective well-being, namely self-reported happiness, which is available in the 2014 and 2018 surveys of the CFPS. In Appendices Table A2.10, we can see that happiness attracts a positive and statistically significant coefficient in our FE model. Next, after employing PA and PD as instrumental variables,[25] we can see in Column 1 to 3 of Appendices Table A2.11 that happiness again attracts a coefficient that is considerably larger than the FE estimate. This again suggests that subjective well-being is important in the formation of political trust, and looking at the direct association between these variables will significantly understate the importance of subjective well-being for the formation of political trust.

[24] For parsimony, we only report the results when using PA and PD as the IV set in the FE-IV estimation as this is the combination which maximises sample size. Our results are qualitatively the same if we use all our IV's individually or combined.

[25] Here we only consider physical attractiveness (PA) and physical discomfort (PD) because the data of parent-child relation is not available in the 2014 survey of the CFPS and happiness in contrast to life satisfaction is only available in 2 (2014 and 2018) as opposed to 4 waves. In Column 2 of Table A2.9, one may note that PD is not as strong an IV for happiness as life satisfaction from the first stage regressions (F-statistic < 10). We thus also run regressions resting on PA and PD separately, from which we can obtain quite similar estimates (see Column 3 and 4 of Table A2.9). The key point here is that we observe the same pattern as reported earlier when using life satisfaction (see Table 2.1). Specifically, happiness is significantly correlated with political trust but that the FE analysis underestimates this effect.

2.6.3 Beyond trust: political trust and regime stability

An important assumption for establishing the mechanism of political accountability in our theoretical framework is that political trust matters. That is, political trust is a mechanism through which citizens can express their dissatisfaction with government and ultimately incentivize their government officials to serve the public interest. There is already a large volume of literature underpinning this assumption (see Section 2.2). Here, we shed light on this issue within our own research setting. A challenge is the lack of data on Chinese people's political behaviours or activities affecting government effectiveness (e.g. tax evasion). Fortunately, a question in the 2014 wave of the CFPS gives us a chance to explore the role that political trust plays in predicting how people will respond to government actions that are detrimental to their own interest, essentially a measure of the readiness to protest against government actions. Building a causal link is difficult given the cross-sectional data, but the association between political trust and this measure will, we argue, still be informative.

The question (from CFPS 2014) that we take advantage of is: "*If you are treated unfairly by the government, such as forced house demolition, land expropriation, compulsory taxation, and unjustified fine, what actions are you likely to take?*". Respondents are asked to answer with the following options (multiple choices): "*1. Appeal to upper-level officials, 2. Appeal to other government departments, 3. Appeal to courts, 4. Express your views through the internet, media, and social groups, 5. Sign a petition/march/sit-in/protest, 6. Bear it without doing anything, 7. Other actions (self-reported)*".[26]

We split these options into two categories depending on the degree to which such actions will undermine regime stability. In general, options 1 to 3 will not hurt the regime as they are actions seeking solutions within the political system. Options 4 and 5, however, reflect people's discontent with the political system and can be seen as actions that could damage regime stability. In terms of option 6, although it largely reflects the fact that people do not believe that they can find a good solution within the political system, it is unlikely to harm the regime directly, at least in the short-term. We

[26] Note that (i) option 6 is exclusive to other options; (ii) the damaging impact of action 4 is the same as option 5, if not greater. This is because social media can circulate such information quickly and widely, which is detrimental to government image. Although there is social media censorship in China, some news negative to the government can still circulate broadly; (iii) in terms of option 7, CFPS 2014 does not provide details of the other actions and it only constitutes around 1.5% of the total answers, we thus do not consider this option in this paper, i.e., samples with this option are removed.

thus put it alongside options 1 to 3, but confirm our results do not change substantively if simply excluding respondents who choose this option. We label the resulting dummy variable as *Upheaval* (1= actions that can damage regime stability and 0 otherwise). In Appendices Table A2.1, we can see that 19% of respondents would choose actions that might be perceived as having the potential to damage regime stability.

Given the binary nature of the dependent variable, we employ a probit model to explore the relationship between political trust and *Upheaval*.[27] The results are presented in Appendices Table A2.12, where we report coefficients and the marginal effects (calculated at means of political trust as well as all covariates). In column 1, we can see that political trust has a significant and negative marginal estimated effect ($\beta = -0.018$, $p = 0.000$). In column 2, a full set of control variables[28] are added into the model, and we can again see that political trust still shows a negative association with *Upheaval* at the 1% significance level, although one that is smaller in size ($\beta = -0.009$, $p = 0.000$). Numerically, these results suggest that a reduction of one SD in political trust (here 2.638 points) is associated with a 2.4 (2.638×0.9) percentage points increase in the probability that a person chooses actions which would damage regime stability. This is equivalent to 12.6% (2.4÷19) of the average probability (19%) that individuals in our sample would choose actions that could damage regime stability (*Upheaval*). As a further comparison, we find in the same model that doubling one's family per capita income will reduce the estimated probability of *Upheaval* by 0.6 percentage points ($\beta = -0.006$, $p = 0.002$). This means that the effect of a one SD increase in political trust is equivalent to a 400% increase in family per capita income when it comes to predicting *Upheaval*. This supports our suggestion that political trust is indeed highly important for regime stability.

2.7 Conclusion

Recently researchers have begun to assess whether there is an electoral incentive to consider subjective well-being as a policy goal (so called happy voters). While endogeneity concerns remain a challenge, this literature suggests that subjective well-being could be important for predicting electoral outcomes (Liberini et al. 2017; Ward 2020). Using the example of China, the question we asked is whether there is an

[27] We also use a linear probability model (OLS) to estimate this effect, the results are also presented in Table A2.12 and closely resemble the estimates obtained when using the probit model.
[28] The model contains the same control variables as in column 2 of Table 2.1. Besides, gender, a province-fixed effect and a county fixed effect are added.

incentive for authoritarian rulers to maximise the happiness of their citizens? We approached this question through the prism of social contract theory and political trust. Our idea here being that there is an intangible contract consisting of a set of mutual obligations between the ruling authority (e.g. governments) and its citizens. On the one hand, the ruling authority will commit to the welfare of the general public, and when this occurs, citizens will commit to supporting (trusting) the ruling authority by consenting to their authority.

In support of this premise, using an individual fixed-effects analysis, we first documented a positive association between individuals' self-reported life satisfaction and their political trust. We addressed endogeneity concerns through the use of an instrumental variables analysis, with three different instruments, namely physical attractiveness, recently experienced physical discomfort and closeness of familial relationships. The consistency of estimates obtained using these instruments separately and in various combinations strongly suggests that our results are credible. Our findings indicate that life satisfaction directly impacts the formation of citizens' trust in Chinese government. To the best of our knowledge, we present the first causal estimates of the impact of subjective well-being on the formation of political trust.

Our research also adds new insights to the extensive literature exploring the origins of political trust. Previous studies have highlighted the importance of history, culture and economic development (Chen et al. 2020; Hetherington and Rudolph 2008; Nunn and Wantchekon 2011). Here we show that individuals' own perceived well-being is a significant driver. Our findings also add to a nascent literature that regards subjective measures of well-being as a factor explaining rather than resulting from certain phenomena. Prior studies have, for instance, demonstrated life satisfaction is a predictor of various individual behaviours such as productivity in the workplace and the decision to get divorced (Clark et al. 2008; Oswald et al. 2015). Our study reveals that although trust might play a significant role in happiness, the converse is equally valid. Overlooking the bi-directional nature of this connection may have biased previous evaluations of the link between political trust and life satisfaction, especially given how strong is the relationship between life satisfaction and political trust.

For a long time, the legitimacy of the Chinese government has been believed to rest on the remarkable economic performance of the country. The strong association we measure between political trust and individual's subjective well-being implies that this legitimacy is also built on aspects that go beyond individual income. We note how the

Chinese government has started to officially recognize the importance of national happiness: The 20th National Congress of the Communist Party of China (CPC) conspicuously added a sentence to the party constitution stating that "*the Communist Party of China has remained true to its original aspiration and founding mission of seeking happiness for the Chinese"* (Huaxia 2022). While one might naively perceive this as an act of pure benevolence, our results imply that focusing on population happiness is key to maintaining political trust and thus regime stability.

Appendices

Table A2.1 Descriptive statistics

VARIABLES	Observations	Mean	SD	Min	Max
Panel A. Key variables					
Political trust (PT)	77,891	5.045	2.637	0	10
Life satisfaction (LS)	77,891	3.723	1.056	1	5
Physical attractiveness (PA)	77,891	5.388	1.234	1	7
Physical discomfort (PD)	77,891	0.317	/	0	1
Parent-child relationship (PCR)	19,083	4.215	0.656	1	5
Panel B. Control variables in main setting					
Age	77,891	49.14	15.99	16	98
Female (female=1)	77,891	0.523	/	0	1
Per Capital Family income (10k/Yuan)	77,891	1.404	20136	0	148
CPC member	77,891	0.086	/	0	1
Residence (urban=1)	77,891	0.468	/	0	1
Hukou (rural=1)	77,891	0.717	/	0	1
Family size	77,891	4.312	1.983	1	21
Marital status					
Single	77,891	0.086	/	0	1
In marriage	77,891	0.835	/	0	1
Divorced	77,891	0.015	/	0	1
Widowed	77,891	0.064	/	0	1
Education level					
Illiteracy	77,891	0.303	/	0	1
Primary school	77,891	0.216	/	0	1
Middle school	77,891	0.273	/	0	1
High school	77,891	0.136	/	0	1
Occupational college	77,891	0.044	/	0	1
University	77,891	0.028	/	0	1
Occupation status					
Student	77,891	0.037	/	0	1
Manager or business owner	77,891	0.031	/	0	1
Professional personnel	77,891	0.036	/	0	1
Staff in public sector	77,891	0.030	/	0	1
Staff in service sector	77,891	0.087	/	0	1
Agricultural practitioner	77,891	0.368	/	0	1
Worker in manufacturing sector	77,891	0.122	/	0	1
Unclear identity	77,891	0.013	/	0	1
Unemployment	77,891	0.010	/	0	1
Exit labour market	77,891	0.267	/	0	1
Evaluation of medical services	77,891	2.257	0.869	0	4
Panel C. Variables used in robustness check					
General trust	77,690	0.547	/	0	1
Happiness	28,822	7.565	2.086	1	10
Education level of the spouse					
Illiteracy	63,006	0.275	/	0	1
Primary school	63,006	0.233	/	0	1
Middle school	63,006	0.300	/	0	1
High school	63,006	0.128	/	0	1
Occupational college	63,006	0.040	/	0	1
University	63,006	0.024	/	0	1
Upheaval	28,758	0.191	/	0	1

Table A2.2 The impact of life satisfaction on political trust: OLS estimation

	(1) FE	(2) FE	(3) RE
Life Satisfaction	0.240***	0.229***	0.341***
	(0.013)	(0.013)	(0.013)
Controls	No	Yes	Yes
Wave FE	No	Yes	Yes
Individual FE	Yes	Yes	Yes
Observations	77,891	77,891	77,891
Number of persons	26,107	26,107	26,107

Note. Control variables include age (squared), marital status, education level, residence, Hukou, CPC membership, occupation status, family per capita income (log-linearised), household size. All control variables are described in Table A2.1. Robust standard errors clustered at individual and county levels in parentheses. *** p<0.01, ** p<0.05, *p<0.1.

Table A2.3 Heterogeneity analysis

	(1) Low income	(2) High income	(3) Less educated	(4) Educated	(5) Male	(6) Female
Panel A. First stage	Dependent variable: Life Satisfaction					
Physical attractiveness	0.024***	0.028***	0.028***	0.025***	0.023***	0.032***
	(0.009)	(0.007)	(0.008)	(0.006)	(0.007)	(0.007)
Physical Discomfort	-0.064***	-0.064***	-0.038**	-0.082***	-0.055***	-0.055***
	(0.020)	(0.013)	(0.017)	(0.014)	(0.016)	(0.015)
F-statistic	7.12	18.28	7.60	25.24	10.55	16.55
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,887	32,663	40,146	36,852	37,135	40,756
Number of persons	12,659	12,366	13,409	12,458	12,490	13,617
Panel B. Second stage	Dependent variable: Political Trust					
Life Satisfaction	1.413**	0.582	0.962*	0.831**	1.352***	0.512
	(0.567)	(0.400)	(0.528)	(0.369)	(0.514)	(0.349)
Anderson-Rubin Wald F-statistic	4.15	1.16	2.03	3.38	4.23	1.33
(p-value)	(0.018)	(0.316)	(0.135)	(0.036)	(0.016)	(0.267)
Hansen J statistic	0.472	0.084	0.384	1.078	0.056	0.172
(p-value)	(0.492)	(0.771)	(0.535)	(0.299)	(0.813)	(0.679)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,887	32,663	40,146	36,852	37,135	40,756
Number of persons	12,659	12,366	13,409	12,458	12,490	13,617

	(7) Rural area	(8) Urban area	(9) The old	(10) The young
Panel A. First stage	Dependent variable: Life Satisfaction			
Physical attractiveness	0.034***	0.020***	0.028***	0.027***
	(0.009)	(0.006)	(0.007)	(0.007)
Physical Discomfort	-0.053***	-0.061***	-0.039**	-0.079***
	(0.017)	(0.014)	(0.015)	(0.017)
F-statistic	9.37	14.46	9.44	18.61
Controls	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	40,510	35,534	41,150	36,741
Number of persons	13,712	12,184	13,338	12,769
Panel B. Second stage	Dependent variable: Political Trust			
LS	0.494	1.348**	0.962*	0.876**
	(0.443)	(0.540)	(0.509)	(0.349)
Anderson-Rubin Wald F-statistic	1.17	3.67	2.20	3.76
(p-value)	(0. 312)	(0.027)	(0.114)	(0.025)
Hansen J statistic	0.563	0.421	0.220	0.494
(p-value)	(0.453)	(0.516)	(0.639)	(0.482)
Controls	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	40,510	35,534	41,150	36,741
Number of persons	13,712	12,184	13,338	12,769

Note. The results are estimated by FE-IV models with full controls, based on the IV set of physical attractiveness (PA) and physical discomfort (PD). Control variables include age (squared), marital status, education level, residence, Hukou, CPC membership, occupation status, family per capita income (log-linearised), household size and evaluation of medical services. All control variables are described in Table A2.1. Robust standard errors clustered at individual and county levels in parentheses. *** p<0.01, ** p<0.05, *p<0.1.

