Manufacturing Obedience: Entertainment Media and Authoritarian Support *

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Abstract

How does entertainment media manufacture political support in authoritarian states? Leveraging a nationwide television (TV) subsidy program in rural China, we find that adolescents with greater formative-era exposure to color TV later exhibit higher regime support, increased political trust, and lower anti-authoritarian resistance. These are attributed to the *illusory accountability effect*: adolescents with increased TV exposure display lower cognitive performance, educational attainment, and political understanding, while experiencing higher emotional satisfaction from entertainment. This combination of impaired critical capacity and heightened low-cost affective satisfaction leads to lower demands for government accountability and fewer protests.

Keywords: Mass Media; Political Support; Illusory Accountability; Entertainment.

JEL Codes: L82, O10, P35, I10

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"Empty their minds, fill their bellies, dim their ambitions, and strengthen their sinews."
— Lao Tse, Ancient Chinese Philosopher.

"Bread and circuses are the two tools for the appeasement of the masses."
— Juvenal, Ancient Roman Poet.

1 Introduction

Mass media plays a central role in shaping citizens' political preferences and behaviors. A substantial body of evidence documents how media influences voting outcomes, trust in democracy, and political participation (DellaVigna and Kaplan, 2007; Gentzkow, Shapiro and Sinkinson, 2011; Durante, Pinotti and Tesei, 2019; Zhuravskaya, Petrova and Enikolopov, 2020). Most of this literature examines political effects of media-party linkages in democracies or propaganda in authoritarian regimes. By contrast, the political impact of apolitical entertainment media remains understudied. In this paper, we investigate how *early-life exposure* to entertainment media bolsters authoritarian support and reduces demands for government accountability. Importantly, we find formative age entertainment exposure increased subjective evaluations of the government even without objective improvements in government performance. The lowered demand for accountability silences political activism and contributes to the long-term stability of authoritarian states.

We refer to this mechanism as the *illusory accountability effect* of mass media, which extends the existing literature in three ways. First, we highlight how *apolitical entertainment* media, rather than political propaganda or biased news, shapes political behavior.[1] We find entertainment media hampers citizens' understanding of politics and diminishes their capacity for political participation. Meanwhile, it fosters a low-cost and accessible form of public satisfaction and dampens the inclination to challenge the status-quo. This reduces the demand for political accountability and stabilizes authoritarian rule. Second, instead of focusing on authoritarian indoctrination or political campaigns (Pop-Eleches and Tucker, 2017; Cantoni et al., 2017), we emphasize that exposure to everyday entertainment media during the formative years shapes political values in the long run. By tracking cohorts over a decade, we document a sharp change in the trajectory of citizens' cognitive and political development. Third, rather than emphasizing how states co-opt the populace by providing public goods from the *supply-side*, we focus on how media exposure subtly alters people's ability and willingness to hold the government accountable from the *demand-side*. Such an "opium for the masses" role offers a low-cost, low-risk strategy to generate public support, complementing traditional instruments of strategic propaganda and media manipulation in sustaining durable authoritarianism (Durante, Pinotti and Tesei, 2019; Szeidl and Szucs, 2021).

1. Existing studies have focused on political media, including direct propaganda (Adena et al., 2015; Huang, 2018; Qin et al., 2018; Chen and Yang, 2019), strategic censorship (King et al., 2013, 2017; Lorentzen, 2014), selective attribution (Rozenas and Stukal, 2019), and soft propaganda (Mattingly and Yao, 2022).

We focus on the "Home Appliances Going to the Countryside" (HAGC) program, which subsidized color television purchases for rural households. The HAGC was first implemented in 2007 across three pilot provinces before being gradually expanded nationwide in subsequent years. Rural residents, previously constrained by severe financial limitations, were particularly affected by the policy and thereby incentivized to purchase TVs.[2] Thus, HAGC policy triggered a massive technological upgrade from black-and-white (B&W) to color TV in rural areas. While B&W TVs existed prior to the policy, their low audiovisual quality failed to capture significant attention. The vivid, high-fidelity experience drastically increased the attractiveness of color TV, thereby attracting rural teenagers to modern forms of entertainment, increasing their consumption of entertainment content. Despite political news or propaganda, Chinese people predominantly consume entertainment content on TV (see Figure A1). This feature accords with our focus on the political implications of entertainment media.

Using policy pilot data from China's Department of Commerce and various nationally representative micro-survey datasets, we employ a generalized difference-in-differences (DID) design to estimate the long-term effects of television exposure. Our identification strategy exploits two sources of variation: (i) cross-county variation in policy roll-out timing, comparing treated and untreated counties across years, and (ii) cohort-level variation, contrasting adolescents aged 7-18 at policy introduction with older cohorts aged over 19.[3] To establish that TV-induced support results from long-term cognitive changes rather than short-term economic benefits, we use survey outcomes eight years after policy implementation (2016-2020 waves), while data from earlier waves (2010-2014) serve as placebos. Since the policy only targeted on rural areas, urban samples also serve as a natural comparison group, enabling a triple-difference design that helps rule out other county-specific cohort trends.

Before turning to our main results, we first establish that the HAGC policy effectively increased exposure to entertainment TV. Specifically, we find that HAGC policy primarily expanded access to color TVs, with little effect on other appliances, and this technological upgrade translated into greater TV viewing among adolescents during their formative years. Importantly, the increase in media exposure was driven predominantly by entertainment programming and was accompanied by a reallocation of time away from study toward watching TV, with little evidence of corresponding changes in political news consumption or other informational content.

In our baseline specification, we find a strong positive influence of TV on authoritarian government support in rural samples. Compared to their older counterparts who missed TV exposure in

2. The HAGC program offers four key advantages. First, its large scale, affecting 600,000 administrative villages and 600 million rural residents. Second, the staggered roll-out and explicit targeting in rural area offer significant regional variation. Third, aiming at stimulating consumption and thereby sustaining economic growth during the 2008 financial crisis, the program would not strategically select certain regions, minimizing selection bias in policy implementation. Finally, the centrally planned nature of the program minimizes the risk of local government of non-compliance.

3. We focus on adolescents (ages below 18), a period in which values and norms remain highly malleable. In contrast, political attitudes, values, and beliefs are more established for adults.

their adolescence, exposed adolescents develop higher government support after reaching adulthood, with an average increase of 0.230 (6.8% of sample mean). The effect is appears only for the 2016-2020 waves, which supports the channel of long-term cognitive changes rather than short-term economic benefits. Using a binary setting, we find that TV contributes to a 10% transition from low support to high support.

We then examine the heterogeneous effects by age at first exposure to the policy. The younger the individual was, the longer she would be exposed to TV, the stronger support she developed. Accordingly, we document stronger government support for cohorts that were exposed earlier to the program. As the policy targeted rural areas, we observe insignificant effects among urban residents. To isolate cohort-specific impacts from factors such as political trends, economic momentum, or technological progress, we also employ a triple-difference (DDD) specification comparing rural and urban individuals, with the findings consistent with the baseline.

We then turn to the policy's broader impact on affected cohorts. Generally, viewers with greater TV exposure report higher satisfaction with the government on political issues, holding government actions constant within the same counties. This suggests that increased satisfaction arises from lowered subjective standards rather than actual improvements. Additionally, these viewers exhibit higher political trust without changes in general trust, indicating that our effects result from reduced standards *toward the government* rather than shifts in general values. As for political behavior, TV exposure predicts fewer protests and conflicts with officials, which may contribute to authoritarian durability.

We conduct a battery of robustness checks, including using alternative treatment definitions, excluding the influence of other contemporaneous policies, and employing different model specifications, with consistent results. Further, with detailed data on TV ownership and program coverage, we show that the policy's effect is only at play for households that acquired a TV after the policy, and is significantly amplified in regions with a higher share of entertainment programming. These results further show that the impact of the HAGC policy on youth political values and behavior is primarily driven by increased access to entertainment TV.

To further validate our findings and explore the mechanisms through which TV influenced viewers, we combine HAGC policy data with individual-level information on cognitive abilities, emotions, and welfare. We find that affected cohorts (adolescents) exhibit worse cognitive abilities compared to less-exposed cohorts. This cognitive decline leads to lower educational achievements: viewers are less likely to be admitted to college, which may result in political ignorance, lowering their *ability* to protest. Meanwhile, we find that viewers report higher levels of positive emotions and lower levels of depression, conditioning on similar objective conditions. We also document lower demands for redistribution and democratic reforms. This pattern implies lower *willingness* to challenge the authority. Lastly, We provide suggestive evidence that exposure may also weaken one's social connectedness, which is essential to acquiring diversified social-political

information and establishing civic social capital. Together, these channels lead to reduced political participation, lower political accountability, and stronger authoritarian obedience, which helps explain increasing political support in our main analysis.

Finally, we rule out several alternative explanations. First, we exclude the propaganda channel of TV. Specifically, we find no significant increase in political content on screen following the implementation of HAGC. Moreover, expanded TV access primarily increased entertainment consumption while reducing political news viewership. Second, while the policy may have temporarily eased household burdens through government subsidies, we find no evidence that this drove long-term support for the government. Third, while TV might provide more employment information and potentially improve future job prospects, we show that the affected cohort did not have better objective living conditions or material well-being as adults. Thus, the observed change is consistent with a perception-based mechanism, supporting the *illusory accountability effect*.

Literature. This study primarily contributes to the literature on the political role of mass media. In democratic context, prior works show that media can affect resource allocation (Strömberg, 2004), shift voter preferences and election outcomes (DellaVigna and Kaplan, 2007; Ellingsen and Hernæs, 2018; Szeidl and Szucs, 2021; Manacorda et al., forthcoming), fuel inter-group divisions (Bursztyn et al., 2023), reshape ideology and polarization (Campante and Hojman, 2013; Martin and Yurukoglu, 2017; Levy, 2021; Kim and Patterson Jr, 2024), alter political accountability in law enforcement and crime (Besley and Prat, 2006; Mastrorocco and Ornaghi, 2025), and shape long-term political participation (Gentzkow, 2006; Olken, 2009; Gentzkow et al., 2011). In authoritarian regimes, leaders control media through direct propaganda (Huang, 2018; Qin, Strömberg and Wu, 2018; Chen and Yang, 2019), censorship (King, Pan and Roberts, 2013, 2017; Lorentzen, 2014), selective attribution (Rozenas and Stukal, 2019), and softer forms of persuasion (Mattingly and Yao, 2022; Mattingly et al., 2024).[4] We depart from the literature on politically oriented media and show that apolitical, entertainment TV *per se* can have persistent effects on political behavior and government accountability.

Our study extends the literature on entertainment technologies. Studies have discussed how such technologies increase the value of leisure, reducing both fertility (Ferrara et al., 2012) and labor supply (Aguiar, Bils, Charles and Hurst, 2021; Fenton and Koenig, 2025), how entertainment may exacerbate racial hate (Ang, 2023), how entertainment content substitute for political information (DellaVigna and La Ferrara, 2015; Campante et al., 2022), and how it can alter the political landscape (Durante, Pinotti and Tesei, 2019; Kim, 2023; Kim and Patterson Jr, 2024). Some stud-

4. Mass media need not serve authoritarian rule: Independent outlets in Russia increased opposition support (Enikolopov, Petrova and Zhuravskaya, 2011); modern media technologies decreased government support (Guriev, Melnikov and Zhuravskaya, 2021) and facilitated mass mobilization in North Africa, Russia, and China (Manacorda and Tesei, 2020; Enikolopov, Makarin and Petrova, 2020; Qin, Strömberg and Wu, forthcoming). As illustrated by Adena, Enikolopov, Petrova, Santarosa and Zhuravskaya (2015), radio under democracy constrained Nazi popularity but under authoritarianism fueled Nazi mobilization. Thus, the effect depends on who controls the use of media.

ies note how entertainment media diverts public attention and promote propaganda effectiveness (see e.g., Baum and Jamison (2006); Kern and Hainmueller (2017); Horz (2021)). Most relevant, Durante, Pinotti and Tesei (2019) find that entertainment TV could bias voters' preferences and generate support for populist parties in the context of Italy under Berlusconi's rule. Our contribution departs from this literature in three aspects. First, we focus on the long-term consequences of *early-life* exposure to entertainment TV. We show that early exposure crowded out educational activities, lowered cognitive skills and political understanding, but improved emotional well-being, thereby increasing support for authoritarian rule, reducing willingness to protest, and weakening demands for accountability.[5] Second, unlike in democratic contexts, we show that entertainment media in an authoritarian setting can sustain regime stability by weakening both the capacity and the motivation to hold the state accountable, rising citizens' satisfaction with the status quo, what we term *illusory accountability*. Third, using uniquely detailed data on program viewership and broadcast shares by genre, we document that the effects scale with exposure to entertainment rather than news or propaganda content. This pattern is consistent with an "opium-style" distraction mechanism (shaping behavior through entertainment rather than political persuasion) that generates *illusory accountability* and reinforces authoritarian rule.[6] Finally, we distinguish between objective performance (e.g., economic and environmental conditions) and subjective evaluations (e.g., human rights and law enforcement fairness). We find that entertainment exposure has little effect on the former but significantly increases the latter, suggesting that its impact operates through perceptions rather than underlying outcomes.

Our study contributes to scholarship on political accountability by shifting attention from *supply-side* to *demand-side* mechanisms. Prevailing accounts emphasize the *supply* of accountability with information provision and sanction in mass media, arguing that media-party linkages weaken citizens' capacity to hold officials to account (Gentzkow, Shapiro and Sinkinson, 2011; Szeidl and Szucs, 2021). Related studies highlight regime strategies that cultivate acquiescence through narratives of meritocratic selection and perceived upward mobility, as well as media frames that foster optimism (Herman and Chomsky, 2021; Liu, 2023; Kim, 2023; Kim and Patterson Jr, 2024). We advance a distinct *demand-side* mechanism: even apolitical entertainment media can reduce citizens' willingness to demand accountability by satisfying affective needs, diverting attention from public affairs, and crowding out critical reflection and civic engagement. Political support rises without changes to socioeconomic or institutional fundamentals, and *subjective support* helps relax pressure for *objective accountability*.

5. This mechanism is consistent with evidence from medicine, psychology, and education on media exposure and cognition (Hancox et al., 2005; of Pediatrics, 2001; Landhuis et al., 2007; Ennemoser and Schneider, 2007), as well as research on habit formation and self-control in digital consumption (Allcott, Gentzkow and Song, 2022).

6. Similarly, Kern and Hainmueller (2017) document an "opium-style" distraction effect of West German television, which raised life satisfaction and political trust among East Germans and thereby reinforced support for the East German regime. They highlight the unintended stabilizing effects of foreign media, whereas our analysis centers on how domestic entertainment media contributes to authoritarian rule.

The remainder of the paper proceeds as follows. Section 2 describes the evolution of television access in rural China and the HAGC program. Section 3 introduces the data. Section 4 presents the empirical strategy and key identification assumptions. Section 5 reports the main results and robustness checks. In Section 6, we explore main mechanisms for the *illusory accountability effect* and exclude alternative explanations. Section 7 concludes.

2 Background

2.1 Media Coverage and Entertainment in Rural China

At the time of its founding in 1949, rural China had no access to radio broadcasts, let alone television. The first television set in China was produced in 1958, which spread gradually among households. By 1978, China had a total of 105 radio stations and 37 TV stations, with the national radio coverage reaching 62% and TV coverage reaching 36% of the population.[7] The reform period since 1978 witnessed tremendous development in media coverage. By 2000, radio broadcast and TV coverage became common in rural China (for radio: 90.4%, for TV: 91.6%). Nonetheless, TV did not become a common source of entertainment for villagers before the HAGC policy due to two reasons.

First, TV signal coverage does not equate to household ownership. Many rural households could not own a TV due to its relatively high price. In 2007, the average annual income for a rural resident was 4040.36 *yuan*, with wage income being only 1596.22 *yuan* . In sharp contrast, a single color TV cost 2000-4000 *yuan* , which means purchasing a TV required nearly an entire year's income, excluding other living expenses.[8] This leads to highly unbalanced ownership pattern between rural and urban areas in China. According to the *China Statistical Yearbook*, by the end of 2000, 100 rural households owned an average of 48.74 TV sets, compared to 116.6 sets in urban households. Considering that some households may have multiple TV sets, the average number of TV listed here is just an upper bound for the actual TV ownership status.

Second, the pre-HAGC television landscape was dominated by black-and-white (B&W) TV sets with low audiovisual quality. Visual clarity, audio quality, and fluency are central to the entertainment value of audiovisual media, and their absence substantially weakens TV's recreational value. Moreover, the broadcast channels and programming diversity were limited (Stockmann and Gallagher, 2011). As a result, despite gradual improvements in signal coverage, TV remained a limited source of entertainment for many rural residents. Instead, they engaged more in card games, chess, and mahjong, typically organized within villages or among kinship networks.

7. See Chinese State Media: Renmin Net for details.

8. We focus on LCD TV (Liquid Crystal Display TV) that took the largest share in Chinese market. Without special reference, the money unit for income/wage/price is *yuan* (RMB).

2.2 The HAGC Policy

In response to the global financial crisis and the ensuing economic slowdown in late 2007, the Chinese government introduced proactive fiscal measures, with the "Home Appliances Going to the Countryside" (HAGC) initiative playing a central role. Launched in December 2007, the HAGC pilot began in three provinces: Shandong, Henan, and Sichuan, lasting four years. Rural consumers in these regions received a 13% subsidy when purchasing color TVs. The policy was later expanded in two phases in 2008 and 2009 to include more counties (see Figure A2).

The policy significantly alleviated the financial burden of rural residents of purchasing TVs. In addition to providing a 13% subsidy on related products, the program set a price ceiling of 2,000 yuan for color TVs. By the end of October 2008, over 3.5 million subsidized HAGC products were sold in the three provinces. Compared with the same period in the previous year, the program led to a 30 p.p. increase in home appliance sales, totaling approximately 4 billion yuan.[9] Within our datasets, we also verify that the policy brought about significant increase in TV ownership (see Table A2 and Table A3).

It is important to note that the policy did not prioritize specific socioeconomic traits, nor did it intend to enhance political control. For the pilot provinces, some are relatively developed like Shandong, while some lie in the less developed West China (Sichuan). More importantly, its expansion was so fast as to boost TV consumption. According to the Ministry of Finance (MOF), the policy sought to foster consumption, thereby stimulating economic growth and alleviating challenges faced by the home appliance and related industries.[10] The 30 p.p. increase in TV sales just confirms the success of their original goal.

The HAGC policy not only made TVs affordable but facilitated a wholesale upgrade from black-and-white to color technology for rural communities. As shown in *the 2012 China Statistical Yearbook*, between 2000 and 2010, the number of color TV sets per 100 rural households rose sharply from 48 to 112, while B&W sets collapsed from 52 to only 6.[11] Figure A3 documents rural residents purchasing TV and watching TV programs under the HAGC policy. Figure A4 traces the transformation in the main characteristics of TV sold in pre- and post-HAGC policy implementation. Specifically, it shows a marked shift in rural TV markets from bulky and cable-oriented models to more advanced, colorful, digital, and flat-screen TVs. The shift to color screens, coupled with improved access to diverse channels (Figure A1), transformed TV from a boring information source into a dazzling vehicle of entertainment. This drastic enhancement in audiovisual appeal was particularly effective in capturing the attention of adolescents, significantly increasing their daily exposure to entertainment content (see Appendix D).

9. See the official website for the central government of PRC for detailed data.

10. Zeng Xiaoan, then deputy director the MOF of China, publicly claimed "HAGC can address excess production capacity in China's domestic home-appliance market to help its factories survive international trade frictions.", see the official website for the central government of PRC.

11. See Table 10-30 of the 2012 China Statistical Yearbook for details.

Overall, the HAGC program offers four key advantages for our analysis. First, its large scale: covering all mainland provincial-level regions, it reached 600,000 administrative villages and 600 million rural residents, significantly improving the daily lives of rural households that previously lacked access to TV. Second, its staggered rollout: the program was implemented in three phases, starting with a pilot in three provinces and expanding nationwide. This variation allows us to estimate its impact more effectively. Moreover, the policy's exclusive focus on rural areas allows us to use urban households as a placebo or control group, effectively addressing cohort trends through a comparative analysis. Third, the program was designed to foster TV consumption and drive economic growth, rather than favoring underdeveloped regions or areas with political significance (e.g., autonomous regions), suggesting little selection bias.[12] Finally, the program was centrally planned and uniformly executed, with subsidies primarily funded by the central government (80%) and provincial governments (20%). This centralized structure minimizes the risk of local government distortions, making the program relatively exogenous to local conditions.

3 Data and Descriptive Evidence

This paper uses nationally representative panel data matched with county-level HAGC implementation timelines to examine how TV coverage from the HAGC program impacts political attitudes, behavior, and authoritarian stability. We identify how individuals were affected by the HAGC policy based on their household registration (*hukou*) type and residence during the HAGC implementation period rather than that in the survey year. We focus on individuals with a rural *hukou* as the program targeted rural areas. Urban samples are used for placebo tests and controls in the triple-difference specification.

3.1 TV Ownership and Programs

CHNS The China Health and Nutrition Survey (CHNS) is one of the longest and most comprehensive longitudinal health surveys in China, consisting of more than 4000 households and 15,000 individuals. Between 2004 and 2009, it includes questions on ownership, quantity, and monetary value of both color TV and black and white TV for each household, which facilitates our analysis on the effectiveness of the HAGC policy.

NFP The National Fixed Points data (NFP) is conducted by the China Ministry of Agriculture Research Center of Rural Economy and has been extensively utilized in economics studies (Chari et al., 2021). It provides comprehensive data on household-level ownership of various home appliances, which complements the CHNS and helps verify the effectiveness of the HAGC policy.

12. The MOF and the Ministry of Commerce also consulted extensively on logistics and support structures to ensure the policy reached rural consumers effectively to stimulate their consumption, see Press Conference of the MOF.

China TV Rating Yearbooks The CSM Media Research is a cooperative enterprise mainly established by CTR Market Research and Kantar Media Dedicated to TV & radio audience measurement research.[13] Since 2013, the CSM has published the *China TV Rating Yearbooks* on annual basis, with comprehensive information on China's TV market. Based on their TV program data, we manually collected and calculated the provincial-level broadcast shares of different TV programs to compare changes in *entertainment programs* and other programs (like political news).

Graphical Evidence We posit that the HAGC program increased the consumption of entertainment programs, thereby shaping the political values and actions of the populace. We aggregate the data from *China TV Rating Yearbooks* at the province-year-category level and plot the averaged values for each program category. Panel A of Figure A1 shows that entertainment programs enjoy a significantly higher viewership rate than news. This indicates that the populace would be prone to the long-term effect of entertainment programs. We will further test these in subsequent analyses.

3.2 Political Attitudes and Behaviors

CFPS The China Family Panel Studies (CFPS) is a nationally representative survey covering 25 Chinese provinces, conducted biannually by the Institute of Social Science Survey at Peking University. We construct three groups of dependent variables from CFPS. First, CFPS collects data on respondents' satisfaction with local government performance (rated on a 5-point scale), their trust in government officials (rated on a 10-point scale), whether they had conflicts with government officials in the past year, and whether they engage in online political discussions. These variables serve as key dependent variables for the baseline. Second, we employ data on cognitive ability, depression, and educational achievement to explore the underlying mechanisms. We use verbal and mathematical reasoning tests from CFPS as indicators of cognitive ability. For measuring depression, we start with the CES-D indicator, a self-reported scale where higher scores indicate a greater tendency toward depression. CFPS provides measurements of individual educational levels, which we use to define educational achievement. Third, CFPS collects a series of labor market outcomes, allowing us to examine possible welfare improvements of HAGC policy. Table A1 summarizes the descriptive statistics for these variables.

CSS The Chinese Social Survey (CSS) is a nationwide, large-scale survey launched by the Chinese Academy of Social Sciences. It includes a wide range of questions about political engagement, covering both political actions, like collective actions and protests, and political attitudes. The survey asks respondents to rate the government's performance in various issues (e.g., healthcare, corruption), their trust in the government, and whether they have participated in petitions

13. See https://www.csm.com.cn/en/.

or protests. It also captures daily political participation like involvement in political discussions or reporting social issues to the media, among other behaviors.

CGSS The Chinese General Social Survey (CGSS) is the earliest nationally representative survey in China, jointly launched by the Department of Sociology at Renmin University and the Survey Research Center at Hong Kong University of Science and Technology. CGSS provides a series of variables concerning time allocation and mass media use, which helps us to check the substitution effect of TV for other activities in the short run. We also focus on a broad range of questions on political participation and political knowledge and include questions related to educational achievement (e.g., college entrance).

ISCIU To capture details on media use and TV watching habits, we employ data from the Ideology Survey of Chinese Internet Users (ISCIU), obtained from Ma and Wang (2015). This survey is commonly used to study the ideological leanings of Chinese netizens. Notably, ISCIU includes questions on the frequency of TV watching and the types of programs watched, providing more detailed insights into TV usage. As supplementary evidence, we also employ indices to measure individuals' general political knowledge, compliance with the government, willingness to appeal, and daily political participation, based on a range of behavioral questions.

Graphical Evidence Similarly, we present graphical evidence on the evolution of political attitudes across Chinese age cohorts. Figure 1 illustrates the distribution of government support and political trust across different age cohorts. Panel A focuses on the rural sample, showing that political support remains stable among individuals aged 18 and older, but for those under 18, a higher proportion expresses long-term support (some achievement) for the government after exposure to HAGC. This effect is more pronounced among younger age groups, whose values are more easily shaped. Panel B compares the urban sample, where the HAGC policy was not targeted. In this case, government support is similarly distributed across cohorts, with no significant changes observed. Panels C and D show similar patterns for government trust. In rural areas, political trust increases among those under 18 after the policy is introduced, while it remains stable for those over 18 and for those living in urban areas.

4 Empirical Strategy

4.1 Baseline Specification

We utilize a generalized difference-in-differences (DID) identification strategy that leverages two distinct sources of variation. First, the implementation of the HAGC policy occurred at dif-

ferent times across counties. Second, within each county, individuals from different birth cohorts experienced varying levels of exposure, with the adolescent cohort (aged 7-18[14] at the time of policy implementation) being particularly susceptible. We estimate the following model using rural samples:

$$Y_{igc} = \alpha_0 + \alpha_1 Exposure_{cg} + \alpha_2 X_i' + \lambda_c + \sigma_g + \mu_p + \epsilon_{igc}. \quad (1)$$

where Y_{igc} denotes outcomes for individual i born in year g at county c.[15] We explore a series of political outcomes including support for the government, trust and satisfaction level with government officials, conflict with officials, and protest actions. $Exposure_{cg}$ equals one if individuals in cohort g living in county c were aged 7-18 when HAGC was introduced in county c. The control group consists of individuals aged 19-31 at the time of HAGC, who had already passed the period when their values were most susceptible to media influence. The coefficient of interest α_1 captures the net-impact of the HAGC on the outcomes of adolescents compared with older cohorts. We restrict to rural samples born between 1982 and 2002 where HAGC was implemented.[16]

We include a comprehensive set of covariates and fixed effects (FEs) in our analysis. X_i denotes the set of individual-level controls, such as gender, marital status, the average age and education level of parents. σ_g represent cohort FEs, and λ_c and μ_p are FEs for the county during HAGC policy and the prefecture-level city of current residence, respectively, to control for migration. These FEs control for unobservable, time-invariant characteristics of both the county during HAGC policy and current residence, which may influence political institutions and culture. Standard errors are clustered at the county level (during HAGC policy) to account for correlations in outcomes between adolescents in the same county.

Concerns may arise that time-varying factors, such as public policies, propaganda strategies, and macro-economic trends, could confound our estimation. Thus, we incorporate additional cross-sectional data into each specification, creating a panel dataset that controls for regional policy fluctuations. The specification is as follows:

$$Y_{igct} = \alpha_0 + \alpha_1 Exposure_{cg} + \alpha_2 X_i' + \alpha_3 X_{p,t=2007}' \times \sigma_g + \lambda_c + \sigma_g + \mu_p + \rho_{mt} + \tau_m \times T_g + \epsilon_{igct} \quad (2)$$

The key distinction from Equation 1 is the incorporation of multiple cross-sectional datasets, more stringent FEs, and higher-level predetermined covariates. Given that government policies may influence citizens' immediate political engagement and understanding, an enlarged sample

14. Children begin primary school at the age of seven in China. From this point until they reach eighteen (a twelve-year period of education), their cognitive abilities and political attitudes are systematically shaped. Our results are fully flexible for alternative age-range choices, as shown in the event-study results. For robustness, we also use alternative treatment windows (to 10-18, 13-18, and 16-18) and control windows and obtain similar results.

15. Our data includes information on individuals' original household registration (*hukou*) during their formative years, as well as their place of residence at the time of the survey. To accurately capture their exposure to the policy, we assign the county of residence during the formative years rather than their current place of residence.

16. Longer cohort choices (such as 1978 to 2002) are discussed in the robustness check.

size allows us to control for the political environment and other policies in province m across various years (waves) t (ρ_{mt}). To ensure that regional trends within cohorts do not bias our findings–that is, to avoid conclusions that merely reflect political attitude shifts among cohorts in different regions due to varying policies or political trends–we controlled for province-cohort-specific linear trends ($\tau_m \times T_g$). Additionally, we account for regional economic characteristics by controlling for a series of pre-determined prefectural characteristics including phone user ratio, internet user ratio, and percentage of secondary industry in GDP (in 2007, before HAGC) and cohort dummy to mitigate potential "bad controls" ($X'_{p,t=2007} \times \sigma_g$).

Immediate and Long-term Impact We evaluate the short-term effects of entertainment exposure using the 2010-2014 waves of our data and assess the long-term impact using the 2016-2020 waves (eight years post-policy). There are three advantages to highlight the difference between short-term and long-term impact. First, as shown in Figure A5, while the majority of respondents in the 2010-2014 waves are under 18, most individuals in the 2016-2020 waves are already adults, with stable cognitive systems and entrenched political values. Second, it helps distinguish cohort trends and other policies from our effects. If the baseline just reflects a cohort trend, where younger individuals are more likely to support the government, similar patterns would be observed in both the short- and long-term samples. Third, it also allows us to isolate two mechanisms: (1) the immediate alleviation of economic burdens and improvement in living standards, which boosts political support particularly in the short run, and (2) the long-term effects of TV exposure on cognitive formation and subjective political demands, which should accumulate in the long run.

TV Exposure In Equation 2, we do not account for age-specific heterogeneity within the 7-18 age group. As younger individuals are exposed to TV for a longer time, and are more susceptible to media influence compared to their older peers, they should exhibit greater value shifts and changes in political attitudes. Following Huang and Liu (2023), we re-define the exposure variable as a continuous measure to capture the percentage and number of policy-overlap years for each county c and cohort g within the 7-18 age range. The first is the percentage of the number of years that respondents exposed to HAGC to the total number of HAGC implementation years (4 years).[17] Another measure is the age difference between the age when the respondents first exposed to HAGC and 18 years old, which measures the length of exposure to TV.[18]

17. Policy overlap exposure 1 are quarterly values within 0-1 (i.e. 0, 0.25, 0.5, 0.75, and 1). For example, a respondent was exposed to HAGC at the age 15, then policy overlap exposure 1 is (18-15)/4=0.75, which means that he or she was exposed to HAGC for 75% of the total implementation time. If another respondent was exposed to HAGC at the age 12, then policy overlap exposure 1 is 1, which means that he or she was exposed to HAGC for all the policy time.

18. Policy overlap exposure 2 are ordered integers from 0 to 11. For example, a respondent was exposed to HAGC at the age 11, then policy overlap exposure 2 is 18-11=7, which means that 7 adolescent years overlapped with HAGC implementation or spillover. If another respondent was exposed to HAGC at the age 22, then policy overlap exposure 2 is 0 because he or she were not exposed to HAGC during adolescence.

Triple-difference Design To address concerns that the core estimated coefficients in Equation 2 might capture inherent variations within cohorts rather than HAGC-induced effects, we expand the original sample, which only includes rural residents, by adding an urban sample as an additional control group. We estimate the following triple-interaction specification using a pooled sample of both rural and urban residents. The HAGC policy affects only rural households in TV consumption, and urban citizens within the same county face similar political environments and economic conditions, so their difference reflects the effect of HAGC policy. Thus, the triple difference (DDD) specification is as follows:

$$\begin{aligned} Y_{igct} = \gamma_0 + \beta_1 Rural_i \times Exposure_{cg} + \beta_2 Exposure_{cg} + \beta_3 Rural_i + \beta_4 X_i' \\ + \beta_5 X'_{p,t=2007} \times \sigma_g + \lambda_c + \sigma_g + \mu_p + \rho_{mt} + \tau_m \times T_g + \epsilon_{igct} \end{aligned} \tag{3}$$

In this specification, $Rural_i$ indicates whether individual i held a rural *hukou* at the time the HAGC policy was introduced. The core coefficient of interest, β_1, differs from Equation 2 by further isolating cohort-specific impacts of other sources such as general political environment or socioeconomic trends. The DDD specification thus eliminates the cohort-specific trends, leading to a more precise causal interpretation of the long-term political effects of the HAGC policy on the younger cohort. We also use separate time windows to differentiate the short-term and long-term impacts of the HAGC policy.

Multiple Hypothesis Testing Our empirical analysis examines a broad set of outcomes spanning several conceptually related domains, including political attitudes, political behaviors, media consumption patterns, cognitive outcomes, among others. The use of multiple measures within each domain raises concerns about false positives arising from multiple hypothesis testing.

To address this issue, we implement two complementary procedures at the outcome-domain level. First, we adjust individual p-values within each pre-specified outcome domain using the Holm step-down procedure (Holm, 1979), which controls the domain-wise error rate while remaining less conservative than the Bonferroni correction. We interpret Holm-adjusted p-values using conventional significance thresholds. Second, we report a Cauchy combination test (Liu and Xie, 2020) as an omnibus test for each outcome domain. This approach aggregates information across related outcomes and allows us to assess whether there is systematic evidence of an effect within each domain, even when individual estimates are noisy. Together, these procedures ensure that our inference is robust to multiple comparisons while preserving power to detect consistent effects across theoretically related outcomes.

4.2 Identification Assumptions

Our generalized DID analyses rely on the assumption of parallel trends across cohorts. This identification strategy does not require HAGC counties to be exogenously or randomly assigned. Instead, it assumes that individuals in counties with and without HAGC exposure would have exhibited similar cohort trends in political outcomes before the implementation of HAGC. A potential threat to this assumption is that its roll-out sequence may be correlated with pre-policy characteristics or cohort trends in the counties. For instance, rural areas with limited media access, lower state penetration, or higher levels of social unrest may have been selected for earlier HAGC implementation. These characteristics could also influence cohort trends in political attitudes. section 2 presents evidence showing that the policy goals are unrelated to these characteristics. Further, we formally conduct a series of statistical tests in this section to address this concern.

Balance Tests First, one of the important identification assumptions is that there are no systematic differences in local traits between counties in various roll-out sequences before policy. We conduct two sets of statistical tests to examine the relationship between the timing of the HAGC policy and local characteristics prior to the HAGC policy. Figure A6 presents differences across HAGC waves in socioeconomic factors (e.g., GDP, industrial structure, population), education, social security, infrastructure, and information technology characteristics, which shows no significant correlation with any of these factors.

Next, Figure A7 uses individual-level data, with individuals exposed to the third wave of the policy serving as the control group, to examine differences in baseline outcomes among those affected by the policy in pre-policy counties. Specifically, we focus on individuals aged 18 or older at the time the policy was introduced, as they had already passed the formative age range and had more stable political attitudes. We analyze potential differences in political attitudes and behaviors across the policy waves and find no significant differences in these areas. Similarly, we observe little differences in cognitive ability, mental health, or education levels.

Furthermore, we check the wave-specific treatment effects at the panel A of Figure C1 to make sure the estimation results are not sensitive to different waves, with consistent results. All in all, these findings suggest that the timing of the HAGC policy roll-out is exogenous and have little to do with local socioeconomic factors or individual potential outcomes.

Cohort Trend across Different Waves We hope that there is no significant trend between cohorts. Otherwise, our results may be driven by trends rather than policy. To further assess the parallel trends assumption for each cohort, we examine county-level characteristics prior to the implementation of the HAGC policy. Using individual-level data from rural sample, we focus on individuals born between 1978 and 1989. These cohorts were adults by 2007, when HAGC be-

gan, making them unlikely to be influenced by the policy in their later outcomes. Specifically, we regress outcomes on the interactions between cohort indicators and the year of HAGC adoption. In Figure A8, we use the cohort born in 1989 as the reference group, and the plotted coefficients show whether pre-policy characteristics differ in one-year intervals relative to HAGC adoption. Our results indicate no significant evidence of differing pre-policy cohort trends in these outcomes across counties with different HAGC start years.

Event Study To formally test the parallel trends assumption, we explore an alternative specification to assess whether the impact of the HAGC policies varies with the different periods of exposure, estimating the following event-study specification:

$$Y_{igct} = \alpha_0 + \sum_{\gamma=9,\gamma\neq 19}^{28} \alpha_\gamma \boldsymbol{I}(Age=\gamma) + \alpha_2 X_i' + \alpha_3 X_{p,t=2007}' \times \sigma_g + \lambda_c + \sigma_g + \mu_{pt} + \rho_{mt} + \tau_m \times T_g + \epsilon_{igct}, \quad (4)$$

where $\boldsymbol{I}(Age=\gamma)$ indicates whether cohort g lived in county c during policy aged γ when the HAGC policy was first introduced. Other variables are specified as in the baseline equation. The dynamics of coefficients α_γ capture the evolution of the difference in individuals' outcomes based on their varying exposure to the policy, relative to the reference cohort. This approach introduces additional variation within the exposure period, allowing for a better understanding of the effects across different exposure windows on various affected cohorts. We test the parallel pre-trend assumption based on the rural sample, with urban samples as placebo tests. For illustrative purpose, we group those aged below 10 and above 28 into endpoint bins.

Following Fadlon and Nielsen (2019), we also test the parallel pre-trend assumption based on the triple-difference specification in the pooled sample:

$$\begin{aligned} Y_{igct} = \beta_0 + \sum_{\gamma=9,\gamma\neq 19}^{28} \beta_{\gamma_1} \boldsymbol{I}(Age=\gamma) \times Rural_i + \sum_{\gamma=9,\gamma\neq 19}^{28} \beta_{\gamma_2} \boldsymbol{I}(Age=\gamma) \\ + \beta_2 X_i' \times Rural_i + \beta_3 X_{p,t=2007}' \times \sigma_g \times Rural_i + \lambda_c \times Rural_i + \sigma_g \\ \times Rural_i + \mu_{pt} \times Rural_i + \rho_{mt} \times Rural_i + \tau_m \times T_g \times Rural_i + \epsilon_{igct} \end{aligned} \quad (5)$$

where β_{γ_1} denotes the introduction-age-varying effects of HAGC on rural residents compared with urban residents, which accounts for unique cohort trends across counties while controlling for common urban-rural trends.