Table A2.4 Robustness check: Including sample with cross-counties migrations

	(1)
Panel A. First stage	
	Dependent variable: Life Satisfaction
Physical attractiveness	0.026***
	(0.006)
Physical Discomfort	-0.056***
	(0.011)
F-statistic	21.75
Controls	Yes
Wave FE	Yes
Individual FE	Yes
Observations	86,575
Number of persons	29,346
Panel B. Second stage	
	Dependent variable: Political Trust
Life Satisfaction	0.721**
	(0.326)
Anderson-Rubin Wald F-statistic	2.61
(p-value)	(0.075)
Hansen J statistic	0.003
(p-value)	(0.954)
Controls	Yes
Wave FE	Yes
Individual FE	Yes
Observations	86,575
Number of persons	29,346

Note. The results are estimated by FE-IV models with full controls, based on the IV set of physical attractiveness (PA) and physical discomfort (PD). Control variables include age (squared), marital status, education level, residence, Hukou, CPC membership, occupation status, family per capita income (log-linearised), household size and evaluation of medical services. All control variables are described in Table A2.1. Robust standard errors clustered at individual and county levels in parentheses. *** $p<0.01$, ** $p<0.05$, *$p<0.1$.

Supplementary analyses A: Self-censorship in the measure of political trust?

In addition to previous analyses by Yao et al. (2022) we check if our results are sensitive to issues of self-censorship through two tests. First, we simply exclude the sample who always report a high score (≥8) of political trust across all waves of the CFPS survey.[1] The results in column 1 of below Table A2.5 show that the coefficient of life satisfaction is very similar to our main results and remains statistically significant. Our second approach is to consider which group of the sample is most likely to give a politically expedient answer. An intuitive candidate here is whether respondents are a member of the ruling party (CPC member). Reporting a low level of trust might have an adverse impact on one's career in the party as it could be seen as an act of political betrayal (Han and Li 2021). Hence we conduct a robustness check by excluding all CPC members from our sample. In column 2 of Table A2.5, we can see that the coefficient of life satisfaction is still very close to our main results and remains statistically significant. This would again suggest that our findings are unlikely to be biased by self-censorship when it comes to how individuals report their political trust.

[1] Note that our finding does not rely on the choice of the level '8', as we can get similar results when using '7', '9' or '10' as the sample selection standard.

Table A2.5 Robustness check: self-censorship in the measure of political trust?

	(1) Exclude extreme answers	(2) Exclude CPC member
Panel A. First stage		
	Dependent variable: Life Satisfaction	
Physical attractiveness	0.028***	0.026***
	(0.006)	(0.006)
Physical Discomfort	-0.059***	-0.055***
	(0.012)	(0.012)
F-statistic	20.09	17.69
Controls	Yes	Yes
Wave FE	Yes	Yes
Individual FE	Yes	Yes
Observations	75,478	69,899
Number of persons	25,241	23,489
Panel B. Second stage		
	Dependent variable: Political Trust	
Life Satisfaction	0.836**	0.828*
	(0.337)	(0.478)
Anderson-Rubin Wald F-statistic	3.55	2.74
(p-value)	(0.031)	(0.067)
Hansen J statistic	0.027	0.019
(p-value)	(0.869)	(0.891)
Controls	Yes	Yes
Wave FE	Yes	Yes
Individual FE	Yes	Yes
Observations	75,478	69,899
Number of persons	25,241	23,489

Note. The results are estimated by FE-IV models with full controls, based on the IV set of physical attractiveness (PA) and physical discomfort (PD). Control variables include age (squared), marital status, education level, residence, Hukou, CPC membership, occupation status, family per capita income (log-linearised), household size and evaluation of medical services. All control variables are described in Table A2.1. Robust standard errors clustered at individual and county levels in parentheses. *** p<0.01, ** p<0.05, *p<0.1.

Table A2.6 Robustness check: Whether attrition causes selection bias

	(1)
Panel A. First stage	
	Dependent variable: Life Satisfaction
Physical attractiveness	0.024***
	(0.006)
Physical Discomfort	-0.056***
	(0.016)
Attrition indicator	0.002
	(0.046)
F-statistic	13.36
Controls	Yes
Wave FE	Yes
Individual FE	Yes
Observations	49,165
Number of persons	19,076
Panel B. Second stage	
	Dependent variable: Political Trust
Life Satisfaction	1.113**
	(0.483)
Attrition indicator	-0.021
	(0.015)
Anderson-Rubin Wald F-statistic	2.61
(p-value)	(0.077)
Hansen J statistic	0.021
(p-value)	(0.884)
Controls	Yes
Wave FE	Yes
Individual FE	Yes
Observations	49,165
Number of persons	19,076

Note. The results are estimated by FE-IV models with full controls, based on the IV set of physical attractiveness (PA) and physical discomfort (PD). Control variables include age (squared), marital status, education level, residence, Hukou, CPC membership, occupation status, family per capita income (log-linearised), household size and evaluation of medical services. All control variables are described in Table A2.1. Robust standard errors clustered at individual and county levels in parentheses. *** p<0.01, ** p<0.05, *p<0.1.

Table A2.7 Robustness check: Alternative choices of standard errors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Heteroscedasticity robust	Different clustering options					
	County & individual		County	CFPS interviewer	Individual	Family	County & Family	County & CFPS interviewer
Panel A. First stage								
	Dependent variable: Life Satisfaction							
Physical attractiveness	0.028***	0.028***	0.028***	0.028***	0.028***	0.028***	0.028***	0.029***
	(0.006)	(0.004)	(0.006)	(0.006)	(0.004)	(0.005)	(0.006)	(0.006)
Physical Discomfort	-0.055***	-0.055***	-0.055***	-0.055***	-0.055***	-0.055***	-0.055***	-0.055***
	(0.012)	(0.010)	(0.012)	(0.011)	(0.010)	(0.012)	(0.012)	(0.012)
F-statistic	18.90	43.80	18.90	21.26	44.96	27.88	18.90	17.94
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	77,891	77,891	77,891	77,891	77,891	77,891	77,891	77,891
Number of persons	26,107	26,107	26,107	26,107	26,107	26,107	26,107	26,107
Panel B. Second stage								
	Dependent variable: Political Trust							
Life Satisfaction	0.875**	0.875***	0.875**	0.875**	0.875***	0.875**	0.875**	0.875**
	(0.347)	(0.272)	(0.347)	(0.343)	(0.269)	(0.343)	(0.346)	(0.347)
Anderson-Rubin Wald F-statistic	3.72	5.55	3.72	3.89	5.67	3.47	3.72	3.76
(p-value)	(0.026)	(0.004)	(0.026)	(0.021)	(0.004)	(0.031)	(0.026)	(0.025)
Hansen J statistic	0.020	0.047	0.020	0.032	0.032	0.030	0.020	0.019
(p-value)	(0.888)	(0.829)	(0.888)	(0.858)	(0.859)	(0.863)	(0.888)	(0.889)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	77,891	77,891	77,891	77,891	77,891	77,891	77,891	77,891
Number of persons	26,107	26,107	26,107	26,107	26,107	26,107	26,107	26,107

Note. The results are estimated by FE-IV models with full controls, based on the IV set of physical attractiveness (PA) and physical discomfort (PD). Control variables include age (squared), marital status, education level, residence, Hukou, CPC membership, occupation status, family per capita income (log-linearised), household size and evaluation of medical services. All control variables are described in Table A2.1. Robust standard errors clustered at individual and county levels in parentheses. *** p<0.01, ** p<0.05, *p<0.1.

Supplementary analyses B: Does the "beauty premium" influence our estimation?

Apart from economic returns, advantages in the marriage market are a frequently mentioned premium of physical attractiveness (Egebark et al. 2021). Although we have controlled for marital status in the model, this might not cover the 'quality' of the spouse as result of the beauty premium.[1] Using spousal education attainment as a proxy, we control for the quality of spouse in our model.[2] The CFPS records the spouse's education level for around 80% of respondents and the results for this sub-group are presented in column 1 of below Table A2.8. Life satisfaction still has a significant effect on political trust. The coefficient size ($\beta = 0.980$, $p = 0.059$) is qualitatively indistinguishable from our previous FE-IV estimates (Table 2.1 above).

Although we have controlled for occupation type and family per capita income, one could argue that these variables may not be precise enough to reflect personal success or failure in the workplace resulting from differences in PA. There might, for instance, be non-monetary discrimination when it comes to work characteristics that are associated with PA. Whilst very unlikely, one could perhaps argue that such discrimination in the labour market may lead to general dissatisfaction with the societal system including the local government and hence deteriorate trust. We address this question by limiting our sample to those who are least likely to be affected by a 'beauty premium' in job characteristics. Specifically, we focus on three types of respondents: students, farmers and people that are out of the labor force (typically retirees).[3] The results presented in column 2 of Table A2.8 are fully in keeping with our main findings discussed before (see Table 2.1). We conclude that PA does not appear to violate the exclusion condition.

[1] We believe that the quality of a spouse is largely stable across years and is hence covered by the individual fixed effect. However, one may divorce and marry again, leading to variation in spousal quality within the same person.

[2] This is based on the fact that intelligence is a key factor in mate selection (Fisman et al. 2006; Hitsch et al. 2010).

[3] In our setting, we only focus on the samples who did not change their occupational status across waves during our sampling period. This means that, for example, a farmer is always a farmer when it is observed in our setting. This is also true for students and non-employed individuals. As farmers and retirees constitute the largest part of our sample, we can still obtain a large number of observations for this test. What is more, those who are out of the labour force in the CFPS are mainly retired (over 90%), others are, for instance, disabled.

Table A2.8 Robustness check: Examination on "beauty premium"

	(1)	(2)
	Spouse quality	Limited sample
Panel A. First stage		
	Dependent variable: Life Satisfaction	
Physical attractiveness	0.028***	0.029***
	(0.007)	(0.008)
F-statistic	17.89	15.26
Controls	Yes	Yes
Wave FE	Yes	Yes
Individual FE	Yes	Yes
Observations	63,006	49,267
Number of persons	20,978	17,041
Panel B. Second stage		
	Dependent variable: Political Trust	
Life Satisfaction	0.980*	0.942*
	(0.515)	(0.566)
Anderson-Rubin Wald F-statistic	4.53	3.33
(p-value)	(0.035)	(0.070)
Controls	Yes	Yes
Wave FE	Yes	Yes
Individual FE	Yes	Yes
Observations	63,006	49,267
Number of persons	20,978	17,041

Note. The results are estimated by FE-IV models with full controls, based on the IV of physical attractiveness (PA). Individual control variables include age (squared), marital status, education level, residence, Hukou, CPC membership, occupation status, family per capita income (log-linearised) and household size. All control variables are described in Table A2.1. Robust standard errors clustered at individual and county levels in parentheses. *** p<0.01, ** p<0.05, *p<0.1.

Table A2.9 Robustness check: Political trust or general trust?

	(1) Controlled for general trust
Panel A. First stage	
	Dependent variable: Life Satisfaction
Physical attractiveness	0.027***
	(0.006)
Physical Discomfort	-0.054***
	(0.011)
F-statistic	17.93
Controls	Yes
Wave FE	Yes
Individual FE	Yes
Observations	77,631
Number of persons	26,048
Panel B. Second stage	
	Dependent variable: Political Trust
Life Satisfaction	0.805**
	(0.355)
Anderson-Rubin Wald F-statistic	2.91
(p-value)	(0.057)
Hansen J statistic	0.000
(p-value)	(0.984)
Controls	Yes
Wave FE	Yes
Individual FE	Yes
Observations	77,631
Number of persons	26,048

Note. The results are estimated by FE-IV models with full controls, based on the IV set of physical attractiveness (PA) and physical discomfort (PD). Control variables include age (squared), marital status, education level, residence, Hukou, CPC membership, occupation status, family per capita income (log-linearised), household size and evaluation of medical services. All control variables are described in Table A2.1. Robust standard errors clustered at individual and county levels in parentheses. *** p<0.01, ** p<0.05, *p<0.1.

Table A2.10 Alternative measure of subjective well-being: OLS estimation

	(1) FE
Life Satisfaction	0.198***
	(0.012)
Controls	Yes
Wave FE	Yes
Individual FE	Yes
Observations	28,822
Number of persons	14,411

Note. Control variables include age (squared), marital status, education level, residence, Hukou, CPC membership, occupation status, family per capita income (log-linearised), household size. All control variables are described in Table A2.1. Robust standard errors clustered at individual and county levels in parentheses. *** p<0.01, ** p<0.05, *p<0.1.