4.3 Identifying the Treatment Margin

Our identification requires a clear interpretation of the treatment itself. A natural concern is that the HAGC policy may be a bundled treatment, capturing not only increased exposure to en-

tertainment TV, but also other home appliances, other information on TV, or gratitude toward the government induced by material benefits. If so, our estimates would be harder to interpret as the political effect of entertainment-media exposure per se. We thus clarify the treatment margin before turning to the main results. Our core interpretation is that the HAGC policy primarily expanded access to color TV, which in turn increased adolescents' exposure to entertainment programs, instead of generating a broad increase in home appliances or a pure material-transfer effect.

Increase in Color TV We first verify that the HAGC policy effectively promoted TV consumption, particularly for color TVs. Based on the CHNS data (2004–2009), using a staggered difference-in-differences specification, Table A2 shows that the policy substantially increased the ownership of color TVs, as well as the quantity and total value. In comparison, the effects on black-and-white (B&W) TVs are much smaller and less significant. Estimates based on the NFP data (2004–2009) in Table A3 confirm this pattern: HAGC significantly raised color TV ownership but generated almost no increase for B&W TVs.

More importantly, the policy led to minimal change in other home appliances(Table A3), including washing machines, refrigerators, electric fans, cameras, landline phones, and mobile phones. These findings indicate that the policy primarily boosted access to color TV rather than other home appliances, and our findings can be ascribed to TV exposure.

Increasing Exposure to TV We then establish the link that increased TV access indeed translate into more time spent on watching TV among teenagers. Figure 2 illustrates changes in teenagers' time allocation following the introduction of HAGC. We use short-term samples (2011), capturing outcomes one to three years after HAGC implementation, when the treated cohorts were still in school. The results show a sizable increase in the frequency of watching TV, with no changes in consumption for other media resources. This pattern is consistent with the effectiveness of HAGC in boosting TV consumption. In addition, Panel B documents a notable reduction in study time, with little changes in other activities. This reflects certain "substitution effect" of TV watching and may pose a negative effect on education, as we verify later.

Increasing entertainment consumption Lastly, the increased TV watching is primarily driven by entertainment use. Table A4 draws on the 2008 CGSS data to explore the instant effects of the policy: individuals who were exposed to the HAGC policy between 7 and 18 were more likely to watch entertainment programs, ranging from films, dramas, to anime. To young rural residents, these new programs is inviting and avant-garde, attracting more consumption via color TV. The enduring nature of this preference for entertainment content is examined in Table A5, which re-

ports the long-term effects of the policy.[19] Notably, exposed teenagers become more inclined to watch entertainment programs even after turning adults, including soap operas, historical dramas, and so on. In contrast, they watch much less political news or current affairs. Taken together, teenagers spent more time watching TV, particularly entertainment programs, and such preferences turned into habits in the long run.

Entertainment versus Gratitude Another related concern is that the HAGC policy increased later political support simply because it delivered a visible material subsidy, generating short-term gratitude toward the government. By subsidizing the cost of TVs, the policy alleviated household financial burdens and improved their living conditions and general well-being. If this short-term material benefit were the main driver, then one would expect improvements in government support and related political attitudes to arise among all treated individuals, regardless of their actual TV watching, and to appear relatively quickly after policy implementation.

We address this concern using three complementary empirical strategies. First, we examine the temporal pattern of the effects, distinguishing between short-term and long-term responses. If the estimates were primarily driven by material benefits or gratitude, one would expect immediate increase in political support. By contrast, if the effects operate through media exposure and socialization, they should emerge more gradually and persist over time.

Second, we exploit heterogeneity in actual TV consumption. The material transfer associated with HAGC is common to all treated households, regardless of whether individuals actively watch TV. In contrast, increased exposure to entertainment content only applies to those who spend time watching TV. We therefore compare individuals with zero or very low TV watching time to those with substantial exposure. If the observed increase in political support were driven by material benefits or gratitude, then there should be little difference between these two groups. Conversely, if the effect only applies to those “heavy-watchers" but does not apply to others, then it supports our interpretation based on media exposure.

Third, we exploit heterogeneity in TV programs (broadcasted programs across regions). Similar logic applies: people develop gratitude regardless of what programs they watch on TV, whereas our arguments applies primarily to those who can watch entertainment programs on TV. If the effect operates through entertainment consumption, then regions with greater exposure to such programs should exhibit stronger effects.

Taken together, these strategies allow us to distinguish between a pure material-transfer effect and an exposure-based mechanism. Combined with the evidence that HAGC primarily increased access to color TV rather than other household durables, and that the additional viewing was concentrated in entertainment rather than political content, we interpret the HAGC treatment primarily as increased formative exposure to entertainment TV.

19. As CGSS lacks relevant data on program choices after 2008, we use the ISCIU dataset in this table.

5 Empirical Results

This section presents the main empirical findings. We show that exposure to entertainment television during adolescence increases support for the authoritarian regime, raises political trust and subjective evaluations of government performance, and reduces protest and conflict with officials. We then examine heterogeneity across media content, and a range of robustness checks.

5.1 Political Support and Government Trust

Differential Effects by Age at First Exposure We directly examine the impact of HAGC policies across the timing of exposure with the event study specification based on Equation 4. The flexible framework allows us to evaluate the effects across various exposure windows and test the parallel trends assumption, specifically, whether trends in adolescent outcomes across different groups would have followed similar trajectories in the absence of the HAGC policy.

Figure 3 uses the group of individuals aged 19 when the HAGC policy was introduced as the reference group. Panels A and B focus on the rural sample. We find that, regardless of whether we measure government support using ordinal or binary variables, the younger individuals were when exposed to HAGC, thus the longer they were exposed during their formative years, the higher government support they have after turning adults. Specifically, those under 18 when exposed to HAGC show significantly higher support for the government later on, which is not significant for individuals over 19, whose values are less prone to media influence. This further supports the parallel trends assumption. Panels C and D use urban samples as a placebo test, showing no significant differences in government support among individuals exposed to the policy at different years. Panels E and F report the event study results under the DDD setting using Equation 3. By pooling both urban and rural samples, we plot the differences in government support between rural and urban *hukou* for each age group, with individuals aged 19 as the reference group. The results are consistent, showing that for those under 18 at the time of HAGC, the rural-urban gap in support increases, with rural residents showing higher government support while urban dwellers showing little change. For individuals over 18, the rural-urban gap remains stable.

In the baseline, we use government support as a continuous variable ranging from 1 to 5, while also reporting results based on a binary grouping (scores of 3-5 coded as high support). Figure B1 presents the event study results for alternative dependent variables, where the cut-off is varied. The results remain robust and show a similar pattern. Figure B2 shows the different effects on political support by age at initial policy exposure with nonlinear methods (including both parametric and nonparametric). This analysis delineates the differential responses across age groups in both rural and urban settings, revealing a pattern consistent with that observed in Figure 3: individuals exposed at younger ages are more likely to have increased government

support, while those affected by HAGC after 18 do not exhibit similar pattern.

Regression Evidence Table 1 reports the baseline results using specifications in Equation 2 and Equation 3. All columns use ordinal measures of government support (1-5, with higher values indicating stronger support) as the dependent variable. Regressions control individual characteristics, prefectural characteristics, county FEs, cohort FEs, current prefecture FEs, province wave FEs, and province cohort-specific linear trends. Columns (1)-(2) present long-term outcomes based on panel data from 2016 to 2018, when most of the treatment group (exposed to the HAGC policy aged 7-18) had reached adulthood. Column (1) shows that exposure to the HAGC policy during 7-18 leads to higher government support in adulthood compared to those with more established values (ages 19-31). The average increase is 0.230, which represents approximately 6.8% (0.230/3.406) of the mean.[20]

To elucidate the forces driving such change, we compare the short-term impact of HAGC with the long-term one. Columns (3)-(4) of Table 1 report the short-term impact on youth groups using cross-sectional data from 2012 to 2014, when most participants had only been exposed to HAGC policies for 2-6 years. As in column (3), we find no significant short-term effects on government support, suggesting that immediate economic benefits and enjoyment from richer entertainment options are unlikely to drive increased support. Rather, shift in cognitive abilities is a long-term process, which is verified in the long-term effects in columns (1)-(2). Thus, we argue that decline in cognitive abilities and consequent reduced political awareness are more likely to account for our baseline findings (see section 6).

Additionally, we pool the urban and rural samples, and apply the DDD strategy in columns (2) and (4) to account for county-specific cohort trends. Column (2) shows that rural residents exposed to HAGC policies during ages 7-18 experience higher government support in adulthood, with an average increase of 0.156, compared to the urban sample.[21] However, as in column (4), we do not observe a similar significant pattern with short-term samples. Consequently, we adopt Equation 2 as the main specification, while Equation 3 is used as supplementary evidence and placed in the appendix for subsequent analyses.

To address the concern that the short- and long-term distinction is driven by arbitrary sample splits, we estimate the effect separately for each survey wave. Figure B3 shows that the estimates are small and insignificant in early waves (2012-2014), but become larger and statistically significant in later waves (2016-2018). This pattern suggests that the effects emerge gradually over time.

20. The results are robust to binary specification, where we define high support ($\geq$3) as 1 and low support ($<$3) as 0. Under binary specification, HAGC exposure leads to a 10% transition from low support to high support. Considering China's large rural population in 2008 (about 0.6 billion), the policy has the potential to turn 60 million low-support rural residents to high-support.

21. Similar results are obtained when adjusting for birth-year-specific cohort distributions and using a binary setup.

Summary: Benchmarking the Effect Sizes To further quantify the magnitude of the effect, we compute persuasion rates (DellaVigna and Kaplan, 2007; DellaVigna and Gentzkow, 2010): the percentage of individuals who had low government support before the policy but who turned to high support after being affected. Following Cantoni et al. (2017), we report both the unconditional and conditional persuasion rate, which is 0.659 and 0.486, respectively.[22] These magnitudes are closely comparable to Cantoni et al. (2017), who report persuasion rates of 0.683 (unconditional) and 0.466 (conditional) for school curricula shaping political trust among Chinese students. Taken together, the evidence indicates that long-term exposure to entertainment TV during adolescence can generate effects on political support comparable to sustained propaganda education, and far larger than short-term exposure to political news.[23]

5.2 Political Evaluation and Anti-government Protest

Positive Subjective Political Evaluation We further examine the long-term impact of the HAGC policy on adolescents' political values. Figure 4 shows the long-term and short-term effects of HAGC policies on government satisfaction and political trust among rural samples, using the CSS dataset. We find a significant increase in both government satisfaction and political trust in the long term, with no effects in the short term, consistent with the baseline results.

In addition, Table B1 shows that exposure to HAGC during adolescence predicts higher government satisfaction in a wide range of issues. This effect spans various aspects of government performance, including social security, human rights protection, anti-corruption efforts, equity promotion, and transparency. These broad subjective evaluations constitute *perceived* legitimacy of the authoritarian state. In contrast, more objective, welfare-relevant evaluations, such as healthcare, environment, and employment, remain unchanged with TV exposure. This pattern suggests that the effect is concentrated in broader, more abstract evaluations of regime legitimacy rather than real improvement in governance quality. A further event study in Figure B4 shows differential effects of first-exposure age on government satisfaction indicators. Individuals exposed to the HAGC policy after age 19 maintain stable across all aspects of government satisfaction, while those exposed before age 18 report higher satisfaction levels. In sum, only subjective indicators are influenced by teenage TV exposure, leaving evaluation on objective welfare unchanged.

A similar effect is observed for political trust among teenagers. Figure 4 shows that affected teenagers display greater trust in the state in adulthood compared to adults. Figure B5 conducts a placebo test for other types of trust, such as trust in parents, neighbors, Americans, or strangers, showing little differences. Additionally, no significant effects are observed within urban samples.

22. The unconditional persuasion rate is the estimated treatment effect divided by the share of individuals who had low government support in the entire sample, while the conditional persuasion rate is the effect divided by the fraction of individuals who would hold low government support in the absence of the policy.

23. Since policy eligibility is an imperfect proxy for actual changes in TV exposure, these estimates likely represent lower bounds on persuasion with respect to effective exposure and should be interpreted with caution.

Lower Anti-government Protest In addition to shaping political attitudes, TV exposure has far-reaching effects in political actions. Figure 4 shows a significant decrease in perceived official malpractice or conflicts with officials for affected teens. Further analysis in Figure B6 shows a placebo test, confirming that the policy's impact does not significantly affect perceptions of conflicts arising from wealth disparities, ethnic issues, religious matters, or labor disputes. Thus, the policy exclusively reduces the perception of conflict with the government, highlighting significant changes in their views towards the state.

We then explore the long-term effects of the HAGC policy on individuals' experiences with conflict and protest. Columns (1)-(2) of Table B2 indicate that affected individuals are less likely to join protests and have fewer conflicts with government officials. One might be concerned that this could be due to differential treatment by government officials. However, Columns (3)-(6) address this concern by including variables indicating whether respondents experienced discrimination, government negligence, unreasonable charges, and income discrimination by officials. After controlling for these negative experiences with the government, we still observe that the HAGC policy reduces the likelihood of conflict or protest with government officials among affected individuals, with stable coefficients. This suggests that, given equal treatment by officials, those more influenced by HAGC are less prone to conflict or protest. Figure B7 shows the varying impacts of the age at first exposure to HAGC on anti-government actions, revealing a consistent trend. Specifically, those exposed to HAGC policies before the age of 18 have lower conflict levels and are less likely to protest, while those exposed after the age of 19 show a more stable response.

Overall, our baseline findings highlight HAGC's long-term implications on authoritarian governance. We find that the policy has led to increased support for the authoritarian state, higher political trust, and rising satisfaction with the government across various issues. As a result, protest participation decreases, and conflicts with authorities diminish. Importantly, these effects are not merely the result of short-term economic benefits or temporary satisfaction. In section 6, we will explore the mechanisms underlying these long-term effects.

5.3 Heterogeneity Analyses

As shown previously, teenagers exposed to HAGC are more likely to develop pro-government values compared with cohorts who missed the policy. If the effect of the HAGC policy on adolescents operates primarily through entertainment TV, then regions with greater exposure to such programs should exhibit stronger effects. Using data from the *China TV Rating Yearbook*, we compute the regional broadcast shares of entertainment programs versus political and news programs in 2006 (predetermined). Columns (1)-(2) of Table 2 interact these broadcast shares with HAGC exposure. We find that the policy's effect on youth support for authoritarian regimes is significantly amplified in regions with a higher share of entertainment programs, while political and news broadcasts have no discernible effect. Columns (3)-(5) further disaggregate entertainment content

by genre (e.g., TV dramas, Reality shows, and musical performances) and reveal that regions with greater exposure to these entertainment programs experience a more pronounced HAGC effect. Given that entertainment content constitutes the majority of Chinese TV programming, these results suggest that the political effects of TV on adolescents are driven primarily by entertainment channels.

5.4 Robustness Checks

In this section, we conduct a series of robustness checks. We present the main results below, with details illustrated in Appendix C.

We begin by addressing concerns related to treatment heterogeneity in staggered DiD settings, where conventional two-way fixed effects estimators may yield biased estimates. First, we estimate wave-specific effects in Panel A of Figure C1. The results are consistently positive and statistically significant across survey waves, indicating that our findings are not driven by any particular sample period. Second, we re-estimate our baseline specifications using two heterogeneous-treatment-robust DiD estimators: the group-time average treatment effect estimator of Sun and Abraham (2021) and the two-stage imputation estimator of Gardner (2022). As shown in Table C1, the estimates remain highly consistent with the baseline in both statistical significance and economic magnitude.

We next examine the robustness of our results to alternative treatment constructions. In Panel B of Figure C1, we replace exposure as a continuous variable, defined as the number of overlap years between ages 7 and 18 during which individuals were exposed to the HAGC policy. This approach uses cohort-specific variation in TV exposure and policy overlap, rather than a binary (0-1) exposure variable. The results remain robust. Second, we find that the baseline results remain consistent under different treatment criteria in Panels C and D, where we apply varying age thresholds to define the treatment and control groups. Lastly, we conduct placebo tests in Panels E and F, where the treatment is defined as exposure for individuals with entrenched values at the time of policy implementation, or where the policy year is falsely altered. The treatment effects are statistically indistinguishable from zero, corroborating our baseline specification.

We randomly assign pseudo-treatment exposure to cohorts or regions that should not have been affected by the actual policy, as shown in Figure C2. These tests suggest that our baseline results are unlikely to be driven by spurious correlations or model mis-specification.

We exclude the influence of other contemporaneous policies. In Panel A of Figure C3, we consider the influence of the expansion of higher education, WTO accession, tax reforms, and the new rural pension policy, the new rural pension policy, the anti-corruption campaign, and the poverty alleviation campaign. The results are highly robust, with economic significance similar to the baseline. We also consider the influence of other confounding factors, such as pre-determined

socioeconomic, educational, security, and media conditions. Panel B and C show robust results. In Panel D, E, F, we further discuss issues like *hukou* change, migration, and data attrition, which shows they pose little threat to our main effects. Table C2 further demonstrates that TV exposure is unlikely to correlate with changes in *Hukou* status or internal migration.

We employ different specifications. In Figure C4 (Panel A-C), we show that our baseline results are not sensitive to different types of cross-sectional estimates, weighted regressions, or various multiple hypotheses tests. Panel D shows that the coefficients in the baseline remain stable as we progressively add control variables, and Panels E-F show that the results remain robust even when accounting for cohort- and region-specific higher-order linear trends and clustering.

6 Mechanisms and Alternative Explanations

How does increased media exposure boost authoritarian government support? In this section, we explore the *illusory accountability effect* of TV on value-forming teenagers. First, we provide evidence suggesting that exposure to entertainment television hampers cognitive abilities and reduces educational attainment, while simultaneously fostering positive emotions. Second, we offer suggestive evidence that TV exposure lowers one's social connectivity and social skills, which is especially important to the nurture civil capital. This combination effectively lowers public expectations, diminishes accountability demands on government work, and enhances satisfaction with the government, thereby diminishing demands for democratic reforms and reducing anti-authoritarian protests. We then rule out alternative explanations, such as short-term government support, welfare improvements, and expansion of pro-communist TV channels.

6.1 The Illusory Accountability Effect

In this subsection, we delve into the mechanisms underlying the *illusory accountability effect* of TV. We first present evidence of an accumulative cognitive penalty. The negative impact on cognitive scores intensifies with the duration of exposure. This suggests that the HAGC policy generated a persistent scar on the human capital of the affected cohorts. Unlike transient opinions which can be swayed by new information, this reduction in cognitive ability creates a durable barrier to political accountability that persists well into adulthood. This cognitive scarring cascades into broader developmental failures. Specifically, the erosion of foundational skills during adolescence impedes subsequent educational attainment and reduces political knowledge.

In parallel with cognitive deterioration, entertainment media reshapes the affective landscape of the citizenry. While their objective human capital declines, affected individuals report higher levels of subjective well-being and lower emotional aggrievement. Deteriorating capacity coupled with heightened contentment is reinforced by the atomizing nature of television consumption,

which degrades social networks and civic skills necessary for collective action. By simultaneously hindering the cognitive capacity to demand change (*ability*) and satisfying the emotional need for comfort (*willingness*), entertainment media cultivates a generation characterized by political apathy and compliance.

6.1.1 Reduced Ability to Demand Accountability

Decline in Cognitive Skills Cognitive ability is key to political participation (Denny and Doyle, 2008; Dawes et al., 2014). TV exposure may reduce viewers' cognitive abilities in the long term, limiting their understanding of political and social issues, thus making them easier to feel satisfied and more prone to political propaganda. In Figure 5, we examine the effects of HAGC policy on the cognitive abilities of rural populations. We use verbal and mathematical reasoning tests from CFPS as proxies for cognitive ability.[24] These tests reflect habits developed over time, which are crucial for rational thinking about social and political issues. In panels A and C, using the identification strategy in Equation 4, we find that individuals exposed to HAGC during adolescence perform significantly worse in the two tests, with lower test scores compared to those exposed during adulthood. Panels B and D report the results from specification Equation 5, comparing rural and urban cohorts within the same county. The pattern remains consistent, with a widening gap in cognitive abilities between rural residents and their urban counterparts due to TV exposure.