Table A2.11 Alternative measure of subjective well-being: IV estimation

	(1)	(2)	(3)
Panel A. First stage			
	Dependent variable: Life Satisfaction		
Physical attractiveness	0.058***		0.057***
	(0.018)		(0.018)
Physical Discomfort		-0.051	-0.052
		(0.037)	(0.037)
F-statistic	10.03	1.96	6.03
Controls	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Observations	28,822	28,822	28,822
Number of persons	14,411	14,411	14,411
Panel B. Second stage			
	Dependent variable: Political trust		
Life Satisfaction	0.717*	0.866	0.710*
	(0.385)	(0.957)	(0.369)
Anderson-Rubin Wald F-statistic	3.04	0.91	1.84
(p-value)	(0.083)	(0.342)	(0.162)
Hansen J statistic			0.032
(p-value)			(0.857)
Controls	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Observations	28,822	28,822	28,822
Number of persons	14,411	14,411	14,411

Table A2.12 Political trust and regime stability

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Upheaval					
	Probit estimation				OLS estimation	
	Coefficients		Marginal effects			
Political trust	-0.065***	-0.041***	-0.018***	-0.009***	-0.018***	-0.009***
	(0.005)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Controls	No	Yes	No	Yes	No	Yes
Province FE	No	Yes	No	Yes	No	Yes
County FE	No	Yes	No	Yes	No	Yes
(Pseudo) R-squared	0.014	0.208	-	-	0.014	0.208
Number of persons	28,298	28,298	28,298	28,298	28,298	28,298

Note. The marginal effects of the probit model are calculated at the means of political trust as well as all covariates. Individual control variables include age (squared), marital status, education level, residence, Hukou, CPC membership, occupation status, family per capita income (log-linearised) and household size. In addition, the gender, province-fixed effect and county-fixed effect are also controlled for. All control variables are described in Table A2.1. Robust standard errors clustered at county levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Chapter 3 Kill Me if You Can: Early Life Adversity and Resilience

Abstract

We show that individuals exposed to early life adversity are more resilient to later life shocks. We do this by linking two exogenous events at the individual level, namely exposure to extreme starvation in childhood as a result of the China Great Famine and the Covid-19 pandemic. We identify causal effects by employing a difference-in-difference strategy where we compare changes in mental well-being before and during the Covid-19 pandemic between those with early life starvation experience and those without. Our results are robust to different measures of psychological well-being and measures of Covid-19 pandemic severity as well as cohort identification strategies. We find this positive resilience-enhancing effect is attenuated by more extreme adversity. These findings are generally in line with the 'steeling effect' theory, which suggests that whatever does not kill you may fortify you.

"*The world breaks everyone, and afterward, some are strong at the broken places.*"

- Ernest Hemingway, *A Farewell to Arms*, 1929

3.1 Introduction

Life often exposes us to hardships, ranging from minor ones like failing an exam to major ones such as facing wars. Confronted with adversities, some people succumb, while others recover swiftly. One explanation for such disparity is that individuals have different levels of resilience, which is commonly defined as one's ability of adapting to adverse environments or recovering from setbacks (Bonanno 2004; Rutter 2012). The question arises as to where individual resilience originates? An argument is that it is something innate to us and that some people do not suffer greatly in terms of psychological distress when exposed to extreme adversity because they are born with a certain type of personality (often referred to as a hardy personality) that protects them (Kalisch and Kampa 2021). Factors beyond the individual such as social support and cultural influences have also been put forward as key to resilience (McCubbin et al. 1998).

In this paper, we examine whether early life adversity affects the formation of individual resilience. Can exposure to stressful events itself foster resilience? On the one hand, many studies have shown that early life adversity often generates long-lasting negative effects on one's mental well-being (e.g. Cui et al. 2020; Gilbert et al. 2009) as well as educational attainment and income (e.g. Kesternich et al. 2014). Given this, it seems intuitive to suggest that if early life adversity has a role to play in shaping resilience, it would be a damaging one.

While experiencing an adverse event is typically associated with poorer future life outcomes, emanating from psychology there is some theoretical work known as the 'steeling effect theory' proposing that moderate levels of adversity may be beneficial for the formation of resilience (e.g. Dienstbier 1989; Seery et al. 2010). This is in keeping with the popular expression '*whatever doesn't kill you, makes you stronger*'. This perspective draws parallels with the notion that exposure to infectious agents, as in vaccination, can enhance resistance to infections. Similarly, early exposure to adversity might foster one's capacity of coping with future challenges.

Whether early life adversity fosters or weakens resilience is still therefore an open empirical question. When it comes to answering this question, it is crucial to remember that a key component of resilience pertains to the aspect of facing challenges: an individual's resilience cannot be effectively assessed without being exposed to hardship

(Masten 2001). An ideal examination of the relationship between early life adversity and resilience thus requires a second shock of adversity. That is, the best way to examine whether early life adversity has affected one's resilience is to expose the person to another setback. This is a key novel feature of the approach we follow in this study. Specifically, we connect two exogenous events at the individual level, namely exposure to starvation during the China's Great Famine (CGF, 1958-1962, Zhou 2012) and the Covid-19 pandemic, and assess how exposure to the former affects individuals' ability of coping with the latter.

The Covid-19 pandemic presents a valuable opportunity for investigating resilience, i.e. providing us with our second shock of adversity. Firstly, it constitutes an unexpected shock affecting all individuals. This alleviates potential endogenous concerns that might arise when utilizing disruptive events at the individual level, as they might be correlated with one's experiences of adversity in early life. More importantly, the pandemic has posed substantial challenges for the general public (Anaya et al. 2023; Brodeur et al. 2021), particularly when it comes to individuals' mental well-being (e.g. Gao et al. 2020; Song 2020; Wang et al. 2020). We thus can identify variation in the levels of resilience among different people by looking at the changes in their mental well-being before and during the crisis.[1] Put simply, all things being equal, a greater (smaller) loss of mental well-being in response to the pandemic should mean a weaker (stronger) resilience. Linking this to individuals' experience of early life adversity thus can plausibly test whether early life adversity affects the development of resilience.

As our measure of early life adversity, we use individuals experience with starvation episodes. We obtain this data from the China Family Panel Studies (CFPS), which in 2010 asked respondents to report any extreme starvation episode (ESE, persistent hunger for at least one week) in the past. Most respondents who report an experience of extreme starvation trace it back to a period coincident with the most devastating years of the China's Great Famine. We leverage this famine related ESEs in our study. The advantage of relying on this event as a marker for early life adversity lies in the fact that the famine was caused by a combination of natural calamities and

[1] It is increasingly common to measure individual resilience by tracking mental well-being changes following a major life event (e.g. Bonanno 2004; Bonanno et al. 2006; Etilé et al. 2021; Orcutt et al. 2014). Similar to us, Johnston et al. (2021) also use the Covid-19 pandemic as a major event to test individual resilience. The validity of this approach is verified by many studies that use more direct measures, e.g. the resilience scales. In particular, studies have found a significant correlation between scores on resilience scales and mental well-being during the Covid-19 pandemic (Ran et al. 2020; Killgore et al. 2020).

policy failures (Kueh 1995; Lin 1990; Meng et al. 2015), which were exogenous to individuals. Given that the famine occurred about 60 years ago, one can reasonably assume that the factors driving individuals' starvation experience will not be confounded with how they cope with the Covid-19 pandemic.

The CFPS is a nationally representative longitudinal survey, which has been conducted every two years since 2010 with the last wave of the survey released in the second half of 2020, after the outbreak of the Covid-19 pandemic in China. This enables us to employ a difference-in-difference (DID) strategy to identify the causal effect of childhood ESE on individual resilience. We do this by comparing the changes in mental well-being (with life satisfaction as the primary measure) before and during the Covid-19 pandemic between individuals with and without experience with an ESE in their childhood. In effect, we are assessing whether individuals exposed to early life adversity, in this case extreme starvation, coped better/worse in terms of their mental well-being during the Covid-19 pandemic. The key variable of interest in our analysis is therefore an interaction term between childhood experience of an ESE and Covid-19 pandemic severity.

Our baseline analysis focuses on the 2018 and 2020 waves of the CFPS. This means we can use the four previous waves of data between 2010 and 2016 to test the identifying assumption of parallel trends. Different from a traditional DID strategy, we also need to assume that the factors affecting the assignment of an ESE (whether a person has experienced extreme starvation in childhood) do not substantially influence how one copes with the pandemic (resilience). This assumption sounds reasonable given that the starvation experience happened about 60 years prior to the survey and the main culprit namely the great famine was largely an exogenous shock to individuals. To further bolster this assumption, we also control for some individual background characteristics (interacting with the Covid-19 pandemic severity) as well as province trends in our baseline model.

Our results show that the interaction term of childhood experience with an ESE and Covid-19 pandemic severity attracts a positive coefficient, indicating that experience of an ESE has attenuated the negative impact of the Covid-19 pandemic on individuals' life satisfaction. This suggests that early life adversity, on average, has strengthened individuals' resilience. This finding is robust to a battery of sensitivity checks including using alternative measures of both mental well-being and Covid-19 pandemic severity. We further rule out the possibility that unobservable factors could be both influencing whether individuals were exposed to an ESE and their future well-being. We do this by testing the sensitivity of our estimates to the use of a propensity score matching approach before the DID regression, randomly assigning ESEs among our samples and evaluating the risk of omitted variable bias with the Oster (2019) approach.

Furthermore, instead of relying on self-reported ESEs to construct the treatment of early life adversity, we also directly compare the cohort born immediately before the famine with the cohort born immediately after the famine. The assumption we make here is that those born before the famine will be more likely to have been exposed to a starvation episode, than those born after. Measurement error is more likely as many individuals born before the famine period will not have experienced an ESE, but such an approach has the advantage of avoiding any endogeneity issues surrounding the assignment of ESEs. Both cohorts should also be broadly comparable in that birth dates do not differ greatly. This test yields similar estimates to that observed in our baseline analysis and so again supports our finding that, at the population level at least, those exposed to early starvation episodes suffered less in mental well-being during the Covid-19 pandemic. Our proposed explanation for these findings is that this is due to greater resilience brought about by experience with prior adversity.

An alternative explanation for our findings is due to mortality selection. The argument here is that severe challenges, like famine, frequently lead to increased mortality, particularly impacting the most vulnerable individuals in the population (Gørgens et al. 2012). Consequently, if the survived cohorts are genetically more resilient, then our positive finding could be due to a mortality selection effect, as opposed to a resilience enhancing effect.

To test whether mortality selection is driving our results, instead of comparing individuals who were exposed to an ESE with individuals who were not, we compared the offspring of both groups. Our rationale being that if individuals with experience of an ESE are indeed genetically more resilient than those who were not exposed to an ESE, one should observe a similar effect between their children. We however do not find such evidence, indicating that our finding is unlikely to be a selection effect. As a further test, we compared cohorts born *during* the famine to the cohort born immediately before the famine. If mortality selection was a factor, one would expect it to be more of a factor for infants, as those born during the famine are more likely to die as a result of famine exposure than those born before (see Hu et al. 2017). In effect, the treatment effect should be stronger for infants born during the famine. The results again suggest, however, that our findings are due to a resilience enhancing effect as opposed to mortality selection.

After documenting the resilience enhancing effect of experience with an ESE, we conducted a preliminary exploration into the potential for non-linear effects. This is inspired by a key argument in the steeling effect theory, namely that the resilience-enhancing impact of early life adversity may be diminished or even reversed when the adversity is too severe (Seery et al. 2010; Rutter 2012). We conduct three tests to examine this issue. First, assuming the reported ESE from provinces with higher excess death rates (EDR) during the famine is more severe, one would expect the estimate of our key variable to diminish with an increase in EDR. This is what we find. This finding also counters again the competing explanation of mortality selection. This is because if mortality selection was the driving force behind our results, we would expect the treatment effect to be larger in those regions where the famine was most severe.

Second, respondents with experience of an ESE also report a time period relating to when the starvation episode happened. We utilize this data as a proxy of ESE intensity. The assumption here is that a longer reported time period could encompass more starvation episodes, that is, a more intense adversity. We observe a non-linear relationship between this measure of ESE intensity and future resilience, which suggests any positive resilience enhancing effect would be attenuated by more severe adversity.

Third, given the prevalent culture of son-preference in China (Gørgens et al. 2012; Zhao and Reimondos 2012), we assume that females were exposed to more severe ESEs. Recognising this, we split our sample into male and female sub-groups and replicated our baseline regression. Here we find that the resilience enhancing effect of an ESE was largely concentrated in the male sub-group. Each of these tests have their own advantages and disadvantages, but taken together they imply that while early life adversity can be beneficial for the formation of future resilience, too much could lead to backfire.

Our research contributes to a nascent economic literature exploring the determinants of individual resilience. Resilience has been shown to play an important role in numerous individual behaviours and life outcomes, for example, sleep quality (Li et al. 2019), longevity (Law et al. 2014), school performance (Scales et al. 2006) and labour market choices (Caliendo et al. 2015; Cheng et al. 2022; Schuler 2017; Yi et al. 2022). However, to date, individual resilience has attracted limited attention from economists (Asheim et al. 2020). The existing research in this area has documented the importance of material aid and training (Phadera et al. 2019), childhood health as proxied by birthweight (Guo et al. 2022), fintech tools (Suri et al. 2021) and non-cognitive skills such as self-efficacy (Johnston et al. 2021) in predicting individual resilience. The novel feature of this work is that here we investigate whether the experience of adversities in early life affects the formation of future resilience.

The study that we are aware of which came closest to empirically testing this theory of psychophysiological toughness is that by Seery et al. (2010). Using longitudinal surveys from the US, they observed a U-shaped quadratic relationship such that people who experienced *some* lifetime adversity fared better in mental health terms than those with no or high levels of adversity. They did this by assessing the degree to which the experience of cumulative past adversity (collected from wave 1) attenuated the impacts of more recent adversities (collected from wave 2) on future mental health (waves 3 and 4). They concluded that '*in moderation, whatever does not kill us may indeed make us stronger*'. One limitation of their study is that the findings only show correlations between cumulative adversities and resilience, not causal relationships. For example, their regression focuses on between-individuals effects, which might be biased by unobservable individual characteristics such as personality traits or developmental environment. Moreover, recent adversities in their study are likely correlated with cumulative past adversities, leading to potential endogeneity issues when it comes to identifying causal effects. The contribution of our paper is that we identity a causal effect of early life adversity on individuals' future resilience through a quasi-experimental method.