Table D1 examines the cognitive impact of HAGC policy across different lengths of exposure. Panel A presents the overall effect, while Panel B assesses the effects across three waves. The findings indicate that the negative impact intensifies over time, suggesting a potential "accumulative effect": for groups influenced by the policy, cognitive abilities deteriorate with prolonged exposure to TV. This gradual cognitive decline also elucidates why long-term political support increases more than short-term support. During this period, rural China underwent a consolidation of school resources. The reduction in the number of schools may adversely impact students' cognitive abilities. Simultaneously, textbook reforms aiming at enhancing political loyalty may have hindered students' critical thinking. In Panels C and D, we control for the cohort-specific impacts of these policies, with consistent findings. In Figure D1, we directly compare the cognitive effects of HAGC in the mid-term (2014 and 2016 waves) and long-term (2018 and 2020 waves), and find consistent results: cognitive decline is more pronounced in the long term than in the mid term.

24. CFPS employs four cognitive measures across different years. In 2010, respondents were assessed on vocabulary and numeracy, with the vocabulary test consisting of thirty-four Chinese characters drawn from primary school textbooks. The numeracy test included twenty-four math problems, also drawn from school textbooks. In 2012, the CFPS adopted two cognitive measures from the Health and Retirement Survey (HRS): a memory test (immediate and delayed word recall) to assess short-term and long-term memory, and a number series test to evaluate numerical reasoning. The number series test is a two-stage adaptive task, with the first stage consisting of three items of increasing difficulty, followed by a second stage with items tailored to the respondent's performance in the first stage.

Lower Educational Achievement and Political Knowledge A direct implication of impaired cognitive skills is lower educational achievements. As teenagers are more prone to TV exposure and exhibit more reduction in cognitive ability, they may perform worse in school and are consequently less likely to pursue higher education. This matters politically because college education can foster civic culture, strengthen critical thinking, and enhance individuals' capacity to evaluate government performance independently. In this sense, higher education may weaken support for authoritarian rule (Glaeser, Ponzetto and Shleifer, 2007; Croke, Grossman, Larreguy and Marshall, 2016). More broadly, as Guriev and Treisman (2019) argue, the expansion of education equips citizens with the skills and knowledge needed to organize opposition under authoritarian regimes. By contrast, reduced educational attainment may limit political knowledge and critical reflection, giving rise to less-informed government support.

In Figure D2, we plot HAGC's effects on educational attainment by age at first exposure, using CFPS dataset in Panels A and B, and CGSS dataset in Panels C and D. Results across data sources show a similar pattern: exposed teenagers have lower likelihood to enter college (and above) education, and the likelihood becomes even smaller for cohorts with longer TV exposure. The impact on senior high school status is trivial, possibly due to its relative easy entry and the state's strong support.[25] More importantly, Chinese high-school education emphasizes rules, discipline, and compliance over creative thinking, where students can hardly develop a civic mind.[26] Panel E and F compare the impact of HAGC on different educational levels, where we find a disproportionate effect on college entrance. Thus, the fact that TV exposure trivially affected high-school attendance but significantly reduced college entry can hinder the development of civic culture and hence bring about less-informed political support.

Table D2 reports the estimation results using Equation 2. We find that long-term exposure to entertainment TV reduced the likelihood of teenagers attaining college by about 10 percentage points (ranging from 0.104 to 0.144) across different datasets (CFPS, CGSS), as indicated in column (4) with the same control variables as the baseline, and in column (5) which includes additional controls for cohort-specific exposure to rural school consolidations and textbook reforms. Given the intense competition of the Chinese *gaokao*, this decline is considerable and could significantly impact the students' future labor-market outcomes.

Finally, we examine whether or not reduced education leads to less political knowledge, proxied by respondents' accuracy in answering a series of political questions. Table D3 shows that the policy has reduced political knowledge among exposed teenagers. This is possibly ascribed to a lack of civil education as shown before, and meanwhile less acquisition of political information,

25. High-school education is the next step of the 9-year compulsory education in China, and local governments usually exert effort to ensure high-school enrollment. Some regions even include high school into compulsory education.

26. While all education levels include communist ideology, college education (and universities) is more open, diverse, and inclusive than the highly standardized, ideologically driven *gaokao* system. Missing this experience may reduce political knowledge, lower accountability, and increase compliance with authoritarian rule.

as discussed later in Table D9. These results demonstrate early-age entertainment exposure has a long-entrenching effect on political knowledge, reducing their ability of political participation.

Discussion Our findings are in contrast to earlier studies that find TV exposure to improve educational outcomes (see e.g., Gentzkow and Shapiro (2008); Kearney and Levine (2019); Nieto Castro (2025)), primarily driven by children from disadvantaged households with limited sources of information. Two caveats may drive our distinctive results. First, while we all document increased TV viewing time, the content of TV programs differs. As shown in Figure A1, entertainment constitute the primary content consumed by Chinese TV viewers. Second, they focus on early-child exposure of TV. For school-age teenagers, TV entertainment is more likely to crowd out learning activities, as shown in Figure 2). In this sense, our findings accord with Malamud and Pop-Eleches (2011) that show how computer use hinder cognitive abilities and educational attainments.

Moreover, the gap in cognitive ability and education appears to widen over time, suggesting the possibility of dynamic mechanisms. On one hand, early TV exposure directly impairs cognitive ability (Figure 5). On the other hand, TV exposure first crowds out study time (Figure 2); this lowers educational achievement, especially college entry (Figure D2); lower education then feeds into weaker cognitive ability, so the cognitive gap becomes larger as time passes by. [27]

6.1.2 Reduced Willingness to Demand Accountability

Less Emotional Aggrievement Another source of government support is the relief of individual aggrievement (Passarelli and Tabellini, 2017). We use two types of variables to measure individuals' emotions. The first is the standardized depression scale provided in CFPS from 2018 onward. The second is the measure of individuals' positive emotions, also available in the CFPS data.

We start with CES-D indicator, a self-reported scale with higher scores reflecting higher tendency of depression.[28] Using the identification strategy in Equation 4, Panels A and C of Figure 6 show that exposed teenagers are generally less prone to depression, with longer exposure reducing depression more. Panels B and D further exclude cohort trends by triple-difference estimation. Table D4 reports the estimated effects of the HAGC policy on adolescent emotions using Equation 2. In the long term, exposure to HAGC decreases CES-D Score-1 and CES-D Score-2 by approximately 0.35-0.37 standard deviations and increases the weekly or monthly positive emotion index by approximately 0.22-0.24 standard deviations.[29]

27. This dynamic amplification effect should be taken with caution since data prevents us from testing it formally. What we can do is to exclude compositional changes that may drive the result: selection into cognitive testing does not differ across survey waves; differential educational trajectories, migration patterns, or labor market sorting cannot explain widening gaps over time; the composition of respondents is also relatively stable across waves.

28. CFPS adopts the depression scale measure from Center for Epidemiologic Studies Depression Scale (CES-D), see (Radloff, 1977). We adopt 2 CES-D measurements, containing 8 or 20 questions indicating tendency to depression, respectively.

29. Data on depression is only available in CFPS from 2018 onward, so we focus on outcomes in 2018-2020. For

The reduced tendency of depression is associated with better subjective emotions. In Table D5 and Figure D3, we show that longer TV exposure has led to higher level of self-reported emotions, such as cheerfulness, happiness, being supported, and feeling satisfied. Similar pattern holds when pooling all the positive emotions, suggesting TV-exposure could reduce one's aggrievement by boosting positive emotions, as shown in panels E and F of Figure 6. In sum, entertainment TV brings better subjective feelings to viewers without improving their objective lives.

Worse Social Network Social connectedness is one of the most important elements of the civic culture of democracy (Keele, 2007; Nannicini et al., 2013). On one hand, increased exposure to TV may isolate teenagers from each other, reducing their social interaction and social skills that are crucial for collective mobilizations. Thus they are less likely to launch protests or demand political reforms. On the other hand, reduced social interaction may make individuals less aware of social issues and more susceptible to state propaganda as they have fewer sources of information. As a result, they are more likely to be satisfied with the status quo and less likely to demand changes. We offer suggestive evidence to role of social networking in Table D6. Columns (1)-(4) demonstrate individuals with more teenage TV exposure have lower interaction with family members, relatives, and friends. Moreover, columns (5)-(8) show TV-affected teenagers have lower social skills: they tend to be more isolated and self-centered when grow up.

Discussion Our findings do not imply that HAGC improved general welfare. Rather, the evidence points to the fact that individuals feel better-off without being better-off. Entertainment itself can partly explain this gap. Moreover, lack of social interaction can restrict unfavorable social comparisons. As Braghieri, Levy and Makarin (2022) shows, social media negatively affects student mental health by fostering unfavorable social comparisons. Our result is exactly the opposite side of the story.

6.1.3 Political Apathy and Reduced Accountability

While media penetration hindered cognitive skills and educational achievement, it could also alleviate viewers from the burden of thinking critically by fulfilling them with easy pleasure (entertainment), signified by better emotions. Thus, the *illusory accountability effect* consolidate authoritarianism in two ways. On one hand, lowered educational performance and cognitive skills can jeopardize her *ability* in political participation, and on the other hand, one's perceived cheerfulness from entertainment reduces her *willingness* to rebel.

First, we explore how entertainment TV affects political accountability by analyzing viewers' demands on government. The CSS dataset asks whether respondents value government transfers

emotional indicators, data from CFPS 2016 can be used, so we use data on positive emotions from 2016 to 2020 in Figure 6 and Table D4.

to low-income groups, taxes on the rich, social equality, and reforms. We use the first two variables to measure preferences for redistribution and the last two to capture demands for change (democratic reforms). Table 3 demonstrates that those exposed to the policy reduced their demands for redistribution and demands for democratic reforms. This not only explains our baseline findings that affected teenagers support the state more and protest less, but highlights an important channel that TV influences government accountability: despite growing economic inequality in China, affected individuals still prefer to uphold authoritarian rule as they are already satisfied with the status quo. This helps explain why authoritarianism in China persists despite rising economic inequality and limited demand for democratic reform.

Next, we find that the HAGC policy led to long-term compliance with the authoritarian government. Using the ISCIU dataset, Column (1) of Table D7 shows that HAGC policy increased compliance with authoritarian rule by 0.8 standard deviations for the affected groups. Placebo tests using compliance with parents, teachers, and employers in columns (2)-(4) show little effects, highlighting its unique role in cultivating political compliance. This reflects that early-age TV exposure shapes long-term personality of political compliance that serves as the psychological foundations of authoritarian rule.[30]

Third, we investigate how the HAGC policy has shaped long-term engagement with political issues. In Panels A and B of Table D8, we extend the analysis to explore its implications on political engagement. The results reveal a similar pattern: compared to those less affected by the policy, the exposed teenagers are less likely to discuss politics in their daily lives and show a diminished interest in voting. Correspondingly, Table D9 shows how HAGC impact political engagement of citizens, including the likelihood of searching online for political information, reading political books, and watching political TV programs. These variables, ranging from 1 to 5, reflect varying levels of political engagement. Our findings indicate that HAGC has led to a significant decline in both formal and informal political involvement.

Combined together, the simultaneous reduction in both *willingness* (higher positive emotions, lower political interest) and *ability* (hindered cognitive skills, lower political knowledge) to involve oneself in civil politics is consistent with the need of authoritarian control. By diminishing cognitive abilities, education levels, and fostering a sense of contentment, the policy reduces political knowledge and participation, while increasing compliance with authoritarian rule and lowering demands for government accountability and democratic reforms. Under these conditions, authoritarian regimes can strategically exploit this ignorance to maintain stable rule (see e.g., Egorov and Sonin (2024)), knowing that citizens lack the structured knowledge to resist government expropriation, including large-scale corruption and malpractice, as we demonstrated earlier.

30. While we consider the set of personality that serves authoritarian governance, the very same traits, along with others, may also led to the rise of populism and strong-man politics in democratic settings.

6.2 Alternative Explanations

Expansion of Political TV Channels The media plays a crucial role for authoritarian regimes in disseminating propaganda information. For example, Ou and Xiong (2021) highlights that the expansion of radio stations facilitated the spread of Cultural Revolution propaganda. Thus, one may be concerned that the positive impact of HAGC policy on political support may not be ascribed to illusory accountability (higher sensitivity to propaganda) but due to more positive political information (more propaganda) on Chinese TV channels. We systematically address this concern through both supply-side and demand-side evidence.

First, we examine TV programming data from the *China TV Rating Yearbook* (2003-2017), which provides detailed viewership statistics by program type and region. The data reveal no significant differences in program composition or viewership patterns across counties (Figure D4). Importantly, Table D10 shows that the share of political programming remained unchanged following HAGC implementation, ruling out increased propaganda supply as a confounding factor. Second, leveraging individual-level viewership data from the ISCIU survey, we document that expanded TV access primarily increased entertainment program consumption while actually reducing viewership of political news and current affairs (Table A5).

These findings suggest that the policy's effects operate through entertainment exposure rather than propaganda channels. By diverting attention from civic affairs and reducing political news consumption, TV access appears to foster political apathy while simultaneously making existing propaganda more effective through both cognitive and emotional channels. This mechanism helps explain how authoritarian regimes can maintain stability without relying on propaganda investments.

Material Benefits or Gratitude to the Government One might be concerned that the increase in political trust and support may be driven by the short-term economic benefits it provides. By subsidizing the cost of TVs, the policy can improve their sense of well-being.

However, we argue that the improvement in short-term material benefits is unlikely to have lasting effects on households' long-term welfare. Our analysis in Figure 4 shows that the short-term effects are not statistically significant for all of the political outcomes. The persistent increase in political support, political trust, and reduced political conflicts, therefore, are unlikely to be fully driven by material benefits.

One may still worry that individuals who received the subsidy in their early lives would be grateful to the government even in the long run. To further address this concern, we exploit heterogeneity in post-treatment television use. Specifically, we compare treated individuals who gained access to TV but reported zero weekly viewing time (non-watchers) with those who actively consumed TV content (heavy-watchers). If the estimated effect were driven primarily by

gratitude for the subsidy, both groups should display similar increases in political support as they equally benefited from the policy. Table D11 shows otherwise. The increase in government support is concentrated among heavy-watchers, whereas non-watchers exhibit no statistically discernible change. We observe a similar pattern when using respondents' perceived importance of TV in daily life: the treatment effect is absent among those who regard TV as unimportant but is significant among those who view it as important.

In addition, the heterogeneity results in Table 2 also help exclude this channel. Note that gratitude should not vary among what programs people watch on TV. The finding that our effect applies primarily to individuals who watch entertainment programs is another piece of evidence for our main channel. Taken together, these results indicate that the effect operates through media exposure rather than mere economic benefits or gratitude to the subsidy.

Labor-market Changes or Health Improvements TV, as a key source of information, can reduce labor market information asymmetry, broaden individuals' horizons, and facilitate skill acquisition. While this information is less relevant for those not yet in the labor market, it becomes valuable once they enter, helping them secure better jobs and higher incomes. This may explain the baseline observation of a non-significant short-term effect but a significant long-term impact of the HAGC policy on public support and political trust.

However, in China, labor market information and job opportunities are seldom conveyed through TV. To test this formally, Panel A in Table D12 shows no significant impact of HAGC policy on labor force participation, wages, or entrepreneurship. Additionally, using the International Socio-Economic Index (ISEI) and the Standard International Occupational Prestige Scale (SIOPS) as proxies for social status, we find that individuals affected by the policy have lower social status. Though not statistically significant, they are also less likely to become leaders, managers, or hold higher job ranks. This suggests that the policy's long-term political effects are not driven by material gains in the labor market.[31]

In contrast, Panel B reveals that those with greater exposure to the HAGC policy report higher satisfaction with their jobs, income, work environment, and promotion, despite actual worsening occupation status. Moreover, their self-reported status increases with TV exposure despite declines in their actual status. This suggests that entertainment TV has long-term effects on life satisfaction, making individuals more easily satisfied.

Finally, Table D13 shows that early-life exposure to TV also harmed physical health outcomes, an essential dimension of human capital, consistent with Nieto and Suhrcke (2021). This reinforces our argument: although material benefits are limited or even negative, individuals report higher life satisfaction, greater support for government, and stronger political trust, making them less likely to protest and thereby bolstering the durability of authoritarian rule.

31. The negative effect may be ascribed to their worsening cognitive skills and lower education.

7 Conclusion and Discussion

This paper examines how authoritarian states utilize media to resist pressures for political accountability. Moving away from previous studies that focus on information manipulation or political propaganda in autocracies, we highlight how entertainment media can affect citizens' capacity and willingness to hold the state accountable.

Empirically, we study the impact of one of the largest TV subsidy programs. Utilizing cohort-location difference in policy exposure, we find teenagers who were more exposed to TV demonstrate a significant increase in government support when they reach adulthood. Compared with their older cohorts who just missed the policy, TV-affected teenagers also develop higher trust in state apparatus, lower inclination to participate in collective actions, and less demand for redistribution. These changes are consistent with the *illusory accountability effect*: Exposure to entertainment TV during adolescence is associated with lower cognitive performance, reduced educational attainment, and weaker political knowledge, while also increasing satisfaction and reducing emotional aggrievement. These patterns suggest that entertainment media can weaken accountability demand through two mutually reinforcing forces: it reduces citizens' ability to evaluate and challenge the state, while also lowering their willingness to do so by providing low-cost affective satisfaction.

Our findings complement and extend Campante, Durante and Tesei (2022), who show that entertainment can crowd out social and political engagement. Much of the existing literature emphasizes content substitution: entertainment displaces politically relevant information, thereby reducing social capital and political participation (DellaVigna and La Ferrara, 2015; Olken, 2009). We find evidence consistent with this channel, as HAGC gives rise to political apathy. But our results also point to additional mechanisms, including worsening cognitive abilities and improving subjective emotions. These channels help explain why affected citizens may become both less able and less willing to demand social or political change.

Contrasting supply-side analysis on authoritarian stability (e.g., AI-based digital surveillance and Internet censorship), we focus on the demand-side mechanism: the stagnation of civil society induced by the illusory accountability effect of entertainment media. While detrimental to cognitive abilities, education, and civic culture, teenage exposure to entertainment in long run could reduce citizens' demand for democratization, solidifying authoritarian rule in a less noticeable but possibly more effective way. This echoes Chen and Yang (2019)'s argument that "*low demand* for uncensored information is a crucial reason why students don't acquire such information." We show that entertainment media can help shape such low demand by making politics less salient, reducing political knowledge, and increasing satisfaction with the status quo.[32]

32. This demand-side mechanism can also operate in the opposite direction. For example, Enikolopov, Petrova and Sonin (2018) show that social media can increase political mobilization and strengthen accountability in Russia.

Our findings also speak to the changing nature of media control. In an era of information explosion with various sources of information, citizens can switch to independent media for alternative information in the face of intensified propaganda on state media (Durante and Knight, 2012; Grosfeld, Madinier, Sakalli and Zhuravskaya, 2024). Moreover, traditional media control measures, like censorship or manipulation, may easily backfire, causing mass disapproval or protests against the censoring regime (Edmond, 2013; Gläßel and Paula, 2020). Entertainment media offers a different logic of control: rather than directly suppressing information or persuading citizens, it can reduce the demand for accountability by diverting attention, reshaping preferences and cognition, and increasing subjective contentment. In this sense, entertainment media can expand the repression-reaction frontier of authoritarian control. Future research could explore the effectiveness of these new methods and their complementarity with traditional control means, extending the discussion on these "spin dictators" (Guriev and Treisman, 2022).