We also add to a broad literature investigating the long-term impacts of childhood experiences or circumstances on future life outcomes (e.g. Almond et al. 2018; Hoynes et al. 2016; Sviatschi 2022). Numerous studies in this field have focused on adverse events, such as experiences of war (e.g. Kesternich et al. 2014; Kesternich et al. 2015; Singhal 2019), neighbourhood deprivation (Chetty and Hendren 2018) and episodes of hunger (e.g. Chen and Zhou 2007; Cui et al. 2020). The majority of these studies have found that early life adversity can result in long-lasting negative outcomes such as lower income and poorer mental health. Our results however suggest that it may also generate certain positive spillover effects, namely enhancing resilience.

The remainder of this paper is organised as follows. In Section 3.2, we provide a more detailed explanation of our proposed link between early adversity and resilience.

This is followed by a description of our two resilience shocks: China's Great Famine and the Covid-19 pandemic. Section 3.3 outlines our data sources. Section 3.4 describes the empirical strategy. In Section 3.5, we document our main findings relating to the effect of early life adversity on the formation of resilience as well as various robustness checks. In section 3.6, we test for non-linear effects between adversity and resilience, in keeping with the steeling effect theory. Lastly, we conclude with a discussion of our main findings in Section 3.7.

3.2 Contexts and theoretical discussion

3.2.1 Early life adversity and resilience

While the effects of early life adversity for future life outcomes (e.g. labour market, health etc.) has been extensively studied, the relationship between early life adversity and future resilience is still an open question. A good starting point in thinking about the potential effects of adversity in shaping future resilience is to note how many studies have shown that traumas in early life generate long-lasting negative impacts on one's future life outcomes (Kesternich et al. 2014; Singhal 2019). For example, Chen and Zhou (2007) show how individuals exposed to the CGF in their childhood have on average shorter height and lower levels of labour supply in adulthood. Further research has documented a link between early life adversity and the development of stress disorders and depression in adulthood (Cui et al. 2020; Gilbert et al. 2009).

Building on this existing work, one may argue that early life adversity would damage future resilience, much like it is detrimental to other future life outcomes. This essentially assumes that better (worse) life outcomes are directly attributed to stronger (weaker) resilience. Following this reasoning, however, one could easily reach an opposite conclusion since early life adversity does not always lead to negative life outcomes. Recent research using the CGF as a marker for adversity, for instance, suggests that individuals who experienced the CGF are more likely to become entrepreneurs (Cheng et al. 2021; Yi et al. 2022). Similarly, Wang et al. (2019) find that, in the long run, scientists who experienced setbacks early in their careers appear to outperform those who do not have such experiences.

Focusing specifically on resilience, in contrast to other outcomes (e.g. health, employment outcomes), there is a dearth of literature, especially empirical literature, on the impact of early life adversity. Perhaps the most notable work in this area is by Dienstbier (1989) who proposed a theory of psychophysiological toughness which became known as the steeling effect theory.[2] The proposition here was that stressful experiences in early life, if coupled with opportunities to recover sufficiently, could foster subsequent resilience. The proposed explanation being that moderate adversity could fine-tune physiological responses to stressors, optimise hormone levels and bolster one's ability to adapt while maintaining emotional stability in challenging situations. Dienstbier also argued that while some adversity may foster resilience, excessive exposure could lead to a lack of toughness, i.e. do more harm than good. There is at least some popular support for this argument amongst the general public given how well known the expression '*whatever does not kill you will make you stronger*' is.

The study that has most closely empirically tested this theory of psychophysiological toughness to date is by Seery et al. (2010). They documented an inverse U-shaped relationship between accumulative lifetime adversities and individuals' mental health in the US. Specifically, moderate levels of accumulative adversity predicted relatively lower distress and higher life satisfaction, while high levels of accumulative adversity were associated with a decrease in mental health.

While Seery et al. (2010) directly looked at the association between exposure to adversity and resilience, other related studies have looked at the potential role of adversity in nurturing individuals' non-cognitive abilities, which have been argued are instrumental in helping individuals cope with future challenges. As an illustration, it has been argued that surviving from adversities could potentially elevate an individual's sense of mastery and control (Minerva and Zinbarg 2006).

An advantage of our work, relative to existing work, is that we link two adversities at the individual level, namely exposure to starvation during the Chinese Great Famine and the Covid-19 pandemic. This allows us to examine how exposure to the former affects individuals' ability of coping, in mental well-being terms, with the latter. We use changes in mental well-being following a major life event as a proxy for resilience, because resilience is fundamentally about how well individuals maintain psychological stability in the face of adversity (Bonanno 2004; Bonanno et al. 2006; Etilé et al. 2021; Orcutt et al. 2014). Thus, assessing changes in mental well-being during the covid-19 pandemic provides a measurable and reliable indication of a person's resilience (Johnston et al. 2021).

[2] After Dienstbier (1989), some scholars have proposed different concepts like immunization (e.g. Başoğlu et al. 1997), stress inoculation (e.g. Lyons and Parker 2007) and steeling (e.g. Rutter 2012), the basic ideas however are similar.

3.2.2 China's Great Famine

China's Great Famine, labelled by the government as the "*Three years of difficulty* (1959-1961)", was one of the deadliest famines in the 20th century. An estimated 15 to 30 million people, mainly from rural areas, prematurely perished during that period (Ashton et al. 1984; Peng 1987). Multiple factors contributed to this catastrophe. For its part, the government attributed it to poor weather leading to a dramatic decline in grain production. This is at least partly true: the average proportion of sown area hit by natural disasters (e.g. drought) in 1959–61 was more than double that which had occurred in the immediate years preceding 1959 and post 1961 (Kueh 1995).

There is also a good deal of evidence to suggest that policy failure also played a crucial role. In particular, the Great Leap Forward Campaign,[3] initiated in 1958 by the top leader with the aim of quickly building China into a modern communist country is now widely recognized as the main factor behind the Great Famine. First, under this program, a large volume of agricultural resources (e.g. labour force) was diverted for industrial production, and this had a significant detrimental effect on grain production (Li and Yang 2005). Additionally, there was a push towards the formation of people's communes in rural regions, wherein all resources for production were pooled collectively, and food provided free of charge to inhabitants of the area. This resulted in both food waste (Chang and Wen 1997) and reduced food output (Lin 1990). Finally, the political climate incentivized local officials to over-report grain production to please upper-level authorities, leading to unsustainable government procurements of grain from rural areas. This directly led to food shortages in some rural areas (Kung and Chen 2011; Meng et al. 2015). These arguments are supported by the observation that the mortality rate started to rise from 1958 onwards, coinciding with the onset of the Great Leap Forward Campaign. Building on this literature, we define the famine period as spanning from 1958 to 1962, characterized by markedly higher mortality rates as compared to other years.

3.2.3 The Covid-19 pandemic in China

The Covid-19 pandemic is a global public health crisis caused by the SARS-CoV-2 virus. Originating in Wuhan, the capital of Hubei province, China, in December 2019, it rapidly spread throughout the whole country (Du Toit 2020). The virus is highly contagious, and infected individuals can develop severe, and in some cases, fatal

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[3] For a more detailed introduction to this campaign, we refer readers to Li and Yang (2005).

respiratory diseases (Rothan and Byrareddy 2020). In order to contain the pandemic, the Chinese government swiftly adopted stringent lockdown measures. All transport in and out of the province of Hubei was prohibited from 23 January 2020. Within the next week, all other provinces in China had initiated similar measures. Local authorities at various levels also rolled out their own individual measures to minimize human interaction, including the prohibition of public gatherings, restricted travel and controlling who can enter and exit neighbourhoods (Tian et al. 2020).

With the implementation of these strong containment policies, the daily reported domestic infection cases in China had fallen to single digits in two months. By the end of March 2020, no new cases were reported in any of the provinces with the exception of Hubei.[4] Since then, lockdown measures were eased but large-scale gatherings were still not allowed, and a valid digital health code was mandatory for accessing any public facilities or travelling between cities (General Office of the National Health Commission 2020). The pandemic itself led to a deterioration in mental health due to the fear of, or actual infection, but also the lockdown measures employed were shown to generate significant negative impacts for individual mental health (e.g. Gao et al. 2020; Song 2020; Wang et al. 2020). The pandemic therefore offers an excellent opportunity to gauge resilience. In particular, it provides us with a second stressor - an exogenous event that has imposed challenges on people's lives – allowing us to evaluate whether early life adversity fosters resilience.

3.3 Data

3.3.1 China Family Panel Studies

Our individual data capturing individuals' mental well-being and their experience with extreme starvation comes from the China Family Panel Studies (CFPS, Institute of Social Science Survey, Peking University 2015). This longitudinal nationally representative survey has been conducted every two years since 2010 with the last wave conducted in the second half of 2020. The CFPS samples approximately 32,000 adults (age ≥ 16 in each wave) across 25 provinces which represent 95% of the total population

[4] This situation sustained until December 2020, when the second wave of pandemic hit China. Hence, we refer to the period between January and December 2020 as the first wave of the pandemic in China, of which the time period before March 2020 is regarded as the most disruptive period.

in China.[5] It captures rich demographic information, including various measures of mental well-being as well as early life experiences.

Our key variable of interest captures whether an individual experienced an extreme starvation episode. In the 2010 wave of the CFPS, respondents born before 1979[6] were asked whether they have experienced persistent hunger for at least one week (hereafter referred to as extreme starvation episode (ESE) and those that do report an ESE were then subsequently asked to indicate the period when the experience happened.[7] Approximately 15% of respondents surveyed reported an ESE and 70% of them specified that the ESE happened in a period which overlaps with the CGF, i.e. either the start year or the end year lies between 1958 and 1962.

We illustrate the spread of initial years when reported ESE events occurred in Figure A3.1. The data indicates a notable clustering from 1958 to 1962, particularly peaking around the onset of the CGF. To leverage the exogenous nature (at individual level) of the CGF, this study focuses on reported ESEs which occurred in a period overlapping with the CGF. We focus on ESEs during this period as we can reasonably assume these ESE events are largely an exogenous shock to individuals, given that the events leading to famine at this time were largely beyond individuals' control. Here we are including all individuals once they reported an ESE between 1958 and 1962. For a small number of these individuals, the reported ESE began just before this period or extended just after. For example, an individual could have reported an ESE beginning in 1957 (just before the CGF) but the duration extended to 1958. This is still likely attributable to the CGF, but we can be less sure about the exogeneity of any such starvation episodes. We therefore conducted a robustness check by only using ESE events which happened strictly within the CGF period, i.e. both the start year and the end year of any reported starvation episode lies between 1958-1962. This yields a similar result as our baseline analysis (see column 8 in Table A3.7).

[5] The 31 provinces in mainland China are covered by the CFPS except Xinjiang, Tibet, Qinghai, Inner Mongolia, Ningxia and Hainan.

[6] The reason why the CFPS chose this timing perhaps is that people are less likely to experience starvation after the economic reform (1978).

[7] The period is specified with a start year and an end year, and the CFPS does not ask the frequency of such extreme starvation experiences that happened during that period. We thus use a dummy variable to indicate whether a person have experienced an ESE or not in our primary analysis. Nevertheless, in a suggestive examination of the steeling effect theory (detailed in Section 3.6.2), we also consider the duration of the reported time period, among others, as a proxy for the intensity of ESE. Our rationale is straightforward: a longer time period is likely to encompass more instances of starvation, although we acknowledge that this is not a precise measure of ESE intensity.

Previous studies assessing the impact of early life adversity on future outcomes such as height and earnings often rely on an examination of differences across cohorts (born before compared to those born after the famine - e.g. Chen and Zhou 2007). One disadvantage with this approach is that it could introduce potential cohort bias in the estimation. The self-reported data of ESEs contained in the CFPS allows us to alleviate such issues as we can focus on a narrow comparable group of cohorts. Specifically, in our baseline analysis, we focus on a narrow group of individuals born just before the onset of the famine, namely between 1950 and 1957 and within this group we can identify those who experienced starvation and those who did not.

In our baseline setting, we do not include individuals born during the famine as if mortality selection is a factor (later we rule this out) it would be most likely to occur amongst infants born during the famine period, as those had the greatest mortality risk (see Hu et al. 2017). Those born during the famine are also less likely to remember and subsequently report a famine experience and so measurement error is likely a bigger hurdle to overcome here. Of course, measurement error is also a possibility when focusing on those born before the famine, but less so and to the extent that measurement error exists, it would lead to a downward bias and so works against us (we discuss this further below). We note here however that including individuals born during the famine period into our baseline setting does not have a material impact on our results discussed later.

We choose 1950 as our cut-off point because this means we can exclude individuals born before the founding of the People's Republic of China (1949). This criterion ensures that none of the respondents experienced the civil wars characterized by significant violence and social upheaval, which itself could potentially influence individuals' resilience. As the famine mainly affected rural areas, we focus on individuals that still hold a rural *Hukou* in 2010.[8] Therefore, our baseline sample covers respondents born in rural China between 1950 and 1957, who were in their childhood during the famine period (aged between 5-12 by the end of the famine, 1962).[9] Individuals who reported an ESE that occurred during the CGF are assigned to the treatment group, while those who do not report any ESE serve as the control group. Note that we can obtain a qualitatively similar result if we remove these sample

[8] This guarantees that they were born in rural areas and stayed there during the famine, because Chinese people were deprived of the right of free migration until 1980s. We prefer focusing on individuals born in rural areas as it makes the samples more comparable, note however we can obtain qualitatively similar results if including urban individuals in our regression.
[9] Early childhood is a key life stage for the formation of resilience (Masten and Barnes 2018).

restrictions, i.e. considering all individuals who were asked to report whether they experienced an ESE in our analysis (see Table A3.7).[10]

One might have concerns surrounding the reliability of the retrospective information on childhood starvation experience. We argue that this is unlikely to be a serious problem in this case. For one, the memory of certain past traumas and pains can be lifelong and even intensify over time (Squire 1987). Second, hunger experience before China's economic reform (1978) does not carry a stigma, and so reporting an ESE is unlikely to be subject to intentional misreports or data manipulation (Cao et al. 2022). Indeed, there is a growing literature taking advantage of such self-reported measures to examine long-term consequences of childhood starvation experience for later life outcomes such as height and mental well-being (e.g. Bertoni 2015; Cui et al. 2020; Kesternich et al. 2014; Kesternich et al. 2015).

In any case, if measurement error is a problem, we would argue that it is likely to work against us and cause us to underestimate the treatment effect.[11] To illustrate, given the binary nature of this information (experienced an ESE yes or no), there can be only two types of misreports: 1) individuals who did experience an ESE claiming that they did not have such an experience; 2) individuals without an ESE reporting that they did have such an experience. Without loss of generality, we can assume that individuals with an ESE have stronger (weaker) resilience. Then moving some of these individuals (misinformants) from the treatment group into the control group will only narrow the gap between our treatment group and the control group, as individuals with stronger (weaker) resilience are shifted to the control group. Similarly, moving individuals who de facto have no experience of an ESE into the treatment group will also narrow the gap between the two groups. In each case, any misreporting is likely to cause us to underestimate the effect of the treatment.

Mental well-being. Our main measure of mental well-being is life satisfaction. To capture life satisfaction, respondents are presented with a 5-point scale and asked to answer the following question: "*Are you satisfied with your life*", with 1 indicating least satisfied and 5 most satisfied. We focus on this measure as it is asked across all waves of the CFPS. In the 2018 and 2020 survey waves, the CFPS also records a measure of self-reported happiness, which we employ as an alternative indicator of mental well-

[10] Here the analysis covers all individuals born before 1979, including those born before 1950 and urban residents. Specifically, the treatment group consists of all individuals who reported an ESE which occurred in a period overlapping with the CGF, irrespective of when or where they were born.

[11] Indeed, studies have found that measurement error in an independent variable based on self-reported data can lead to attenuation (downward) bias in the estimate (e.g. Lassen 2005).

being in a robustness check. Here respondents are presented with a 10-point scale and asked: "*Are you happy overall?*", with 1 indicating least happy and 10 most happy. The CFPS also regularly includes a depression index assessed by the Centre for Epidemiologic Studies-Depression Scale which we also use as an indicator of mental well-being (CESD scale, Radloff 1977, see Table A3.1).

To assist with identifying the effect of the ESE from any confounds, we also include some demographic information from the CFPS in our baseline analysis. This includes age, gender, the number of siblings, the family class ("red (good)" class, "ordinary" class, "reactionary (bad)" class)[12], marital status and residence (rural or urban area). Our results are not sensitive to the addition of any of these controls. Further details regarding the primary variables of interest are available in Table 3.1.

3.3.2 Covid-19 pandemic severity

In this study, the Covid-19 pandemic serves as our second 'stressor'. We look at how the group exposed to an ESE cope in terms of mental well-being with the pandemic as compared to those who did not experience an ESE. To capture variations in the Covid-19 pandemic severity, we use the infection rate (the number of infections per 100,000 population) at each province as our primary measure of 'Covid-19 adversity'. This is because, all things being equal, a rise in infected cases generally led to stricter lockdown measures by the local government and hence adversity when it came to restrictions on their everyday lives. Additionally, individuals might be more worried about being infected when there are more cases in their own province, which in turn would bring greater psychological distress.

As a robustness check, we also use the number of death cases at each province as a measure of Covid-19 pandemic severity.[13] In a further sensitivity check, instead of relying on spatial variation in Covid-19 severity, we rely on temporal variation. Here, we simply assume that all individuals were treated to the same degree in terms of adversity irrespective of location and simply compare the change in life satisfaction before and during the pandemic between the treatment and control groups.

[12] The class labels might influence how much public resources were allocated to a family in China before the reform and openness, and this factor also has been shown to have a long-run impact on individuals' success (Treiman and Walder 2019).

[13] We do not discount this data to a per capita level as the death cases during the first wave of the pandemic in China (except for Hubei province) is in a very small scale. Each death case was influential in terms of local government actions and news coverage.

We obtained our pandemic severity data from the Real Time Big Data Report of Covid-19 Pandemic made by *Baidu* (Baidu 2020), a Chinese giant of digital technology. The CFPS 2020 survey interviews were conducted between July and December of 2020. As mentioned in Section 3.2.3, the most disruptive period of the first pandemic wave in China ended in March 2020 and there were no more reported cases of infections until December 2020.[14] We thus use the data of total infection rate up to the end of March 2020 as a proxy indicator capturing the degree of adversity experienced by individuals in these provinces. We exclude Hubei province (1.43% of the sample) from our baseline analysis because its infection (death) cases are at least 30 times more than the other provinces.[15] The CFPS contains a geographic identifier which allows us to exclude individuals who migrated across provinces between 2018 and 2020.[16]

If focused specifically on the impact of the pandemic for mental health, one may contend that we could underestimate the impact, as the most disruptive period of the pandemic happened prior to the interviews conducted between July and December 2020. Our purpose, however, is not to precisely evaluate the impact of the pandemic for mental health. Instead, we are using the pandemic as an instrument for assessing individuals' resilience, which, by definition, addresses one's ability of bouncing back and/or coping with a crisis (Bonanno 2004; Rutter 2012). Therefore, we can appropriately capture people's resilience even if they had started to recover from the most disruptive period of the crisis. We also note that at the time period of these surveys, the Covid-19 pandemic was still a disruptive force in people's lives as vaccinations had still not taken place and some policies aimed at restricting travel remained effective (see Section 3.2.3).

3.3.3 Descriptive statistics

Our baseline analysis focuses on the 2018 and 2020 CFPS survey waves, from which we obtain a balanced panel of 2,856 individuals aged 63 to 70 in 2020 after observations with missing values on variables were excluded.[17] The summary statistics of all

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[14] 2% of respondents were interviewed in or after December 2020, and these were removed from our study because they may experience the second pandemic wave in China.

[15] Even though it represents a small percentage of our sample (1.43%), including such an outlier could lead to very misleading results as the OLS regression estimates the average effect which means the estimated coefficient will be driven to 0 by such an outlier. Nevertheless, in a robustness check where we rely on temporal variations (a dummy variable indicating before and after 2020), instead of infection rates to capture the shock of pandemic, Hubei province is included and the results are qualitatively similar to our baseline analysis.

[16] This only consists of 0.4% of our sample because our samples are comparatively older people who are less likely to migrate. Of course, our results remain similar if including those immigrants.

[17] 9% of our samples are excluded due to missing values, we however confirm that the results remain very similar when including those observations (no controls are added into the regression).

variables are presented in Table 3.1. There are 602 observations in the treatment group, accounting for 21% of the total sample. Before the pandemic, the mean value of life satisfaction in the treatment group is modestly smaller than that of the control group while the situation shifted after the pandemic. Although this difference is statistically insignificant, it points towards a potential positive impact of childhood ESEs on individuals' resilience.

The two groups are broadly comparable in terms of background characteristics. There are some small differences in terms of education with the treatment group having less education overall. They are also less likely to become a member of the CPC. These differences are not unexpected as previous studies have shown that famine experiences in early life could generate negative impacts on future life outcomes (e.g. Chen and Zhou 2007; Cui et al. 2020). Noticeably, one can see that the proportion of females in the treatment group is also somewhat lower than in the control group. At first glance, this seems counterintuitive as one could argue that girls should be more likely to experience extreme starvation, in light of a strong culture of son preference in China (Das Gupta et al. 2003). Previous research has however established that women did experience more starvation episodes during the famine but this also translated into a higher mortality rate (Zhao and Reimondos 2012) and this could explain somewhat the higher proportion of males in our treatment group.

Table 3.1 Descriptive statistics

	Treatment group (A)			Control group (B)			t-test between groups (A – B)	
Variables	N	Mean	SD	N	Mean	SD	Difference	T-value
Key variables								
Life satisfaction (2018)	301	4.166	1.055	1,127	4.191	0.978	-0.025	-0.37
Life satisfaction (2020)	301	4.296	0.888	1,127	4.268	0.895	0.028	0.48
Covid (infection rate)	602	1.549	1.345	2,254	1.549	1.345	-	-
Baseline controls								
Age	602	65.83	2.254	2,254	65.06	2.393	0.778***	7.42
Gender (female=1)	602	0.455	0.498	2,254	0.519	0.5	-0.064**	-2.79
Sibling	602	3.957	1.843	2,254	3.811	1.905	0.146	1.72
Residence (urban=1)	602	0.346	0.475	2,254	0.35	0.477	-0.005	-0.21
Family class								
Bad class	602	0.056	0.235	2,254	0.053	0.225	0.003	0.31
Ordinary class	602	0.243	0.426	2,254	0.24	0.427	0.002	0.12
Good class	602	0.701	0.457	2,254	0.706	0.456	-0.005	-0.25
Marital status								
Single	602	0.017	0.13	2,254	0.003	0.056	0.014*	2.53
In marriage	602	0.867	0.33	2,254	0.885	0.32	-0.018	-1.14
Divorced	602	0.012	0.109	2,254	0.008	0.091	0.003	0.67
Widowed	602	0.105	0.293	2,254	0.104	0.305	0.001	0.06
Variables used in robustness checks and alternative explanations								
Height (centimetre)	601	161.6	8.535	2,254	161.5	8.917	0.108	0.27
Body mass index (weight/height^2)	601	22.77	3.214	2,245	22.997	3.506	-0.227	-1.50
CPC member	582	0.07	0.255	2,254	0.114	0.318	-0.045***	-3.61
Family per capita income (10k CNY)	577	1.56	3.891	2,231	1.475	1.652	0.085	0.52
Household size	577	3.955	2.085	2,231	3.957	2.188	-0.002	-0.02
Education level								
Illiteracy	602	0.591	0.492	2,254	0.477	0.500	0.114***	5.03
Primary school	602	0.226	0.419	2,254	0.232	0.423	-0.007	-0.34
Middle school	602	0.133	0.34	2,254	0.209	0.407	-0.077***	-4.70
High school and above	602	0.05	0.218	2,254	0.081	0.273	-0.031**	-2.92
Labor status								
Workers in non-agricultural sector	585	0.096	0.294	2,186	0.111	0.314	-0.015	-1.08
Agricultural practitioner	585	0.65	0.478	2,186	0.621	0.485	0.029	1.29
Non-labour (mainly retiree)	585	0.255	0.436	2,186	0.269	0.443	-0.014	-0.68
Self-report health status	582	2.6	1.312	2,254	2.577	1.305	0.022	0.37
Happiness	578	7.621	2.184	2,237	7.645	2.189	-0.024	-0.23
Depression score	598	14.31	4.522	2238	14.13	4.518	0.181	0.868
Covid (death cases)	602	4.667	4.896	2,254	4.667	4.896	-	-

Note. Data on Covid-19 are calculated at province-year level, there is no difference between the treatment group and the control group.

3.4 Identification strategy

In the baseline analysis, we employ a two-period (2018 and 2020) difference-in-difference (DID) model to evaluate the effect of childhood ESE on individuals' resilience as follows:

$$LS_{ipt} = \beta \cdot ESE_i \times Covid_{pt} + \boldsymbol{X}_i^a \times Covid_{pt} \cdot \boldsymbol{\gamma} + \boldsymbol{X}_{it}^b \cdot \boldsymbol{\varphi} + \vartheta_t \times \mu_p + \vartheta_t + \lambda_i + \varepsilon_{ipt} \quad (3.1)$$

Where LS_{ipt} is life satisfaction of individual i in province p at wave t, ESE_i indicates whether individual i experienced extreme starvation during the CGF, it equals 1 if yes and 0 otherwise. $Covid_{pt}$ denotes the severity of the Covid-19 pandemic in province p at wave t (note it equals 0 when t=2018). As the time interval between March 2020 and the interview timing of the CFPS 2020 might influence the degree to which one has recovered from the most disruptive period of the pandemic, we control for an interaction term of this time interval (*Recover*, measured by months) and $Covid_{pt}$. This is included into the vector $\boldsymbol{X}_i^a \times Covid_{pt}$. To improve the estimation precision, we control for a few time-varying individual characteristics, $\boldsymbol{X}_{it}^b$, which includes age (squared), marital status and residence. ϑ_t captures the wave effect, λ_i denotes the individual fixed effect (this also absorbs provinces fixed effects) and ε_{ipt} is the random error term. Note that the variable $Covid_{pt}$ does not appear in equation 2.1 as it is absorbed by the province trends, $\vartheta_t \times \mu_p$.

The coefficient in front of $ESE_i \times Covid_{pt}$, β, is the quantity of interest. β captures the difference in the change of life satisfaction before and during the Covid-19 pandemic between individuals with and without a childhood ESE. If β is significantly positive (negative), it would mean that experience of an ESE in childhood has strengthened (damaged) individual resilience.

The reliability of a standard DID identification depends on the assumption that in the absence of the treatment, the average outcome for the treatment and control groups would have evolved in parallel. This would support the conjecture that the key coefficient β captures an effect triggered by the Covid-19 pandemic (i.e. an adversity shock) rather than unobservable confounders that have heterogeneous effects on life satisfaction across the two groups. However, one may still argue that this is insufficient to establish a causal relationship between childhood ESE and resilience. The reason is that there might be some factors simultaneously affecting the assignment of ESE and one's ability of coping with the pandemic. For example, some individual (family) characteristics may affect the likelihood of experiencing extreme starvation during the famine. This poses a risk of omitted variable bias in our estimation if these factors also influence how one copes with the pandemic. This is very unlikely in this setting as the factors leading to extreme starvation are unlikely to remain effective after 60 years.