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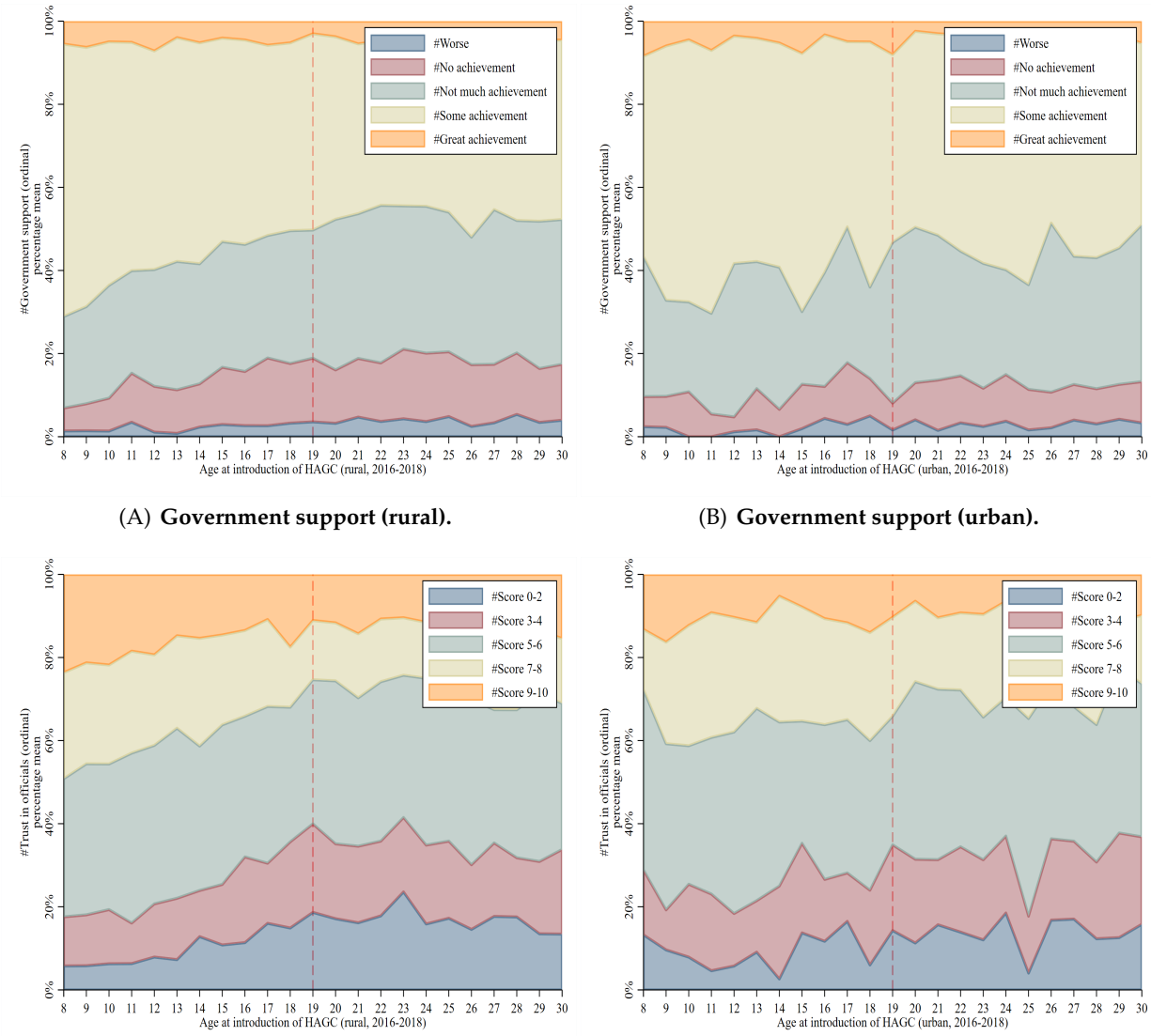
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8 Figures



(A) **Government support (rural).**

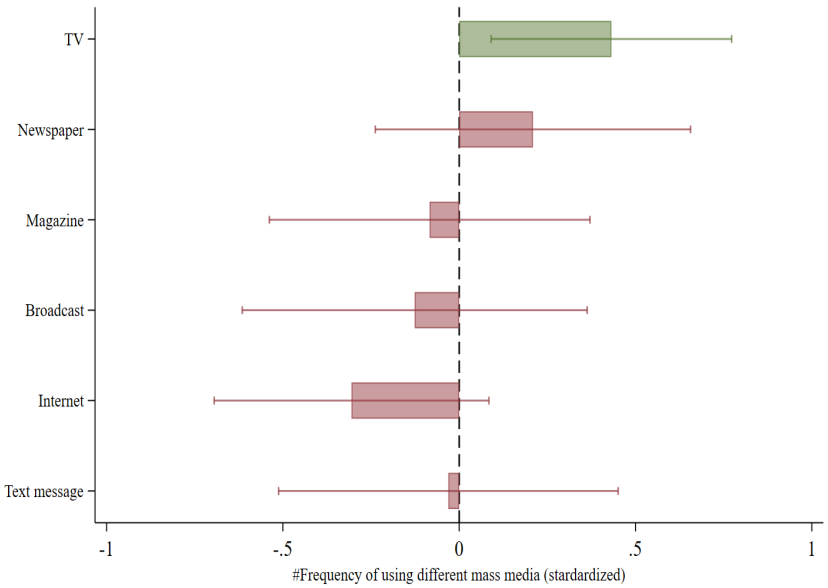
(B) **Government support (urban).**

(C) **Trust in officials (rural).**

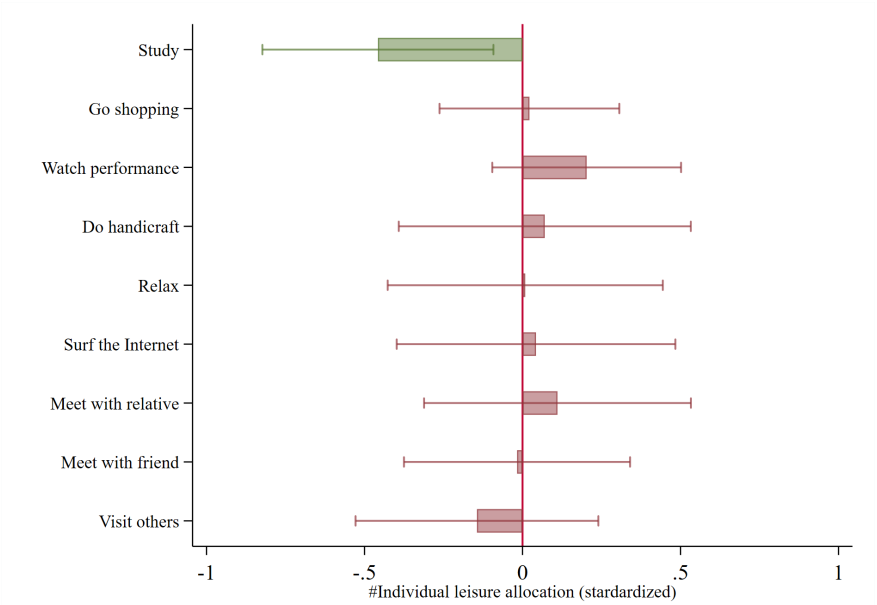
(D) **Trust in officials (urban).**

FIGURE 1 Distribution of Political Attitudes across Cohorts (Age Groups).

Notes: Figure 1 describes the raw data of political attitudes (government support and trust in officials). The data is derived from CFPS from 2016 to 2018. Panel A and B describe the distribution of government support by respondents' ages at the introduction of HAGC in rural and urban areas, respectively. Panel C and D describes trust in government officials by respondents' ages. Different colors represent varying levels of support/trust, with the top yellow areas representing the highest level and the bottom blue areas representing the lowest level.



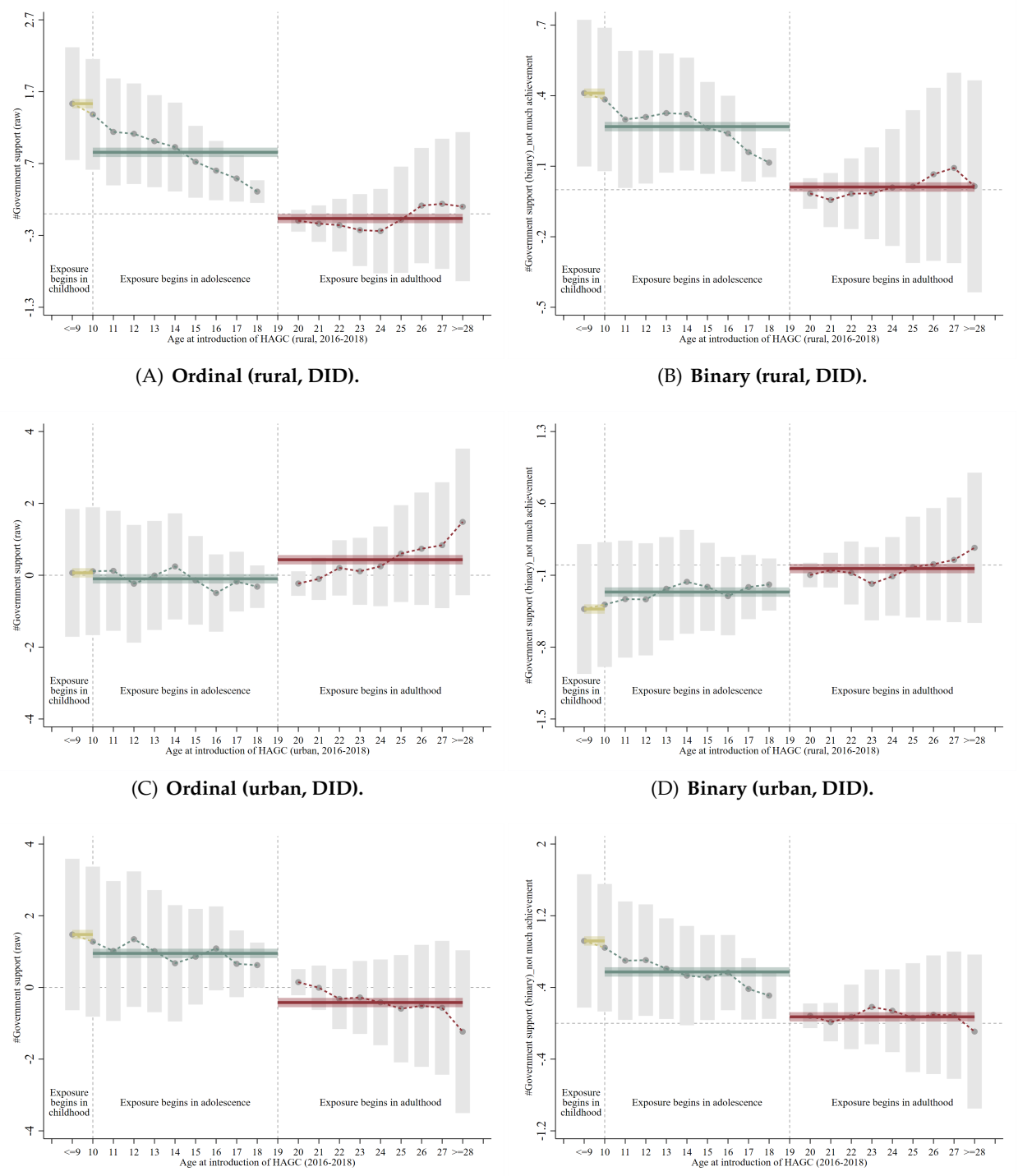
(A) **Frequency of using mass media (short-term).**



(B) **Individual leisure allocation (short-term).**

FIGURE 2 Mechanism: Effects of HAGC on Media Usage and Leisure Allocation.

Notes: Figure 2 presents the effect of HAGC on individuals' usage of different mass media and their leisure allocation. The data is derived from the CGSS dataset, focusing on respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The dependent variables in panels A are the frequency of watching TV, reading newspapers, reading magazines, listening to broadcasts, surfing the Internet, and reading text messages, respectively, while those in panel B are the time spent in studying, going shopping, watching performances, doing handicrafts, relaxing, surfing the Internet, meeting with relatives, meeting with friends, and visiting others. Both Panels standardize dependent variables in the whole sample to compare each other. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The solid line represents the 95% confidence interval.



(A) **Ordinal (rural, DID).**

(B) **Binary (rural, DID).**

(C) **Ordinal (urban, DID).**

(D) **Binary (urban, DID).**

(E) **Ordinal (all, DDD).**

(F) **Binary (all, DDD).**

FIGURE 3 Baseline: Long-term Effects of HAGC on Government Support by Age Groups.

Notes: Figure 3 presents the event study results of exposure to HAGC on long-term government by different ages. Panels A and B present rural samples with DID specification in Equation 4, while Panels C and D present urban samples. Panels E and F use all samples with DDD specification in Equation 5. The dependent variable is government support. Panels A, C, and E use raw scores from 1 to 5. Panels B, D, and F use binary evaluation with score 3-5 (positive evaluation) recorded as 1, otherwise 0. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. The gray shaded area represents the 90% confidence interval.

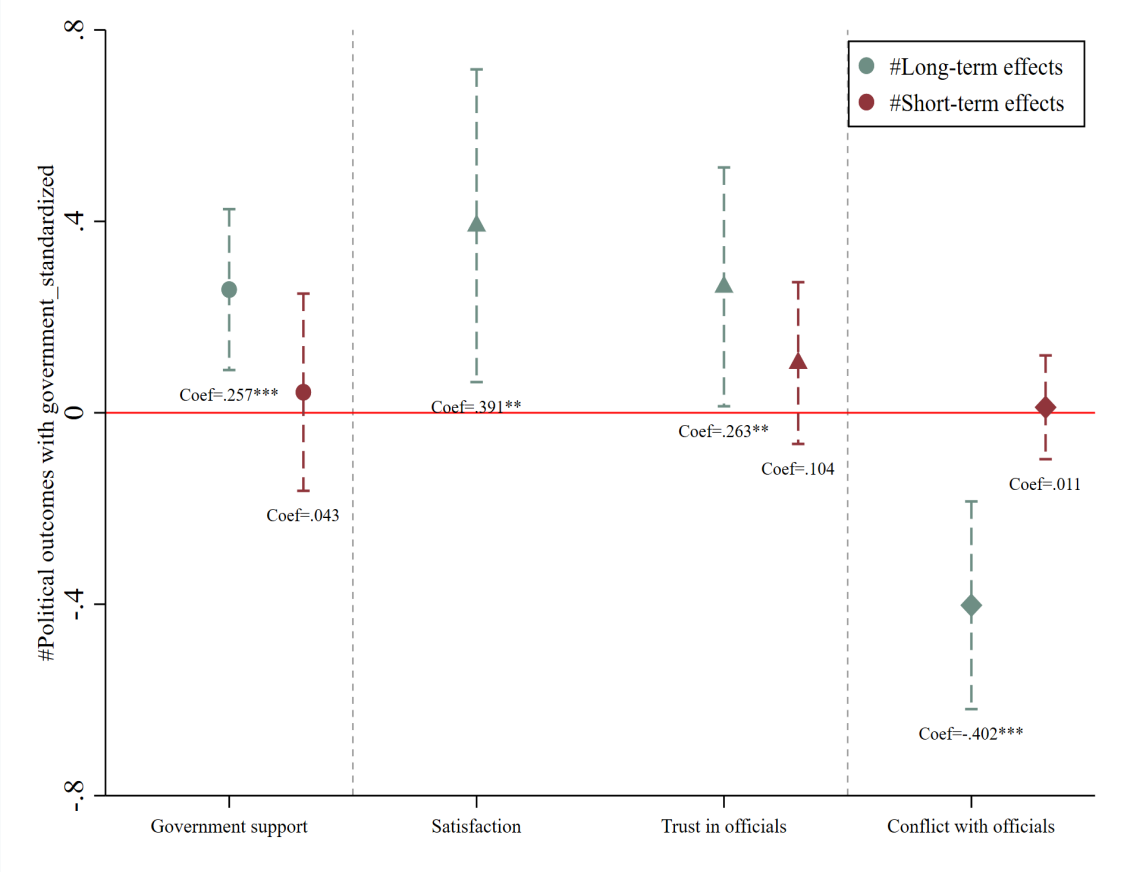
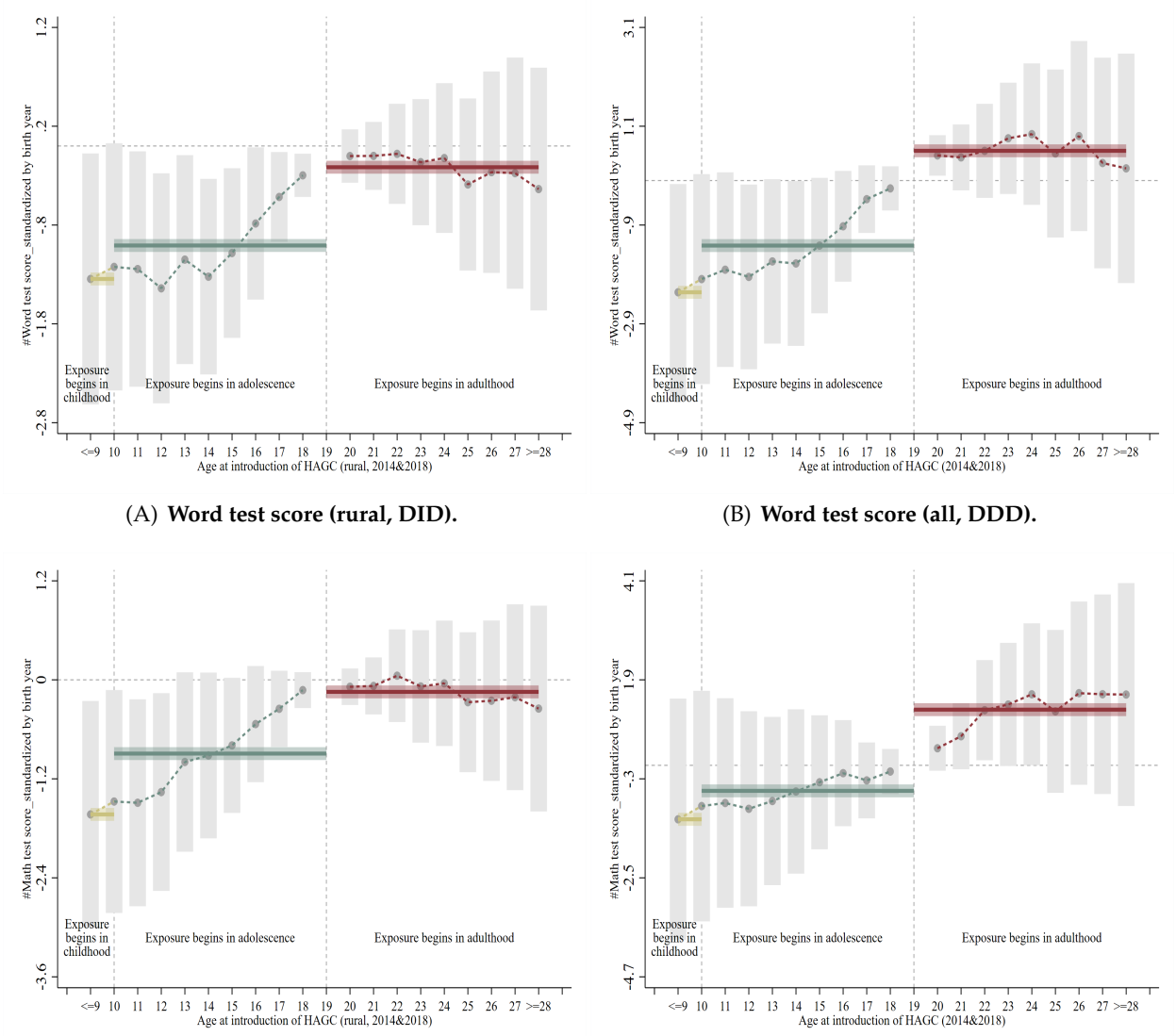


FIGURE 4 Baseline: Comparing Short-term and Long-term Effects of HAGC.

Notes: Figure 4 presents the effects of HAGC on short- and long-term political outcomes, with the data drawn from the CFPS and CSS dataset. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The dependent variables include government support, satisfaction, trust in officials, and conflict with officials. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The point estimates represent the DID specification results in Equation 2, while the dashed line represents the 95% confidence interval.



(A) **Word test score (rural, DID).**

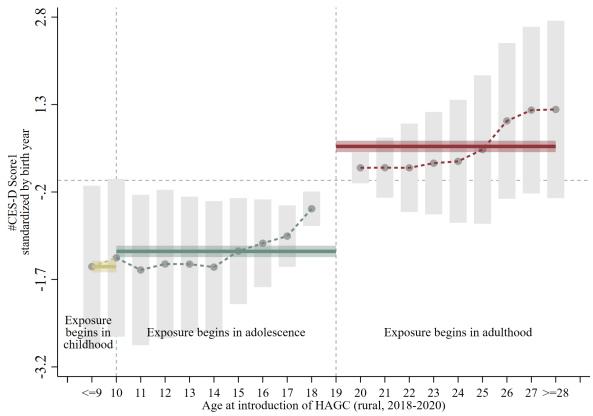
(B) **Word test score (all, DDD).**

(C) **Math test score (rural, DID).**

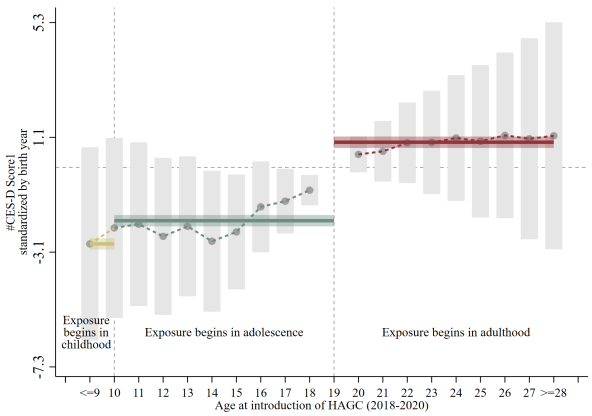
(D) **Math test score (all, DDD).**

FIGURE 5 Mechanism: Long-term Effects of HAGC on Acquired Cognitive Outcomes by Age at First Exposure.

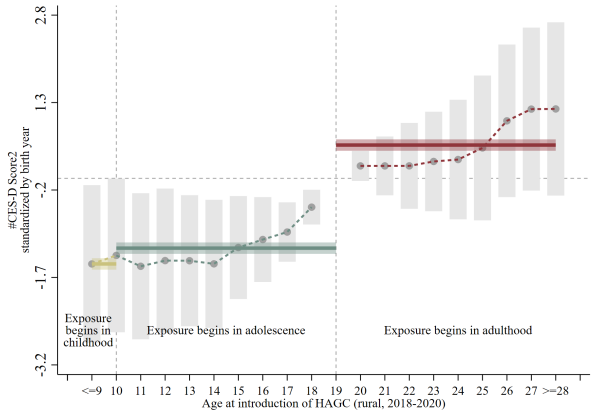
Notes: Figure 5 presents the event study results of cohort-level differential exposure to HAGC on long-term acquired cognitive outcomes. Panels A and C present rural samples with DID specification in Equation 4, while panel B and D use all samples with DDD specification in Equation 5. The dependent variable is word test score in the first two panels, and math test score in the last two panels. Both Panels standardize dependent variables by birth year. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The gray shaded area represents the 90% confidence interval.



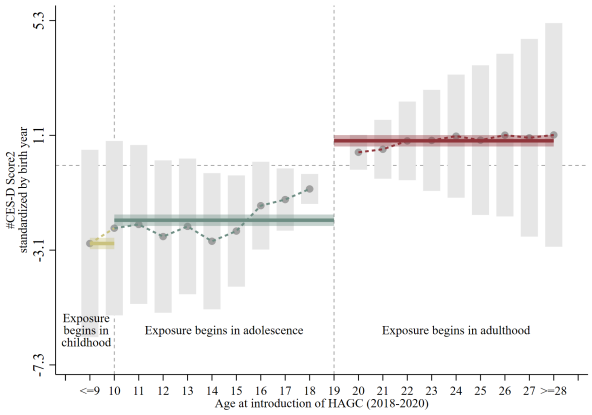
(A) **CES-D score1 (rural, DID).**



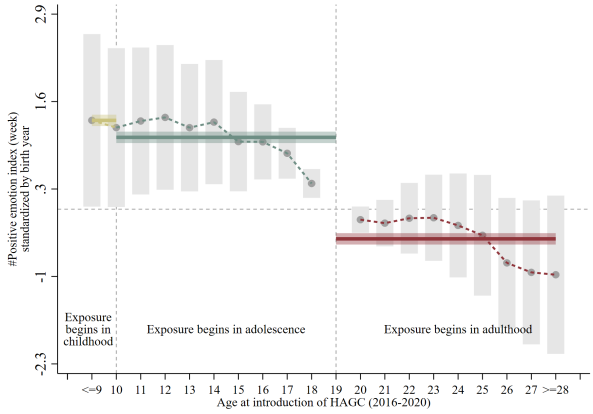
(B) **CES-D score1 (all, DDD).**



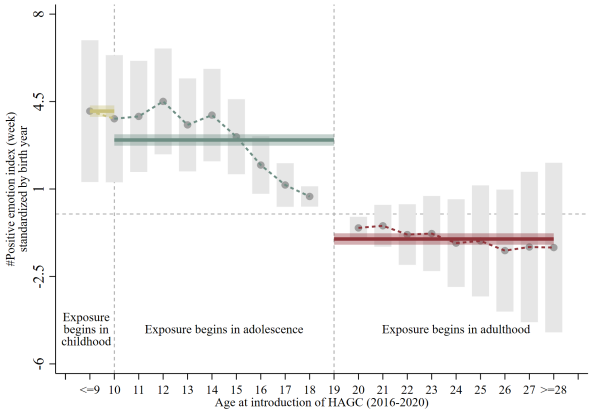
(C) **CES-D score2 (rural, DID).**



(D) **CES-D score2 (all, DDD).**



(E) **Positive Emotion index (rural, DID).**



(F) **Positive Emotion index (all, DDD).**

FIGURE 6 Mechanism: Effects of HAGC on General Emotion by Age at First Exposure.

Notes: Figure 6 demonstrates the event study estimates of cohort-level differential exposure to HAGC on various emotion indicators. Panels A, C, and E present rural samples with DID specification in Equation 4, while panel B, D, and F use all samples with DDD specification in Equation 5. From Panels A to F, the dependent variables are CES-D score 1 (20-question, 80-scale), CES-D score 2 (8-question, 32-scale), and the weekly positive-emotion index, respectively. All dependent variables are standardized by birth cohort. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. Shaded areas indicate 90% confidence intervals.

9 Tables

TABLE 1 Baseline: Government Support

Dependent variable:	Government support			
Sample:	Long-term (2016-2018)		Short-term (2012-2014)	
	DiD	DDD	DiD	DDD
	(1)	(2)	(3)	(4)
Exposure to HAGC	0.230***	0.034	0.038	0.067
	(0.075)	(0.069)	(0.092)	(0.138)
Exposure to HAGC × Rural		0.156***		0.018
		(0.044)		(0.047)
Controls	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Prefecture (now) FE	✓	✓	✓	✓
Province-wave (now) FE	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓
Dependent-variable mean	3.406	3.428	3.375	3.381
Observations	9,034	14,483	8,647	14,274

Notes: Table 1 reports the effect of cohort-level differential exposure to HAGC on long-term government support. The data is derived from the CFPS dataset. Columns (1) and (3) use DID specification in Equation 2 while columns (2) and (4) apply DDD specification in Equation 3. The dependent variable is government support. Columns (1) and (3) consists of respondents with rural *hukou* during HAGC policy while column (2) and (4) use both rural and urban sample. All regressions include individual-level controls (gender, marital status, parental age and education, age squared), predetermined prefectural-level characteristics (phone user ratio, internet user ratio, and percentage of secondary industry in GDP at 2007) interacted with cohort, county FE, cohort FE, prefecture (now) FE, province (now)-wave FE, and province-cohort-specific linear trend. Standard errors are clustered at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE 2 Heterogeneity: Program Broadcast

Dependent variable:	Government support				
	Major program		Representative entertainment programs		
$X =$	News & Information	Entertainment	Television dramas	Reality shows	Musical performances
	(1)	(2)	(3)	(4)	(5)
Exposure to HAGC $\times X$	0.004	0.090***	0.106***	0.100***	0.049*
	(0.042)	(0.031)	(0.035)	(0.035)	(0.028)
Exposure to HAGC	0.228***	0.251***	0.242***	0.254***	0.230***
	(0.069)	(0.085)	(0.075)	(0.082)	(0.074)
Inference robustness (Exposure to HAGC $\times X$)					
Robust p-value			0.00246***	0.00427***	0.08012*
Holm-adjusted p-value			0.00737***	0.00855***	0.08012*
Cauchy-combined p-value				0.00459***	
Controls	✓	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓
Dependent-variable mean	3.406	3.406	3.406	3.406	3.406
Observations	9,034	9,034	9,034	9,034	9,034

Notes: Table 2 reports the heterogeneous effects of cohort-level differential exposure to HAGC on long-term government support. The data is drawn from CFPS. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2 with an interaction term. The moderators (X) are a series of predetermined (2006) TV program broadcast shares with standardization at the provincial level. "News & Information" includes news, legal, finance, and life programs while "Entertainment" includes TV series, drama, movie, music and other entertainment programs. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. The moderators (X) are provincial level and thus are absorbed by the fixed effects. Standard errors are clustered at the county level. The Holm-adjusted p-values control the family-wise error rate (Holm, 1979). The Cauchy-combined p-value provides a joint test across outcomes (Liu and Xie, 2020). ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE 3 Mechanism: Government Accountability

Dependent variables:	Demand for redistribution		Demand for democratic reform	
	Increase protection for the underclass	Tax the rich more	Change the unreasonable social structure	Social democratic reform
	(1)	(2)	(3)	(4)
Exposure to HAGC	-1.969***	-0.126	-0.456**	-0.387*
	(0.556)	(0.345)	(0.205)	(0.215)
Inference robustness (Exposure to HAGC)				
Robust p-value	0.00040***	0.71495	0.02612**	0.07186*
Holm-adjusted p-value	0.00159***	0.71495	0.07837*	0.14372
Cauchy-combined p-value	0.00156***			
Controls	✓	✓	✓	✓
2007 predetermined # cohort	✓	✓	✓	✓
FEs	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓
Observations	2,277	6,137	6,137	6,137

Notes: Table 3 reports the effect of cohort-level exposure to the HAGC policy on government accountability, measured by respondents' demands for redistribution and democratic reform. Data are drawn from the CSS dataset. The dependent variables include support for more protection for the underclass, higher taxation on the rich, restructuring of social hierarchy, and democratic reform, respectively. For comparison, all these variables are standardized. All regressions use the DID specification in Equation 2, with standard controls, fixed effects, and provincespecific trends as in Table 1. Standard errors are clustered at the county level. The Holm-adjusted p-values control the family-wise error rate (Holm, 1979). The Cauchy-combined p-value provides a joint test across outcomes (Liu and Xie, 2020). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Online Appendix for "Manufacturing Obedience: Entertainment Media and Authoritarian Support"*

Haotian Bai Siyuan Fan Wenyi Lu Shangkun Xie

Appendix A More on Data and Background **A2**

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Appendix A More on Data and Background

A.1 Background Information

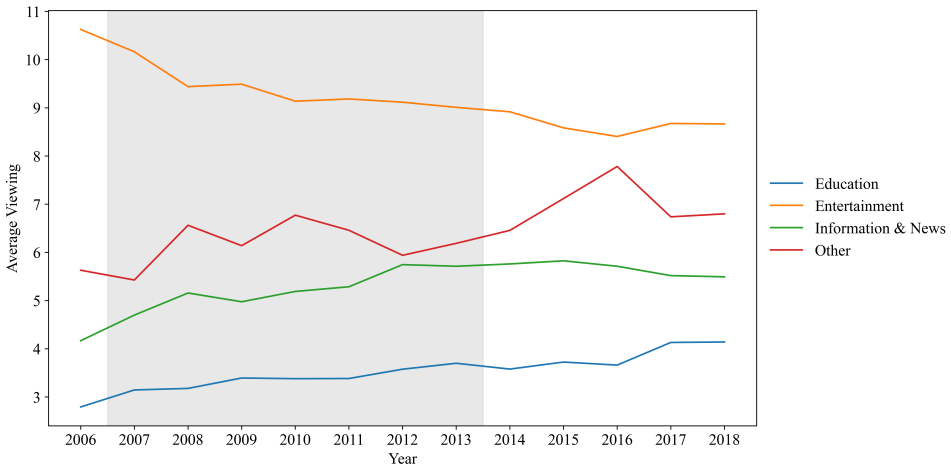


FIGURE A1 Average Share of Viewing Contents of TV in China

Notes: The data is drawn from the *China TV Rating Yearbook*. We aggregate it at the province-year-category level. Viewing values represent yearly averages across provinces for each program category. "Education" includes educational and youth programs. "Entertainment" includes TV series, drama, movie, music, foreign entertainment and other programs. "Information & News" includes news, legal, finance, and life programs.

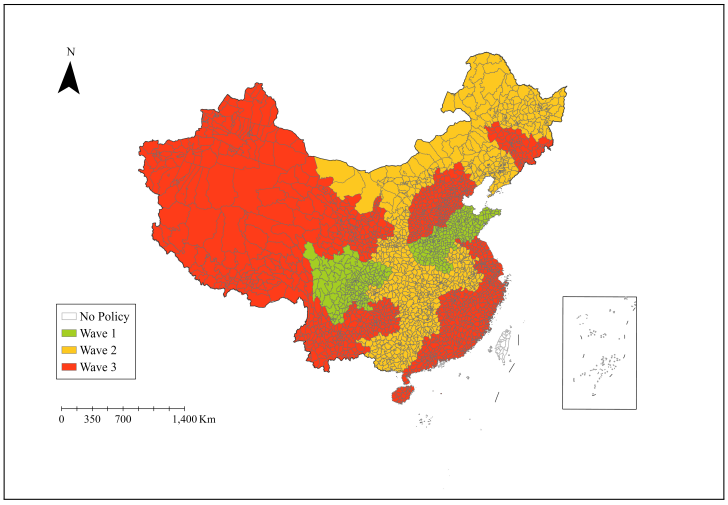


FIGURE A2 The Staggered Implementation of HAGC Policy.

Notes: Figure A2 maps the implementation process of the "Home Appliances going to the Countryside" (HAGC) policy. The three waves were implemented in 2007, 2008, and 2009 respectively.



(A) **HAGC Example.**



(B) **TV Viewers.**

FIGURE A3 Photos of the HAGC Policy in Rural Areas.

Notes: Panel A shows how firms promoted TVs among rural residents, with the red slogan saying "expanding demand, promoting economic development, keeping economic growth, through Home Appliances Going to Countryside". See CCTV for reports. Panel B is a photo showing Chinese rural residents watching Olympic games. They usually gathered together watching TV programs, as many of them do not possess a color TV at home. See The New York Times for reports.



(A) **TVs before HAGC Policy in Rural Areas.**



(B) **TVs after HAGC Policy in Rural Areas.**

FIGURE A4 Photos of TVs before and after HAGC Policy in Rural Areas.

Notes: Panel A illustrates the televisions in Chinese rural areas prior to the implementation of HAGC, characterized by bulky designs, cable features. Panel B presents the situation after the HAGC policy, with a prominent display of contemporary flat-screen TVs in a well-stocked entertainment appliance store. The promotional banners promote the HAGC policy and the store itself, calling for rural consumers to purchase color TVs. This juxtaposition highlights the transformative impact of the HAGC policy in facilitating access to color TV in rural China.

A.2 Detailed Data Description

A.2.1 Detailed Variable Information: CFPS Cognitive Tests

The cognitive battery in CFPS comprises two sets of questionnaires, encompassing four distinct types of tasks: word tests (or verbal tests), mathematics tests, memory tests, and numerical tests. The word and mathematics assessments primarily evaluate respondents' educational achievements (acquired skills), while the memory and numerical tests are designed to capture individuals' innate cognitive abilities. Overall, rural samples perform worse than urban samples.

Word and Mathematics Tests The word and mathematics assessments are structured based on the Guttman Scale, where items are arranged in increasing order of difficulty, such that a correct response to a particular item implies the ability to answer all easier items correctly. Conversely, an incorrect response indicates the inability to solve that item and all subsequent challenging items. Specifically, in the word (mathematics) test, respondents are required to recognize 34 Chinese characters (24 mathematics problems) presented in ascending order of difficulty. The test is terminated if a respondent consecutively answers three items incorrectly. The score is determined by the position of the most difficult character (mathematics problem) answered correctly. To minimize interference and practice effects within households, different respondents within the same household answer different sets of questions in a single survey wave, and the same respondent is assigned different sets of questions in follow-up surveys.

Memory and Numerical Tests The memory test is adopted from the Health and Retirement Study (HRS). During the test, interviewers read aloud ten commonly encountered words (e.g., mountain, rice, river) to the respondents, and then respondents are asked to recall as many words as possible, with the number of correctly recalled words constituting the immediate memory score. Five minutes later, respondents are again prompted to recall the same ten words, and the number of correctly recalled words constitutes the delayed memory score. The test does not require respondents to recall the words in the order presented. The numerical test is conducted in two stages. In the first stage, respondents answer three sequence problems, resulting in a score ranging from 0 to 3 based on the number of correct answers. In the second stage, individuals with more correct answers in the first stage will receive more challenging tests. CFPS provides sequence test scores calculated using the Rasch model based on Item Response Theory. These scores are independent of the specific

items answered and effectively reflect the respondents' true cognitive abilities.

A.2.2 Detailed Variable Information: CFPS CES-D Tests

The Center for Epidemiological Studies Depression (CES-D), developed by Radloff (1977), is one of the most widely utilized instruments for assessing depressive symptoms. In CFPS, respondents are required to self-evaluate the frequency of depressive symptoms experienced over the past week. The response options are as follows: "rarely or none (less than one day)" (scored as 1 point), "sometimes (1-2 days)" (scored as 2 points), "often (3-4 days)" (scored as 3 points), and "most or all of the time (5-7 days)" (scored as 4 points). The total CES-D score is calculated by summing the scores of all items, with higher scores indicating more severe depression. CFPS employs two scoring methodologies: an 8-item short form with scores ranging from 8 to 32, and a 20-item full form with scores ranging from 20 to 80. The specific items are categorized as follows:

1. Physical symptoms: bothered; poor appetite; inability to concentrate; difficulty doing things; poor sleep; less talking; inability to continue living.