We employ a number of strategies to alleviate this concern, nevertheless. First, to take into account regional differences (e.g. local government quality), we add interactions of province dummies and $Covid_{pt}$ to our baseline model, which is captured by province trends, $\vartheta_t \times \mu_p$. Second, we control for interactions of $Covid_{pt}$ and some individual characteristics that might influence the probability of experiencing extreme starvation during the famine. This includes gender, age, the number of siblings and the family class. These interactions are captured by the vector $\boldsymbol{X}_i^a \times Covid_{pt}$.

Furthermore, we conduct several tests to examine the degree to which our finding is sensitive to omitted variable bias. First, we employ a propensity score matching method (PSM) to match more similar control samples for each treated individual before conducting the DID regression. Second, we use the Oster (2019) method to evaluate the possibility of omitted variable bias in our estimation. Third, we run a permutation test by randomly rearranging the ESE among our samples and then implementing the DID regression based on these false treatment groups. If our finding were driven by omitted variables, one would expect a non-null effect in this test. Our final strategy is to simply compare cohorts born before the famine with a small group of cohorts born immediately after the famine. Our intuition here is simply that, overall, the group of individuals born before the famine will have had a greater exposure to the treatment (starvation). Doing this means that we can completely circumvent any confounders surrounding the assignment of ESE.

To further establish the causal relationship between childhood ESE and individual resilience, we also rule out a competing explanation. Specifically, we examine whether our finding is driven by a mortality selection effect. The argument here being that individuals with ESE are genetically more resilient because the famine was a natural screening process (Gørgens et al. 2012). Finally, inspired by the steeling effect theory, we also implement a preliminary examination on whether there is a non-linear relationship between ESE intensity and future resilience. We leave the detailed discussion to Section 3.6.

3.5 Main findings

3.5.1 Baseline results

The results of our baseline analysis are presented in Table 3.2. We start with a parsimonious regression in which only our *Covid* variable, which captures pandemic severity, and individual fixed effects, in addition to wave dummies are included in the model. In column 1, we can see that our key interaction term, *ESE* × *Covid*, attracts a positive coefficient at the 1% significance level (β = 0.052, p = 0.003). This suggests that experience of an ESE is positively correlated with individuals' ability of coping with the pandemic. The estimates pertaining to the key interaction term do not change much when we further control for individual characteristics (column 2) and province trends (column 3). Finally, we add all controls depicted in equation 2.1 into the regression, and our key interaction term *ESE* × *Covid* remains significantly positive and qualitatively similar to our baseline estimates (β = 0.077, p = 0.000).

These results indicate that individuals who experienced extreme starvation in childhood show greater resilience, in terms of their mental well-being as captured by life satisfaction, to the challenges posed by the Covid-19 pandemic. This lends support to the proposition that early life adversity can enhance one's resilience. In numerical terms, when comparing those with ESE to those without, a one standard deviation (SD, 1.345) increase in the Covid-19 infection rate corresponds to a 0.104 points difference in reported life satisfaction. This difference is equivalent to 12% of the SD (0.892) of the average life satisfaction in 2020, or 17% of the impact (0.611) of becoming divorced on life satisfaction.

To help get a better understanding of the size of these effects, we can compare it to the impact of age in coping with the pandemic. The results in column 4 show that within our sample (which consists of those aged between 63 to 70 in 2020), the negative estimated impact of the pandemic increases with age. Specifically, being one-year older leads to an estimated 0.011 points reduction in life satisfaction when the Covid-19 infection rate increases by one standard deviation. This is to be expected given that comparatively older people are more vulnerable to the virus, and this likely leads to greater anxiety concerning the consequences of becoming infected. Our findings suggest that, within the same sample, the resilience developed from childhood ESE is equivalent to being 63 as opposed to 70 when it comes to predicting how well one copes in mental well-being terms with the pandemic.

Table 3.2 Baseline results

	Dependent variable: Life satisfaction			
VARIABLES	(1)	(2)	(3)	(4)
ESE × Covid	0.052***	0.053***	0.059***	0.077***
	(0.017)	(0.017)	(0.018)	(0.020)
Covid	-0.013	-0.012		
	(0.012)	(0.011)		
Time-varying individual controls				
Log age		10.075	8.488	1.765
		(11.783)	(11.727)	(11.185)
Residence (urban=1)		-0.032	-0.019	-0.041
		(0.163)	(0.166)	(0.167)
Marital status (refer to inmarriage)				
Single		0.914***	0.893***	0.928***
		(0.017)	(0.004)	(0.011)
Divorced		-0.660***	-0.582***	-0.611***
		(0.160)	(0.155)	(0.141)
Widowed		-0.098	-0.072	-0.087
		(0.128)	(0.131)	(0.134)
Interactions of individuals factors and Covid				
Age × Covid				-0.011***
				(0.004)
Female × Covid				0.019
				(0.018)
Siblings × Covid				-0.014***
				(0.005)
Family class label × Covid (refer to bad class)				
Ordinary class × Covid				0.019
				(0.033)
Good class × Covid				-0.002
				(0.037)
Recover × Covid				0.011
				(0.012)
Individual FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Province trends	No	No	Yes	Yes
R-squared	0.007	0.009	0.022	0.025
Observations	2,856	2,856	2,856	2,856

Note. Robust standard errors clustered at province-year level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

3.5.2 Parallel trend test

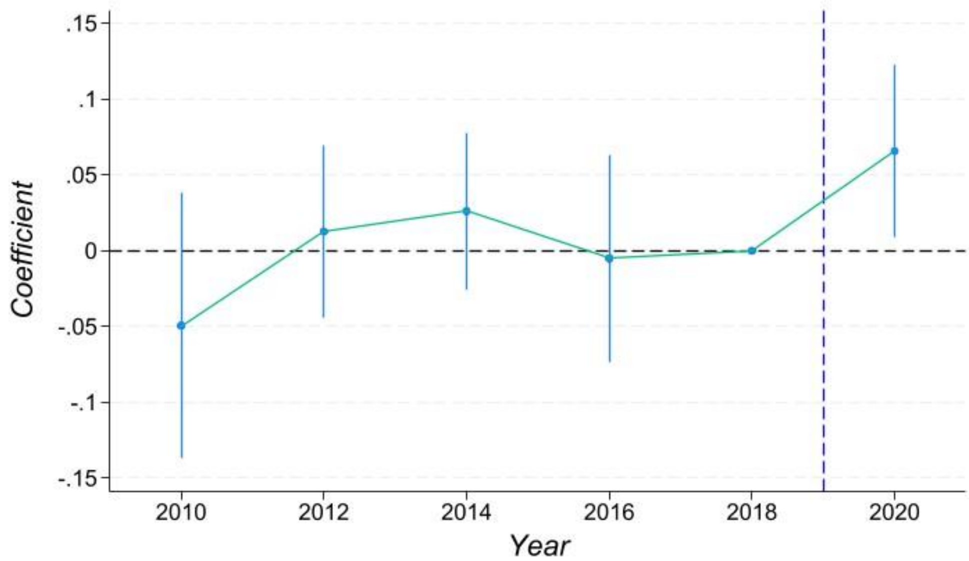
The key assumption of our identification strategy is that the treated and the control groups have a parallel trend in the pre-treatment period. This helps to rule out the possibility that our findings are driven by some time-varying confounders that are correlated with the Covid-19 pandemic and have heterogeneous effects on life satisfaction across the two groups. We now examine this assumption using data from all waves (2010 to 2020) of the CFPS through the following equation:

$$LS_{ipt} = \sum_{k=2010}^{2020} \beta_k \cdot ESE_i \times Covid_{pt}^{k} + \vartheta_t \times \mu_p + \vartheta_t + \lambda_i + \varepsilon_{ipt} \qquad (3.2)$$

Where $Covid_{pt}^{k}$ represents a series of treatment variables that equal to 0 except in the year k $(k = 2010, 2012, 2014, 2016\ and\ 2020)$, where it takes the true value of

Covid-19 infection rate in 2020. As the Covid-19 pandemic only happened in 2020, we should observe an insignificant β_k for all $k \neq 2020$ if our findings are not caused by time varying confounders. Here we do not control for any individual characteristics or their interactions with Covid-19 severity, which means our results will not be conditional on the selection of these covariates.[18] To obtain more observations, here we rely on an unbalanced panel data, but the estimates are largely the same when using a balanced panel setting. We plot the estimates of β_k in Figure 3.1 (the details can also be found in Table A3.2). It shows that all estimates before 2020 are statistically insignificant and most of them, particularly in the time-periods immediately before the Covid-19 shock, are indeed close to zero.

Figure 3.1 The parallel trend test



Note. All coefficients are estimated with referring to the year 2018, which means β_{2018} is omitted in the estimation. Whiskers denote 95% confidence intervals, which are calculated based on standard errors clustered at province-year level.

3.5.3 Sensitivity tests on the assignment of ESE

The difference between our study and a standard DID analysis is that we are interested in an interactive effect of two treatments, namely ESE and the Covid-19 pandemic. This loads an additional layer of complexity on our identification strategy. It means we also need to consider whether our estimation is biased by omitted variables that simultaneously affect the assignment of ESE and how one copes with the pandemic. Such a variable seems unlikely because the starvation experience occurred about 60

[18] Note however that the results remain similar if we control for the same variables as depicted in equation 3.1.

years prior to the Covid-19 pandemic. Having said that, as we describe below, several sensitivity tests are carried out to demonstrate that our finding is unlikely to be driven by such omitted variables bias.

Estimation by PSM-DID

We start with a propensity score matching (PSM) method, which matches similar individuals from the two groups based on the probability of being treated by an ESE, conditional on a set of observed characteristics. With the assumptions of selection on observables and common support,[19] the samples matched by PSM can be regarded as being randomly assigned into the two groups. Implementing the DID regression based on those matched samples thus can largely relieve concerns surrounding omitted variables bias.

The characteristics we use for matching the samples include age, gender, family class label, the number of siblings, marital status, residence, and province dummies. We calculate the propensity score (probability of being treated by ESE) based on a Probit model and then match the samples with a one-to-four nearest neighbour approach (calliper = 0.05). In the matching process, the samples are reweighted by the frequency with which they are used as a match. The following DID regression thus rests on the reweighted samples (those not matched are discarded[20]). The results based on the PSM-DID approach are presented in Table A3.3. It shows that the estimate of our key variable remains significantly positive and qualitatively similar to our baseline estimates (β = 0.072, p = 0.004).

The Oster (2019) evaluation

Oster (2019) developed an efficient method for evaluating the degree to which an OLS estimation is likely to be biased by omitted variables. The basic logic of her method is to compare the estimated coefficients of the key explanatory variable when a set of observed variables are controlled and not controlled, with the change in R-square being considered at the same time. Put simply, if the estimates remain stable when adding a

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[19] For a detailed introduction on this method and its assumptions, we refer readers to Caliendo and Kopeinig (2008). Although verifying the assumption of selection on observables is difficult, we can achieve the goal of testing the sensitivity of our finding to possible omitted variables bias once we can somewhat reduce such probabilities. The plots of common support condition before and after the matching can deliver an intuitive sense on how the matching reduces the probability of omitted variable bias. As shown in Figure A3.2, the distributions of propensity score of the two groups are much closer after matching, indicating a reduced probability of omitted variable bias.

[20] 4 observations from the treatment group and 984 from the control group are not matched, accounting for about 38% of the sample in total.

set of observed variables with relatively good explanatory power into the regression, one may assume that unobservable factors are unlikely to be able to significantly change the result. In practice, this method produces a ratio δ, which represents the explanatory power that the unobservable variables, relative to the observed variables, should have on the outcome for overturning an estimate (driving it to 0). A larger positive δ means that there is a smaller chance of suffering from omitted variables bias. Empirically, Oster (2019) suggests that there should be a small chance of omitted variables bias when δ is larger than 1.

We conduct this evaluation with the Stata command *psacalc* developed by Oster and the observed variables used here are exactly those used in the baseline regression (note that province trends and wave effects are also regarded as observed variables). Following the suggestion of Oster (2019), we assume the R-square with all unobservable and observed variables included, is 1.3 times that of the regression with the full observed variables. The results presented in Table A3.4 show that δ is 6.35, much larger than the critical value of 1. This means that, for overturning our finding, the unobservable variables should have at least 6 times the explanatory power of the observed variables on the outcome. This is very unlikely given the scale of current controls in the regression.

Permutation test

Another useful way for examining whether our finding is driven by omitted variables is to randomly rearrange the treatment among our sample. This is the so-called permutation test. Here we randomly rearrange the values of the variable *ESE* among our sample (keeping the same value for each individual in both 2018 and 2020) and then conduct the DID regression depicted by equation 3.1, based on the false treatment and control groups created. After repeating the randomization and estimation process 500 times, we obtain the distribution of these placebo estimates of β. As plotted in Figure A3.3, the placebo estimates concentrate around zero and our baseline estimate (vertical blue line) is larger than most of these estimates (p-value=0.060[21]). This would again support our suggestion that our finding is not driven by omitted variables.

[21] The null hypothesis is that the placebo estimate is larger than our baseline results.

Treatment by cohorts

Assuming that the group of individuals born before the famine will have experienced starvation to a greater degree than those born immediately after the famine, we now construct a new treatment and control group simply relying on people's birth years. Specifically, all individuals born between 1950 and 1957 (just before the famine) are assigned into the treatment group.[22] The control group now consists of individuals born between 1963 and 1966 and so just after the famine. The choice of a narrow cohort is to reduce potential bias that might be caused by generational differences. For example, it guarantees that both groups fully experienced the Cultural Revolution (1966 to 1976), a violent social unrest and do not experience the civil wars leading up to the founding of the People's Republic of China in 1949.