2. Depressive emotions: frustrated; down; defeated; scared; lonely; want to cry; sad.

3. Positive emotions: not worse than others; hopeful; happy; pleased.

4. Interpersonal relationships: people are unfriendly; people don't like me.

Notably, the items pertaining to positive emotions are reverse-coded to correctly reflect depressive symptomatology.

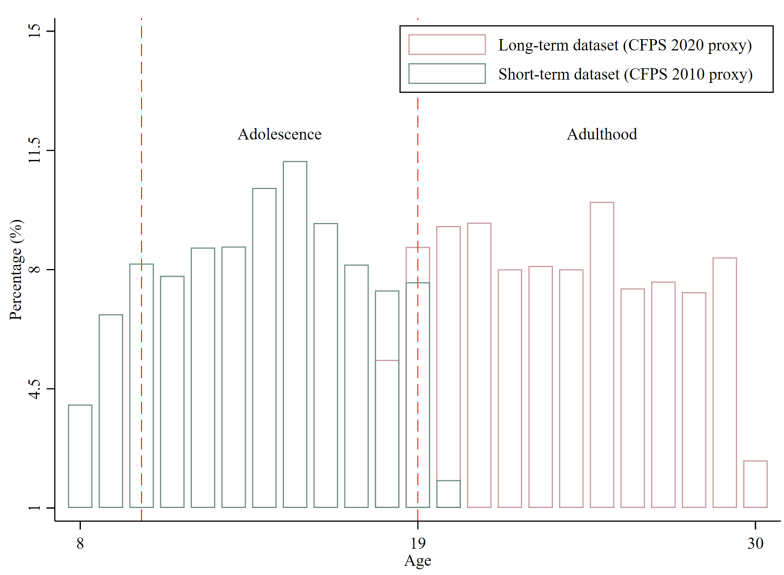
A.2.3 Summary Statistics

TABLE A1 Summary Statistics

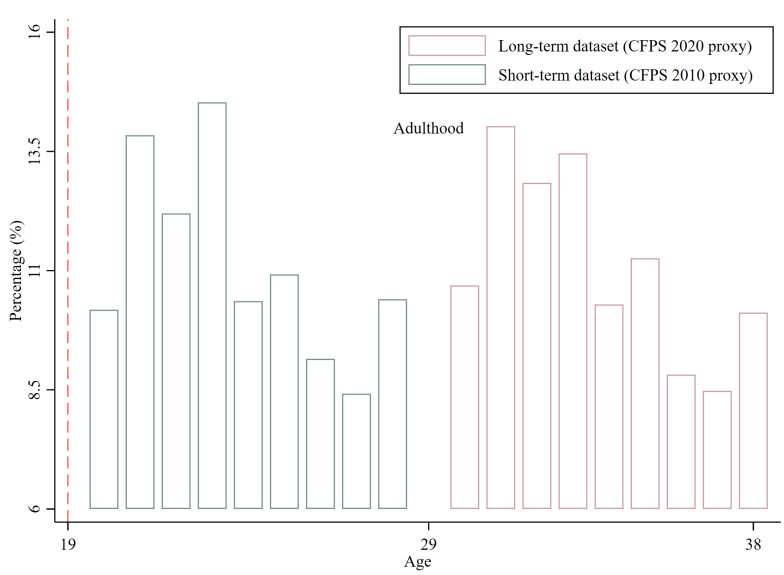
	Mean	Std.Dev.	Min	Max	N	Waves
	(1)	(2)	(3)	(4)	(5)	(6)
Political outcomes						
Government support	3.391	0.879	1	5	17,699	2,3,4,5
Trust in cadres	4.802	2.500	0	10	17,943	2,3,4,5
Conflict with cadres	0.026	0.161	0	1	13,632	2,3,4
Cognitive outcomes						
Word test score	23.446	8.077	0	34	14,129	1,3,5
Math test score	13.140	5.731	0	24	14,129	1,3,5
Emotional outcomes						
CES-D score 1 (20-question, 80-scale)	32.742	6.958	22	72	7,885	5,6
CES-D score 2 (8-question, 32-scale)	22.363	2.826	5	25	7,883	5,6
Emotion index (weekly)	13.339	3.512	8	32	7,880	2,4,5,6
Emotion index (monthly)	17.726	2.227	5	20	17,382	1,3
Educational outcomes						
Education level	3.372	1.168	1	7	4,771	5
Education year	9.834	3.635	0	18	4,771	5
College and above qualification	0.148	0.355	0	1	4,771	5

Notes: Table A1 reports the summary of statistics of the main outcome variables. The data is drawn from CFPS. The sample consists of respondents born in 1982-2002 with rural hukou during HAGC policy. Survey wave 1-6 respectively represent 2010, 2012, 2014, 2016, 2018, 2020. For time-invariant variables (such as educational outcome), we use single wave after imputation to avoid double counting.

A.3 Identification Assumptions



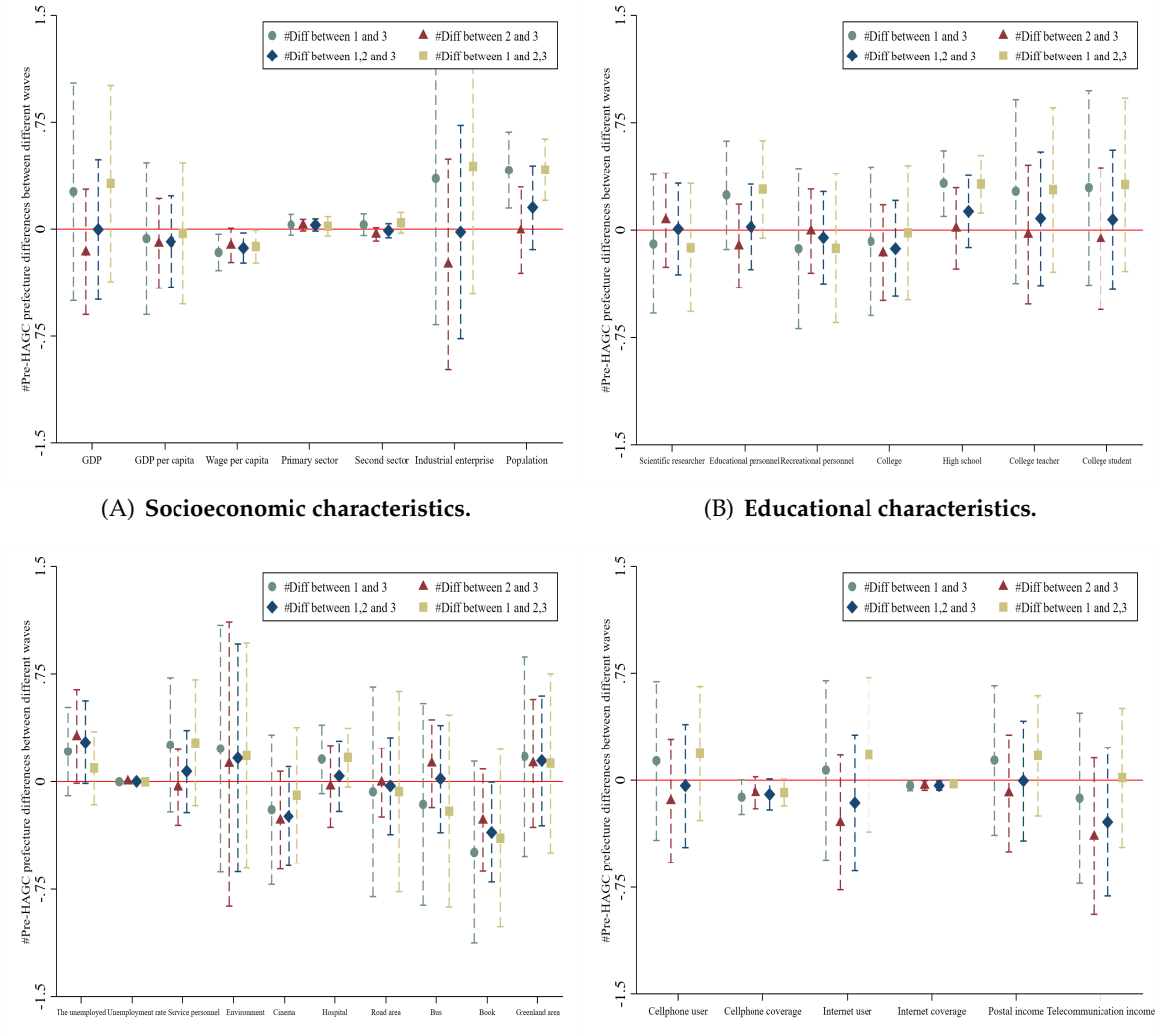
(A) **Treated group.**



(B) **Control group.**

FIGURE A5 Distribution of Respondents' Ages in Short- and Long-term Datasets.

Notes: Figure A5 shows the age distributions in short-term (2010 proxy) and long-term (2020 proxy) datasets in CFPS, respectively. The sample consists of respondents with rural *hukou* during HAGC policy. Panel A shows the treated group distribution while panel B shows that in control group.



(A) **Socioeconomic characteristics.**

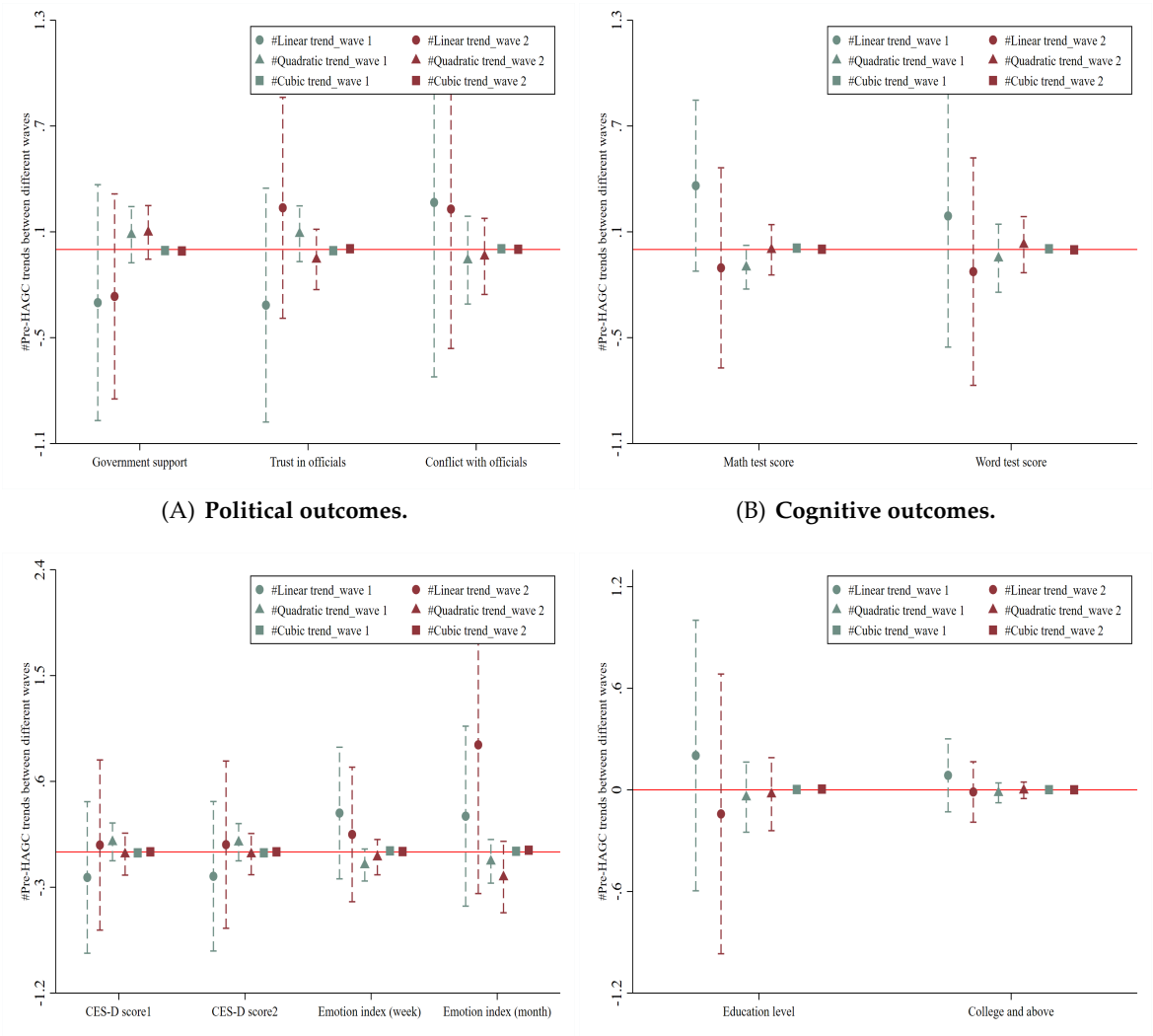
(B) **Educational characteristics.**

(C) **Social security and infrastructure characteristics.**

(D) **Information Technology characteristics.**

FIGURE A6 Pre-HAGC Prefecture Differences between Different Waves.

Notes: Figure A6 shows the pre-HAGC (2007 proxy) prefectural characteristic differences between different HAGC-waves. The data is derived from China Prefecture Statistical Yearbook (2007). The circles, triangles, diamonds, and squares in each panel respectively show the differences of wave 1 and 3, wave 2 and 3, wave 1&2 and 3, wave 1 and 2&3. The dependent variables from panel A to D are socioeconomic factors (GDP, per capita GDP, per capita wage, primary and second sector outputs, industrial enterprise number, population size), educational factors (scientific and educational personnel, entertainment personnel, number of colleges, senior high schools, college teachers, college students), social security and infrastructure factors (unemployment figures, social service personnel, environmental investments, cinemas, hospitals, road area, bus availability, library collections, green areas), and information technology conditions (number/coverage of mobile/Internet users, postal and telecommunication incomes), respectively, all logged where applicable. Standard errors are clustered at province level. The dashed line represents the 95% confidence interval.



(A) **Political outcomes.**

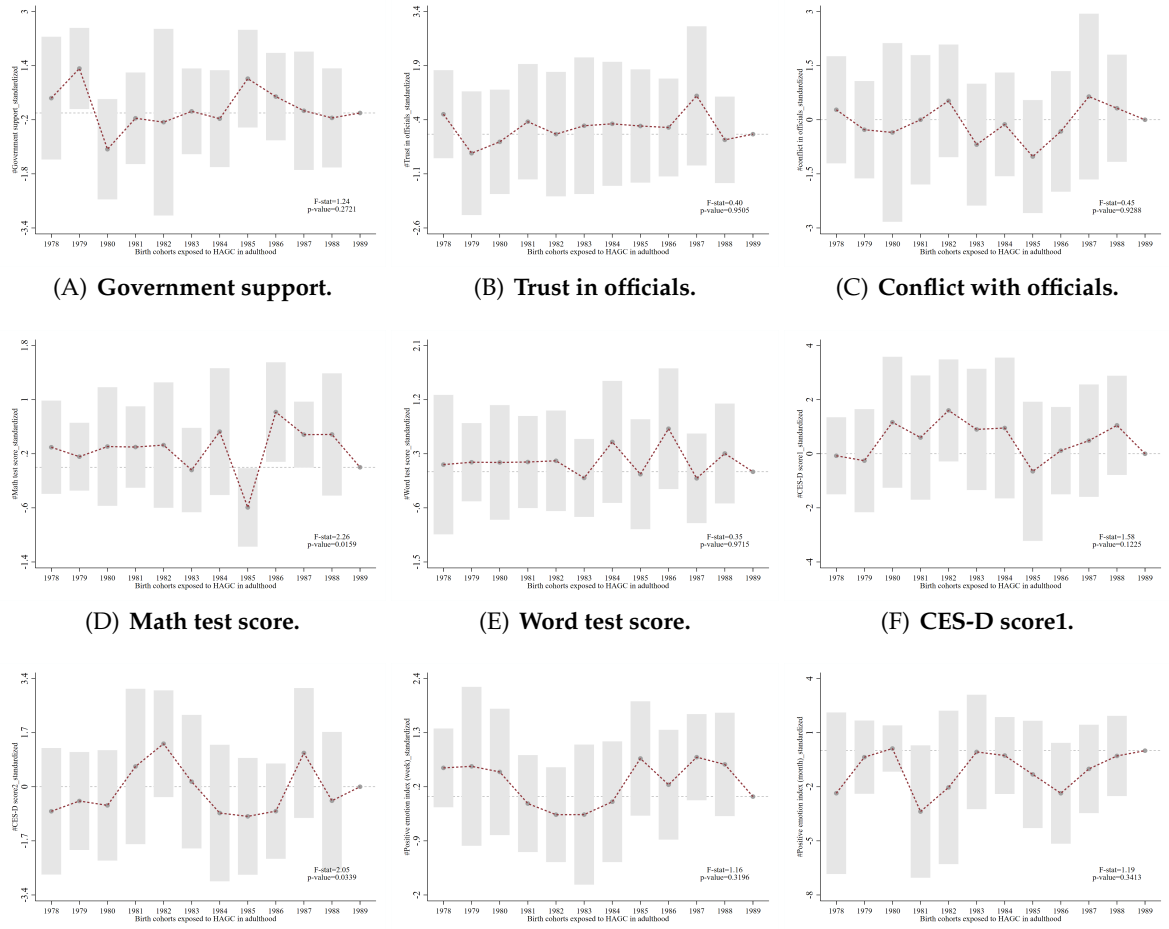
(B) **Cognitive outcomes.**

(C) **Emotion outcomes.**

(D) **Educational outcomes.**

FIGURE A7 Pre-HAGC Characteristic Trends between Different Waves.

Notes: Figure A7 shows the pre-HAGC trends between different HAGC-waves. The data is drawn from CFPS. The sample consists of respondents born in 1982-1989 with rural *hukou* during HAGC policy to make sure that nobody was exposed to HAGC policy during adolescence. Every regression includes a series of interaction terms (cohort linear tread with treated wave dummies (wave 3 omitted), cohort quadratic tread with treated wave dummies(wave 3 omitted), cohort cubic tread with treated wave dummies(wave 3 omitted)). The circles, triangles, and squares in each panel respectively show the linear/quadratic/cubic trend differences between different treated waves (teal for wave 1 and marron for wave 2) and wave 3 (as baseline group). The dependent variables from panel A to D are respectively government support, trust in officials, conflict with officials, math test score, word test score, CES-D scores (simple & full versions), emotion index (weekly & monthly), education level, and college and above qualification. All the dependent variables are standardized. Control variables and fixed effects are the same as in Table 1. Standard errors are clustered at the county level. The dashed line represents the 95% confidence interval.



(A) **Government support.** (B) **Trust in officials.** (C) **Conflict with officials.**

(D) **Math test score.** (E) **Word test score.** (F) **CES-D score1.**

(G) **CES-D score2.** (H) **emotion index (weekly).** (I) **emotion index (monthly).**

FIGURE A8 Pre-HAGC Trends Examination in Counties with Different Timing of HAGC Adoption.

Notes: Figure A8 demonstrates the cohort trends in counties with different timing of HAGC. The sample comes from CFPS dataset and includes the 1978-1989 birth cohorts with rural *hukou* status. Each figure plots the coefficients and the 95% confidence interval on the interactions between indicator variables for cohort groups and the year of HAGC adoption from a separate regression. The analysis is at the county-birth level. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The 1989 cohorts are the omitted group so that plotted coefficients demonstrate whether pre-reform characteristics by every period differ by the year of HAGC adoption.