We run the same regression as depicted in equation 3.1, only with the variable *ESE* replaced with this new group setting. The results are presented in Table A3.5. It shows that individuals born before the famine outperform those born immediately after the famine when it comes to dealing with the effects of the pandemic for mental well-being (β = 0.164, p = 0.000). Indeed this estimate is significantly larger than that of our baseline estimate. This we speculate is likely due to measurement error in our baseline setting. As we discussed in Section 3.3, relying on self-reports has the advantage of minimising the potential for cohort bias, but is more likely to encompass measurement error which could cause a downward bias in our estimation.

3.5.4 Resilience or other individual differences

So far we have shown that our findings are unlikely to be biased by factors driving the assignment of ESEs, i.e. the causes of childhood ESEs. One however might contend that this is still not sufficient to claim that our finding is caused by different levels of resilience, instead, it could be driven by other individual differences resulting from having experienced a childhood ESE. Many studies have, for instance, shown that the famine could generate long-term impacts on various aspects of individuals' future life outcomes such as in the areas of labour and health. One thus may argue that our results could be driven by such individual differences rather than resilience.

To address this potential concern, we interact $Covid_{pt}$ with a number of individual characteristics (taking the value in 2018) that could reflect potential consequences of

[22] Following our main setting, here our treatment group also does not include samples born during the famine (1958 to 1962). Note however we can obtain qualitatively similar results if including those born during the famine into the treatment group.

childhood ESE in our model. This includes height, BMI, self-reported health status, residence, marital status, political status, employment status, education level, family per capita income and household size. Alternatively, we can also control for these factors (time-varying ones, without interacting with $Covid_{pt}$) directly in our regression. The results are presented in Table A3.6 and we observe very similar estimates to our baseline ones. This suggests that our findings cannot be explained by other individual differences.

3.5.5 Robustness checks

We also conduct several general robustness checks on our findings. All these results are reported in Table A3.7. First, we use alternative measures of mental well-being namely happiness and a depression score as our outcome variable. Second, we use the number of death cases as the measure of Covid-19 pandemic severity. Third, instead of leveraging the spatial variations in the pandemic severity across provinces, we simply use a dummy variable (1 for the 2020 wave and 0 for 2018 wave) to proxy the treatment effect of the pandemic. This means that we do not rely on differences in severity across provinces and thus can relieve any concerns that our results are driven by provincial confounders correlated with our measures of Covid-19 pandemic severity.

Fourth, our baseline analysis restricts the sample to individuals born in rural areas between 1950 and 1957. Now we remove this sample restriction and simply include all individuals who were asked to report any extreme starvation episode into our analysis, including those born before 1950 and all urban residents. This means that the treatment group now consists of all respondents (including those born during the famine) that report having an ESE during the famine period, irrespective of when or where they were born. Those who do not report any ESE are assigned into the control group. Fifth, instead of only using 2018 and 2020 waves in the baseline analysis, we also implement a test based on all waves of the CFPS (this means that all observations in 2010-2018 are equally considered as pre-pandemic samples). Sixth, in addition to the current province's trends, we also control for one's birth province's trends in the model. The results from all these checks are similar to our baseline estimate, suggesting that individuals who experienced an ESE in childhood exhibit stronger resilience in face of the Covid-19 pandemic.

3.6 Further analyses

3.6.1 Mortality selection or a resilience enhancing effect?

We interpret our findings as evidence that early life adversity can strengthen individual resilience. A competing explanation is that our finding is driven by a selection effect. The argument here would be that the famine may have acted as a natural screening process, where genetically stronger individuals survived (Gørgens et al. 2012). In other words, it is not that the famine nurtures individual resilience, but that they (survivors) were born with stronger resilience. In turn, our positive interaction effect (ESE × Covid) is driven by differences in innate characteristics between our treatment and control group, rather than differences in individual resilience nurtured by experience of early adversities.

We test to what extent mortality selection could be a factor explaining our results using a number of different strategies. We start with comparing how the offspring of our samples perform during the pandemic when it comes to mental well-being. The underlying logic is straightforward: if our finding is a selection effect, i.e. individuals with an experience of a childhood ESE are genetically more resilient, their children should also exhibit greater resilience as well. The genealogic code in the CFPS enables us to match our samples with their children who are also interviewed in the survey. We conduct a regression depicted by equation 3.1,[23] but this time focusing on the children of the original sample. The treatment group here consists of individuals whose parents (at least one of them) experienced extreme starvation during the CGF. The control group consists of individuals whose parents (both) did not report an ESE. We gathered 1,026 observations in total with 230 of them belonging to the treatment group. The results are presented in column 1 of Table A3.8. It shows that the coefficient of the key interaction term is negative and insignificant, and in the opposite direction to what would be expected if selection effects were a factor driving our results. This suggests that individuals with experience of an ESE are not genetically more resilient.

Second, we compare the resilience of cohorts born during the famine (1958 to 1962) to that of the cohorts born immediately before the famine. Our rationale is simply that

[23] Here we do not include interactions of *Covid* and siblings and family class label. For one, there are lots of missing values in the data on siblings (we will lose 40% of the sample in this test). This is because that a large part of the offspring studied here are included into the CFPS after the 2010 survey and are not provided with information on siblings. Second, these factors are not quite relevant when it comes to the offspring of our baseline samples, most of them were born after the China's reform and openness (1978). However we can get similar results if including them into the regression (with a smaller size of observations).

the mortality effects associated with the famine are likely to be more pronounced for infants born during the famine, as opposed to those born before. This is because malnutrition is associated with higher mortality in infants as compared to older children (World Health Organization 2023). As such, if mortality selection drives our results, we would expect to observe a larger treatment effect for those born during, as opposed to before the famine, as for this group mortality will be higher. The results in column 2 of Table A3.8 suggest that mortality selection is not a factor. The coefficient of our key interaction term here is not statistically significant and in the opposite direction to what would be expected if mortality selection was a factor.

While we can rule out differential rates of mortality as an explanation for higher resilience in the treatment group, we do not take a stance on the specific mechanism that gives rise to the resilience effects we observe. Prior literature has put forward a variety of traits such as skills when it comes to emotion regulation, self-esteem, locus of control, communication skills and even spirituality as important components in shaping individual resilience (e.g. Collishaw et al. 2007; Mineka and Zinbarg 2006; Tugade and Fredrickson 2007). Other factors of a biological nature, like the workings of stress response mechanisms (e.g. hormone secretion, Heim et al. 2002) and neural pathways have also been proposed. Our analysis is designed to test whether early life adversity affects the formation of mental resilience, not to tease apart the various psychological and biological mechanisms as to why that might be the case. What we do show is that early life adversity can have a resilience enhancing effect.

3.6.2 Can you have too much adversity?

A key argument in the steeling effect theory is that while some adversity can be resilience enhancing, excessive exposure to adversity might result in a deficiency in toughness. In other words, a curvilinear or even inverted U-shaped relationship may exist between the degree of adversity experienced in early life and future resilience. A challenge in examining this hypothesis is that there is no direct measure of ESE intensity in the CFPS. To overcome this limitation in the CFPS, we employ three different strategies, each with its own strengths and limitations, whereby we use measures which proxy for variation in ESE intensity.

Our first strategy is to capture ESE intensity with provincial excess death rates (EDR) observed during the famine. This has been commonly used as a measure of famine severity in related studies (e.g. Chen and Zhou 2007; Lin 1990; Meng et al.

2015).[24] Here we make what we argue is the reasonable assumption that the intensity of reported extreme starvation would be larger in areas with higher EDRs. We then add a triple interaction of the EDR and our key interaction term ($ESE_i \times Covid_{pt}$) into our model. If the steeling effect theory holds true, the triple interaction should attract a negative coefficient, as it would suggest that any resilience enhancing effect attenuates in the face of more adversity. This is what we observe (see Table A3.9, column 1).

We also note here that in keeping with the analysis in last subsection, this finding also supports our suggestion that differential rates of mortality do not explain why those exposed to starvation episodes show greater resilience to the shock of Covid-19 pandemic. This is because in areas with higher EDRs, if mortality selection was a factor, a larger proportion of people at the low end of the distribution of genetic resilience should perish. In turn, the triple interaction should attract a positive, not a negative coefficient.

Our second strategy for testing the steeling effect theory relies on the occurrence period of ESEs reported by respondents in the CFPS. Although the CFPS does not ask respondents about the frequency or intensity of ESEs during the reported period, it seems intuitive that a longer reported period is likely to be associated with more starvation experiences. We thus use the duration of the reported starvation period as a proxy of ESE intensity. To examine whether there is a non-linear relationship between ESE intensity and future resilience, we replace the dummy variable of *ESE* in our baseline specification with the reported duration (*Duration*) and add a triple interaction of the square of *Duration* and Covid-19 intensity. As our purpose is to examine whether experiencing more adversities is better than less when it comes to enhancing individual resilience, here we focus on samples who reported having an ESE (the treatment group in our baseline setting).[25] The results in Table A3.9 (column 2) show that the coefficient of the triple interaction is negative and statistically significant. This again aligns with the steeling effect theory that excessive exposure to adversity can diminish its resilience enhancing effects.

Our third strategy for testing the veracity of the steeling effect theory is inspired by the observation of a lower proportion of females appearing in the treatment group of our sample. Given the prevalent culture of son preference in China, a possible explanation for this gender gap is that a larger percentage of girls may have perished

[24] This data relating to provincial excess death rates comes from Lin and Yang (2000).
[25] This also ensures essential variation in the variable of ESE intensity as otherwise the control group (79% of our samples) will have a zero value for this variable.

due to worse treatment during the famine (Zhao and Reimondos 2012). Recognising this, we split the sample into two sub-groups by gender. Assuming that females were treated worse during the famine due to the culture of son-preference, one would expect a smaller coefficient in the female group if excessive adversity mitigates any resilience enhancing effect. The results in column 3 and 4 of Table A3.9 show that the estimate of β in the female group is much smaller (and statistically insignificant) than that of the male sub-group. This reaffirms our earlier findings that some adversity in early life helps foster resilience but too much may not. While each of these individual tests of the potential for non-linear effects have their limitations, taken together they suggest that while some early life adversity contributes positively to the development of resilience, too much adversity may not be beneficial.

3.7 Conclusion

The notion that ‘*what does not kill you makes you stronger*” has been believed by many often based on personal observations and anecdotes rather than empirical analysis. The expression that adversity builds character is frequently echoed by parents, schoolteachers and employers alike. The question we posed in this study is whether exposure to adversity truly enhances resilience. Using a difference-in-difference identification strategy, we find that individuals who experienced extreme starvation in their childhood suffer much less in terms of their mental well-being when exposed to the Covid-19 pandemic. This finding is robust to different metrics of mental well-being and cohort identification strategies. We also show this finding is not caused by differential rates of mortality between the treatment and control groups, namely a famine selection mechanism.

We also find suggestive evidence indicating a non-linear relationship between early childhood adversity and the formation of resilience. These findings are in line with the prediction of the steeling effect theory which suggests that moderate early life adversity can nurture individuals’ resilience, but excessive exposure might backfire. To the best of our knowledge, this is the first study providing a causal link between early life adversity and the development of resilience. Uniquely, we linked two exogenous shocks decades apart, namely starvation in childhood with a pandemic experienced in adulthood to see how experience coping with one stressor influenced individuals’ ability to manage another.

Studies in this area have largely indicated that early life adversities can lead to long-lasting negative consequences. However recent research suggests that these adversities may also be associated with some positive outcomes such as higher likelihood of becoming entrepreneurs (Cheng et al. 2021; Yi et al. 2022). A speculative explanation for these positive findings is that individuals' resilience was enhanced through their experience with past adversity. Our paper provides causal evidence supporting this argument. Note, however, that we do not intend to overlook the negative impacts of adverse experiences or praising hardship, because clearly it is tragic to each victim. Instead, we highlight human's ability of adapting to adverse environments and, importantly, such resilience may be strengthened during that adaption process. In short, whatever does not kill you may fortify you.

Appendices

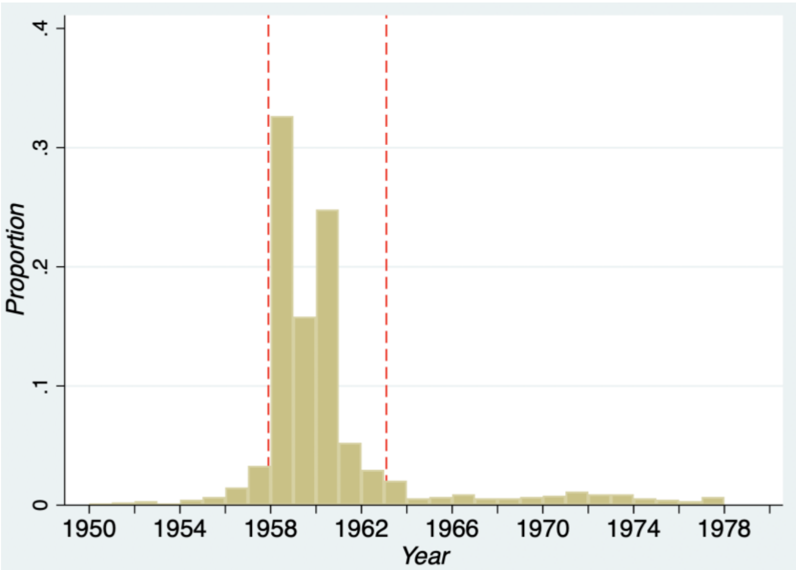
Table A3.1 Center for Epidemiologic Studies Depression Scale (CES-D) 8-item, NIMH

Below is a list of the ways you might have felt or behaved. Please tell me how often you have felt this way during the past week.

	During the past week			
	Rarely or none of the time (less than 1 day)	Some or a little of the time (1-2 days)	Occasionally or a moderate amount of time (3-4 days)	Most or all of the time (5-7 days)
1. I felt depressed	□	□	□	□
2. I felt that everything I did was an effort	□	□	□	□
3. My sleep was restless	□	□	□	□
4. I was happy	□	□	□	□
5. I felt lonely	□	□	□	□
6. I enjoyed life	□	□	□	□
7. I felt sad	□	□	□	□
8. I could not get "going."	□	□	□	□

Scoring: 1 for answers in the first column, 2 for answers in the second column, 3 for answers in the third column, 4 for answers in the fourth column. The scoring of positive items is reversed. Possible range of scores is 4 to 32, with the higher scores indicating the presence of more symptomatology.

Figure A3.1 The distribution of the start years of the reported ESE



Note. This figure is based on samples born in rural China between 1950 and 1957, in line with our baseline analysis. We only show the ESE happened between 1950 and 1978 in this figure. The vertical value is the proportion of frequency in each single year over the total number of reported ESE.

Table A3.2 The results of parallel trend test

VARIABLES	Dependent variable: Life satisfaction (1)
ESE × Covid (2020)	0.066**
	(0.029)
ESE × Covid (2016)	-0.005
	(0.035)
ESE × Covid (2014)	0.026
	(0.026)
ESE × Covid (2012)	0.013
	(0.029)
ESE × Covid (2010)	-0.049
	(0.045)
Individual FE	Yes
Wave FE	Yes
Province trends	Yes
Observations	9,631

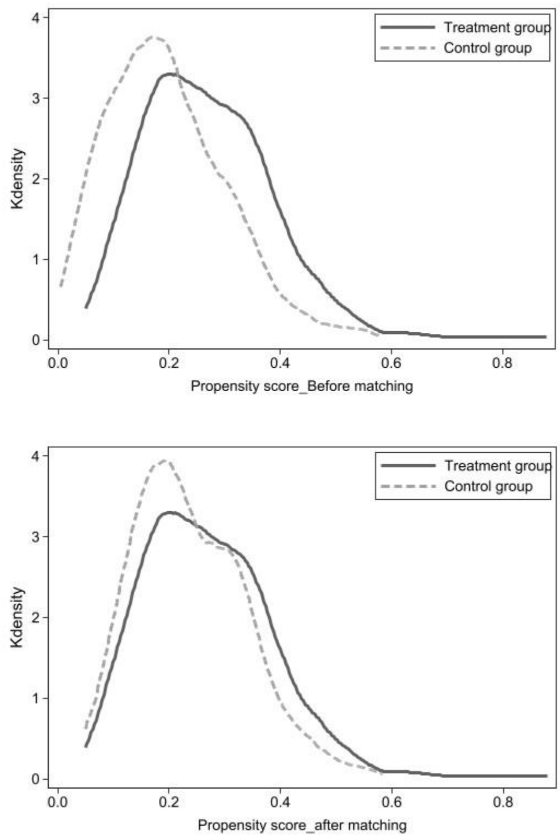
Note. Covid (k) equal to 0 except in the year k $(k = 2010, 2012, 2014, 2016, 2018\ and\ 2020)$ which takes the true value of Covid-19 infection rate in 2020. ESE × Covid (2018) is omitted for collinearity. Robust standard errors clustered at province-year level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A3.3 The results of PSM-DID estimation

VARIABLES	Dependent variable: Life satisfaction (1)
ESE × Covid	0.072***
	(0.024)
Controls	Yes
Individual FE	Yes
Wave FE	Yes
Province trends	Yes
R-squared	0.047
Observations	2,980

Note. The propensity score based on a Probit model and the matching process is carried out with a one-to-two nearest neighbour approach (calliper = 0.05). In the following DID estimation, we weight the samples with the frequency with which each sample is used in the matching process (individuals not matched are discarded). The observations shown in the column is the number after weighting. Controls are the same as equation 3.1 depicted. Robust standard errors clustered at province-year level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure A3.2 Common support condition before and after PSM



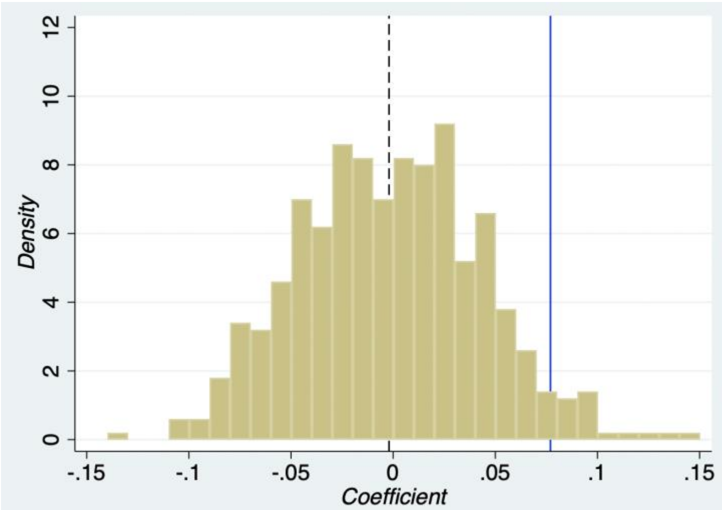
Note. The matching is based on a one-to-four nearest neighbour approach (calliper=0.05), with propensity score being calculated by a Probit model. The left graph covers all samples before the matching, the right graph only includes samples that are matched.

Table A3.4 The Oster (2019) evaluation

Dependent variable:	Estimate without controls ($\dot{R^2}$)	Estimate with full controls ($\widetilde{R^2}$)	δ for β=0 (R^2max=1.3*$\widetilde{R^2}$))
	(1)	(2)	(3)
Life satisfaction	0.084 (0.003)	0.077 (0.025)	6.35

Note. In column 1, there is no any controls except for individual fixed effect. In column 2, controls are exactly depicted in equation 3.1. This evaluation is carried out by Stata command *psacalc* developed by Oster. As suggested by Oster (2019), we assume the R^2 (max) with unobservable and observed variables included, is 1.3 times that of the regression with the full observed variables.

Figure A3.3 Permutation test



Note. The DID regression is the same as our baseline analysis as depicted by equation 3.1, vertical value is the p-values which are calculated based on standard errors clustered at province-year level. The solid vertical line denotes our baseline estimate.

Table A3.5 The results of treatment by cohorts

VARIABLES	Dependent variable: Life satisfaction (1)
ESE × Covid	0.164***
	(0.040)
Controls	Yes
Individual FE	Yes
Wave FE	Yes
Province trends	Yes
R-squared	0.014
Observations	4,928

Note. Here *ESE* equals 1 if a person was born between 1950-57, 0 if 1963-66. Controls are the same as the equation 3.1 depicted. Robust standard errors clustered at province-year level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A3.6 Tests on channels rather than mental resilience

VARIABLES	Control for interactions of *Covid* and life outcomes (1)	Control for time-varying life outcomes (2)
ESE × Covid	0.059***	0.072***
	(0.020)	(0.022)
Controls	Yes	Yes
Individual FE	Yes	Yes
Wave FE	Yes	Yes
Province trends	Yes	Yes
R-squared	0.033	0.040
Observations	2,706	2,732

Note. Controls depicted in equation 3.1 apply here as well. In column 1, the individuals characteristics used for interacting with *Covid* include height, BMI, self-reported health status, residence, marital status, political status, employment status, education level, family per capita income (log-linearised) and household size. In column 2, the time-varying life outcomes include family per capita income (log-linearised), BMI, self-reported health status and employment status. Robust standard errors clustered at province-year level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A3.7 Robustness checks

VARIABLES	Outcome variable: Happiness (1)	Outcome variable: Depression score (2)	Measure *Covid* with death cases (3)	Measure *Covid* with dummy variable (4)	All samples born before 1979 (5)	CFPS 2010-2018 (6)	Control for birth province trends (7)	Restrict ESE to 1958-1962 (8)
ESE × Covid	0.163**	-0.281**	0.006**	0.087*	0.022***	0.075***	0.080***	0.116***
	(0.061)	(0.119)	(0.002)	(0.044)	(0.007)	(0.025)	(0.021)	(0.040)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared (Log-likelihood)	0.024	0.042	0.027	0.026	0.009	0.160	0.033	0.026
Observations	2,835	2,835	2,856	2,856	16,484	9,409	2,854	2,586

Note. Controls are the same as equation 3.1 depicted. Robust standard errors clustered at province-year level in parentheses. In column 6, we also try to cluster at birth province level and both birth and current provinces level, the results are similar. *** p<0.01, ** p<0.05, * p<0.1.

Table A3.8 Tests on selection effect

VARIABLES	Offspring Test (1)	Infants exposure (2)
ESE × Covid	-0.050	0.037
	(0.046)	(0.033)
Controls	Yes	Yes
Individual FE	Yes	Yes
Wave FE	Yes	Yes
Province trends	Yes	Yes
R-squared	0.054	0.018
Observations	1,036	4,408

Note. Controls are the same as equation 1.1 depicted. In column 1, the controls do not include interactions of *Covid* and siblings and family class label. In column 2, *ESE* equals 0 if a person was born between 1958 and 1962, 0 if 1950 and 1957. Robust standard errors clustered at province-year level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A3.9 Further analysis: Suggestive evidence on steeling effect theory

	Excess death rate (EDR)	ESE duration	Gender difference	
			Female	Male
VARIABLES	(1)	(2)	(3)	(4)
ESE × Covid	0.139***		0.033	0.112***
	(0.034)		(0.045)	(0.028)
ESE × Covid × EDR	-0.016***			
	(0.005)			
Duration× Covid		0.856***		
		(0.287)		
Duration× Duration× Covid		-0.139**		
		(0.064)		
Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Province trends	Yes	Yes	Yes	Yes
R-squared	0.027	0.027	0.038	0.066
Observations	2,856	302	1,444	1,412

Note. Controls are the same as equation 3.1 depicted. In column2, the samples only consist of individuals who report having ESE strictly within the period of CGF. Robust standard errors clustered at province-year level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Conclusion

With three independent empirical studies focused on China, my thesis adds new insights into several ongoing economic debates from the perspective of happiness economics. The first chapter sheds light on the relationship between the disclosure of anti-corruption investigations on high-ranking public officials and individual life satisfaction. Although it is established that corruption harms subjective well-being, I hypothesised that anti-corruption policies or efforts may not necessarily lead to a happier society. This is what I found: the publicity of anti-corruption investigations has generated a modest negative impact on life satisfaction at the population-level. Perhaps most importantly this effect is found to be significantly moderated by the levels of one's political trust. Specifically, the disclosure of information surrounding anti-corruption investigations appears detrimental to the life satisfaction of individuals with low political trust, whereas it appears to be well-being enhancing for those with high political trust.

I discuss these findings within a framework of signalling theory. Although the government seeks to signal its efforts on cracking down corruption by publicizing anti-corruption investigations, the signal may be received in two opposing ways. Drawing on the theory of confirmation bias, I propose that people with high trust in the government would interpret the information more positively (i.e. a positive signal), while those with low trust in the government may read the information in a negative way (i.e. a negative signal). A key implication of this study is that if faced with low trust, policy-makers may be disincentivised from tackling certain social issues if doing so risks making those issues more salient among the public. More broadly, this chapter highlights the importance of political trust when it comes to predicting how people will interpret government actions.

One promising direction for future research that can be drawn from this study could be to examine whether similar effects exist for other government policies, such asenvironmental or food safety inspections that might also heighten public awareness of those issues. By highlighting the importance of political trust, this chapter I hope also encourages future studies on the determinants of citizens' political trust, particularly in authoritarian regimes. Indeed, the second chapter of my thesis was motivated in part by this.

The second chapter examines the relationship between individual life satisfaction and political trust. Using three independent IVs for life satisfaction, I show that it has a strong positive impact on political trust. This finding can be explained within the framework of social contract theory. Simply put, I assume there is an intangible contract between the government and its citizens imposing mutual obligations on both sides. Citizens will commit to supporting the government, but only if they are satisfied with their lives. I also provide evidence suggesting that citizens' political trust is predictive of individuals stated intention to engage in protests in China. Taken together, an important implication of this study is that the government, even in authoritarian regimes, will have an incentive to improve the subjective well-being of the general public, as not doing so may undermine political trust and hence regime stability. Furthermore, these findings also add to the emerging literature that use happiness to explain various individual level outcomes. Particularly, my study highlights the importance of life satisfaction in determining political trust, whereas previous studies have focused on the reverse. A good avenue for future work could be to explore whether subjective well-being has an impact on other government-related individual behaviours such as tax evasion or engagement in corrupt working practices.

The final chapter studies whether early life adversity can promote individual resilience. Employing a difference-in-difference method, I find that people who experienced extreme starvation exhibited significantly greater resilience in mental well-being when faced with the Covid-19 pandemic. I also exclude the possibility that this finding is driven by a mortality selection effect (i.e. survivors of the famine are genetically more resilient). This lends support to the popular notion of '*what does not kill you makes you stronger*'. Furthermore, I provide suggestive evidence of a non-linear relationship between the intensity of early life adversity and later resilience. This aligns well with steeling effect theory which argues that moderate early life adversity is beneficial to the development of resilience, but excessive exposure might offset or even reverse the effect.

Regarding avenues for future study, one direction in this field could be to more cleanly identify the turning point in the non-linear relationship between early life adversity and resilience, that is, the level of adversity at which its positive impact on resilience begins to diminish. This of course yields high demand on the data. In addition, one can also explore the role of adversity in early life for determining resilience in different study contexts such as the financial crisis.

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