A.4 Identifying the Treatment Margin

TABLE A2 Policy Effectiveness: TV

Dependent variables:	Color TV			Black and white TV		
	Binary (yes=1)	Number	Value (logged)	Binary (yes=1)	Number	Value (logged)
	(1)	(2)	(3)	(4)	(5)	(6)
HAGC	0.089***	0.093**	0.114**	0.025*	0.031*	0.052
	(0.028)	(0.041)	(0.049)	(0.013)	(0.018)	(0.044)
Inference robustness (HAGC)						
Robust p-value	0.00148***	0.02331**	0.01999**	0.05447*	0.08503*	0.23728
Holm-adjusted p-value	0.00444***	0.03998**	0.03998**	0.16341	0.17006	0.23728
Cauchy-combined p-value		0.00390***			0.08839*	
Controls	✓	✓	✓	✓	✓	✓
2007 predetermined # year	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Dependent-variable mean	0.533	0.588	7.541	0.362	0.442	6.944
Observations	9,264	9,264	9,264	9,264	9,264	9,264

Notes: Table A2 reports the effect of the HAGC policy on the TV number including both color TV and black and white TV. Data are drawn from the CHNS dataset (2004-2009). The dependent variables are dummies for TV ownership, TV number in family, and total monetary value of TV, respectively. All regressions use a staggered DID specification, with standard controls, province fixed effect, and year fixed effect. Standard errors are clustered at the county level. The Holm-adjusted p-values control the family-wise error rate (Holm, 1979). The Cauchy-combined p-value provides a joint test across outcomes (Liu and Xie, 2020). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A3 Policy Effectiveness: Home Appliances

Dependent variables:	# of TV		# of other home appliances					
	Color TV	B&W TV	Washing machine	Refrigerator	Electric fan	Camera	Landline phone	Mobile phone
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HAGC	0.112***	0.051*	0.034*	0.022	0.019	0.004	0.028	0.037*
	(0.029)	(0.030)	(0.019)	(0.018)	(0.022)	(0.018)	(0.018)	(0.022)
Inference robustness (HAGC)								
Robust p-value	0.00011***	0.08913*	0.07354*	0.22162	0.38779	0.82414	0.11981	0.09260*
Holm-adjusted p-value	0.00022***	0.08913*	0.44123	0.66487	0.77558	0.82414	0.47926	0.46302
Cauchy-combined p-value	0.00022***		0.17114					
Controls	✓	✓	✓	✓	✓	✓	✓	✓
2007 predetermined # year	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	18,511	18,511	18,511	18,511	18,511	18,511	18,511	18,511

Notes: Table A3 reports the effect of the HAGC policy on the number of home appliances. Data are drawn from the NFP dataset (2004-2009). The dependent variables are the number of color TV, black and white TV, washing machine, refrigerator, electric fan, camera, landline phone, and mobile phone with normalization, respectively. All regressions use a staggered DID specification, with standard controls, province fixed effect, and year fixed effect. Standard errors are clustered at the county level. The Holm-adjusted p-values control the family-wise error rate (Holm, 1979). The Cauchy-combined p-value provides a joint test across outcomes (Liu and Xie, 2020). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A4 Policy Effectiveness: Entertainment Programs (Short-term)

Dependent variables:	Entertainment TV program		
	Film	Drama	Anime
	(1)	(2)	(3)
Exposure to HAGC	0.188**	0.089**	0.055**
	(0.092)	(0.038)	(0.025)
Controls	✓	✓	✓
2007 predetermined # year	✓	✓	✓
FEs	✓	✓	✓
Province-cohort trend	✓	✓	✓
Observations	3,415	4,413	3,855

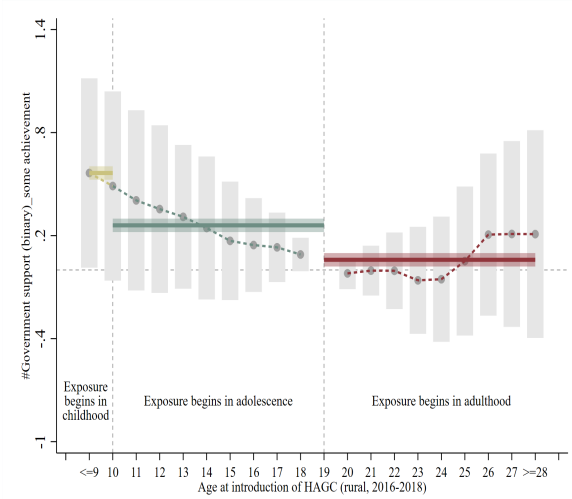
Notes: Table A4 reports the effect of cohort-level exposure to the HAGC policy on the frequencies of watching different TV programs. Data are drawn from the CGSS dataset (2008). The dependent variables are the frequencies of watching films, dramas, and anime with normalization, respectively. All regressions use the DID specification in Equation 2, with standard controls, fixed effects, and provincespecific trends as in Table 1. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A5 Policy Effectiveness: Entertainment Programs (Long-term)

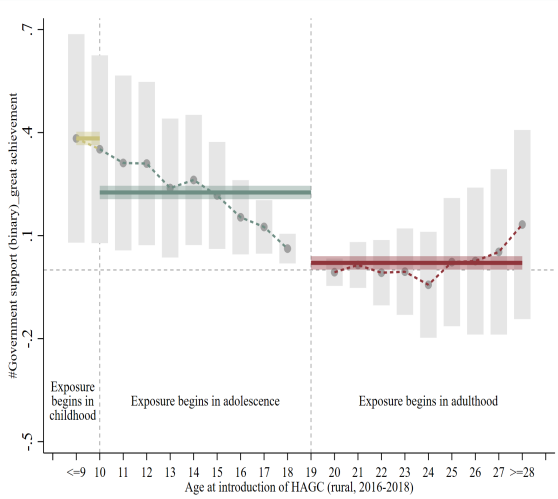
Dependent variable:	The level of Preferences for TV programs				
	(1)	(2)	(3)	(4)	(5)
Panel A.					
	Entertainment	Soap opera	Historical drama	Theater and opera	Action, horror etc. movie
Exposure to HAGC	1.232***	1.773***	2.082***	0.686	0.695
	(0.464)	(0.573)	(0.649)	(0.825)	(0.537)
Dependent-variable mean	3.451	3.375	3.499	3.559	2.939
Observations	2,277	2,277	2,277	2,277	2,277
Panel B.					
	Science and education	Documentary	Sport	Political and social news	Current affair commentary
Exposure to HAGC	-0.086	-0.867	-0.933*	-1.292**	-0.934*
	(0.652)	(0.545)	(0.490)	(0.635)	(0.488)
Dependent-variable mean	3.255	2.949	3.167	2.531	2.533
Observations	2,277	2,277	2,277	2,277	2,277
Controls	✓	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓

Notes: Table A5 reports the long-term effects of HAGC exposure on one's choice of viewing different TV programs. The Data is drawn from ISCIU. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The dependent variables are preferences for different TV programs (scores 1-4). The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

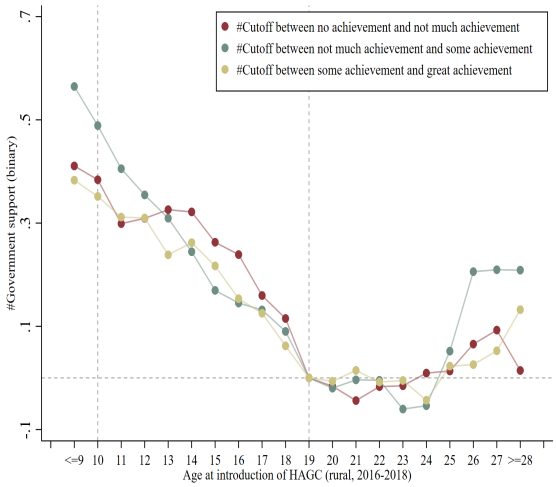
Appendix B More on Baseline



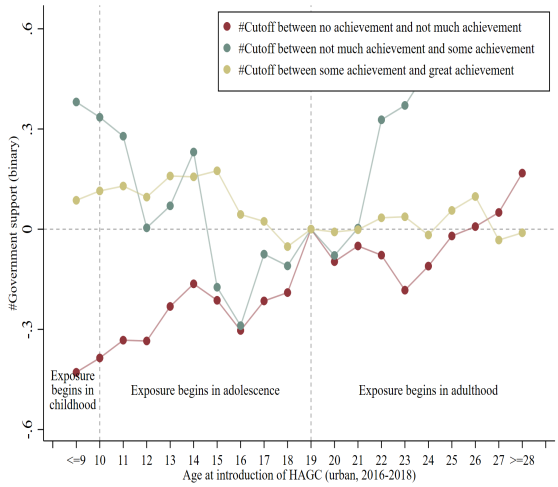
(A) **Some achievement (rural, DID).**



(B) **Great achievement (rural, DID).**



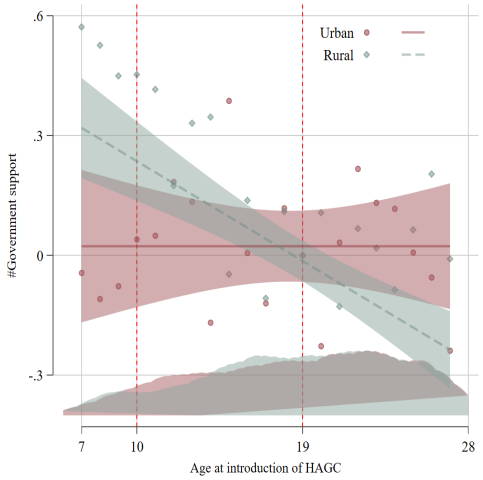
(C) **Alternative Cut-off (rural, DID).**



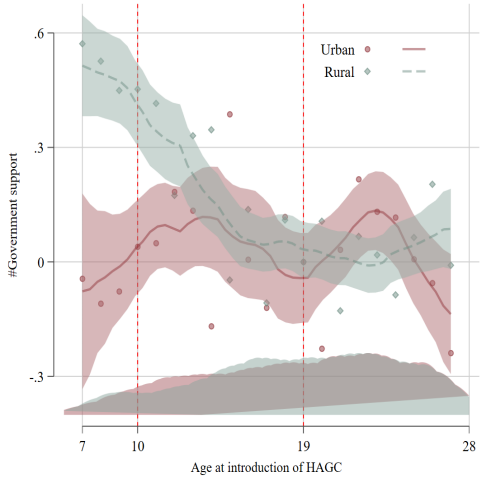
(D) **Alternative Cut-off (urban, DID).**

FIGURE B1 More on Baseline: Long-term Effects of HAGC on Government Support.

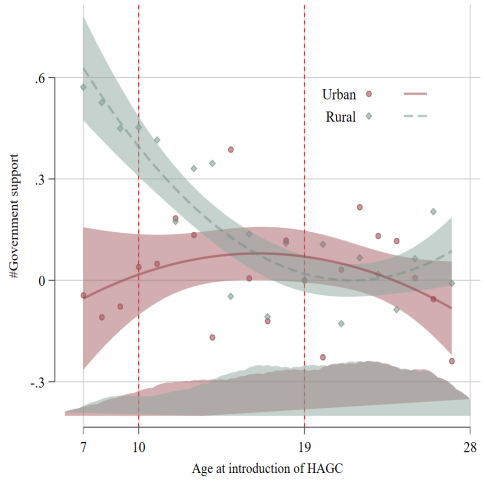
Notes: Figure B1 presents the event study results of cohort-level differential exposure to HAGC on long-term government support with binary setting using different thresholds. The data is drawn from CFPS. All the regressions use event study specification in Equation 4. The dependent variable is government support. Panels A and B use binary evaluation with scores 4-5, and 5 as 1, respectively. Panels C and D summarize the results for the rural and urban samples under three different cutoff settings of the dependent variable. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The gray shaded area represents the 90% confidence interval.



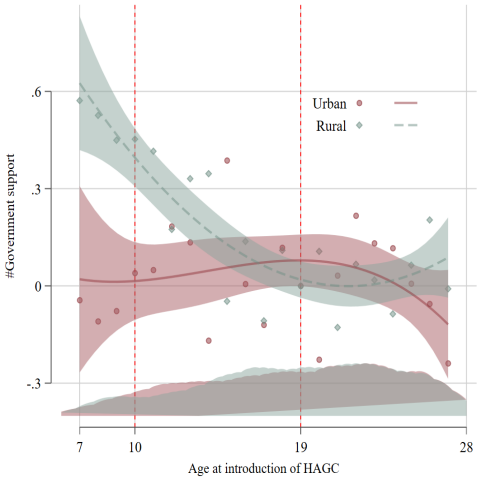
(A) **Quantile estimation.**



(B) **Local polynomial estimation.**



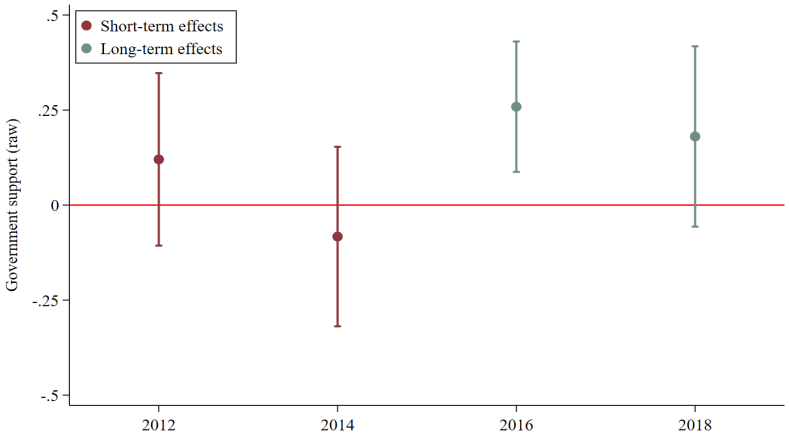
(C) **Quadratic polynomial estimation.**



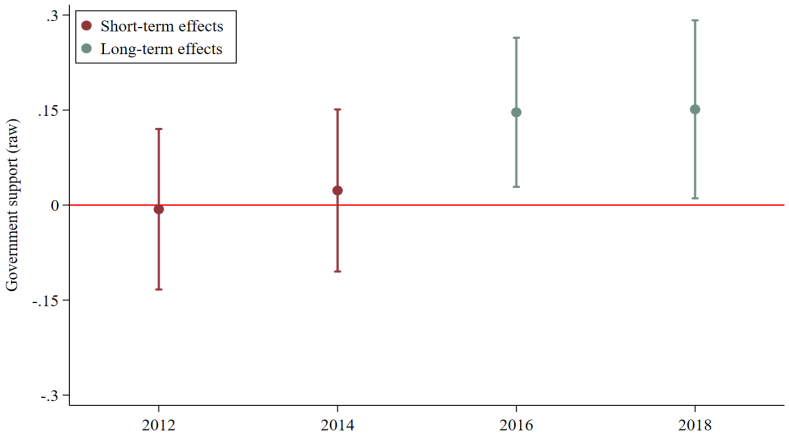
(D) **Cubic polynomial estimation.**

FIGURE B2 Baseline: Relationship between HAGC and Long-term Government Support by Age Groups.

Notes: Figure B2 presents the relationship between HAGC policy and long-term government support by different ages. The data is derived from the CFPS dataset. Panels A to D respectively use quantile estimation (40% as the cutoff), local polynomial estimation, quadratic polynomial estimation, and cubic polynomial estimation. The analysis is at the county - exposure-age level. The shaded area represents the 90% confidence interval, and the density shading along the horizontal axis depicts the sample distribution.



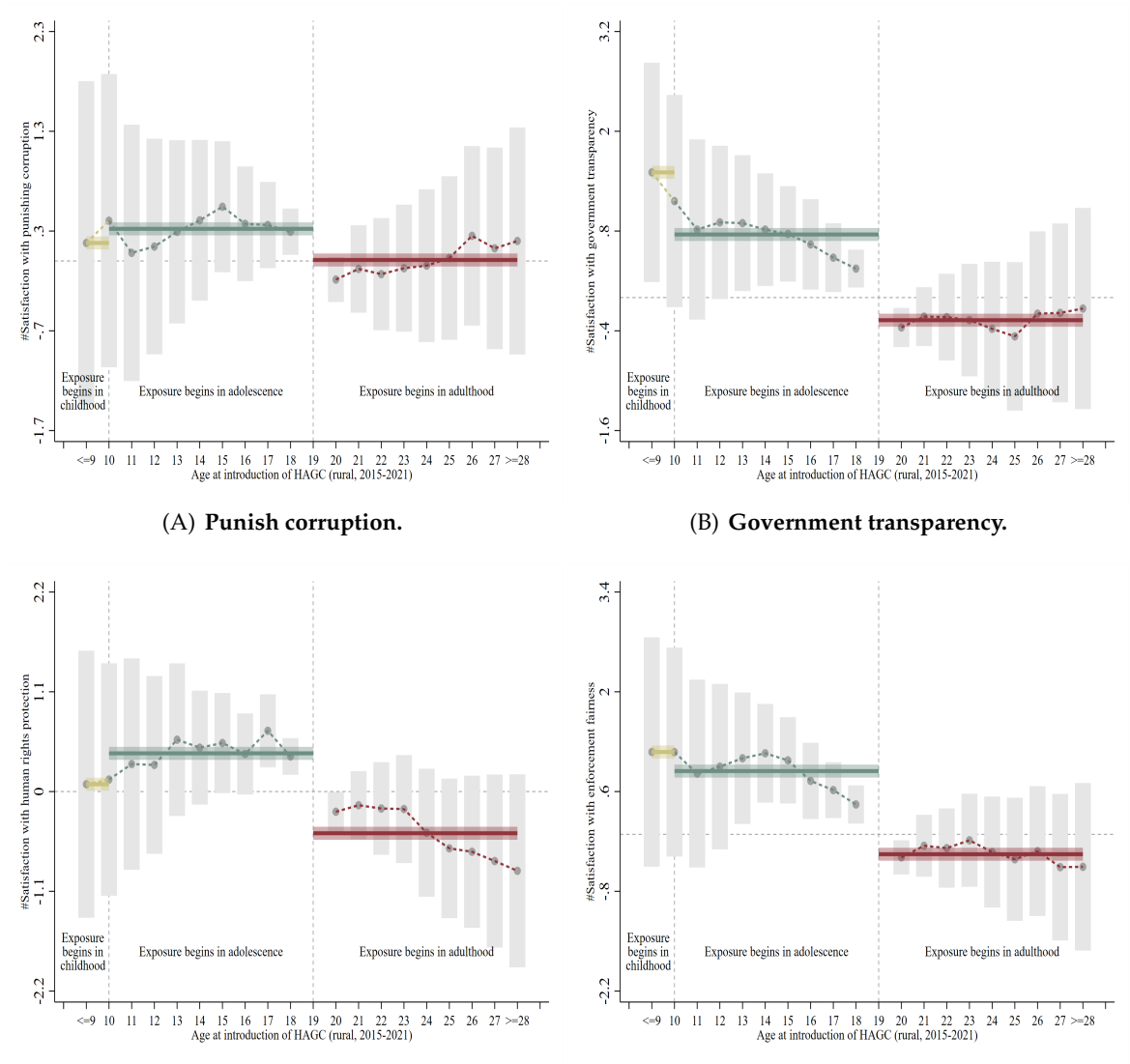
(A) **Ordinal (rural sample, DID).**



(B) **Ordinal (pooled sample, DDD).**

FIGURE B3 Dynamic Effects of HAGC on Government Support

Notes: Figure B3 presents wave-specific estimates of the effect of HAGC on government support using different survey waves (2012, 2014, 2016, and 2018). The data are drawn from the CFPS dataset. Panel A uses the DID specification in Equation 2, while Panel B incorporates the urban sample and applies the DDD specification in Equation 3. Standard errors are clustered at the county level. Point estimates correspond to the coefficient on HAGC exposure, and vertical bars indicate 95% confidence intervals.

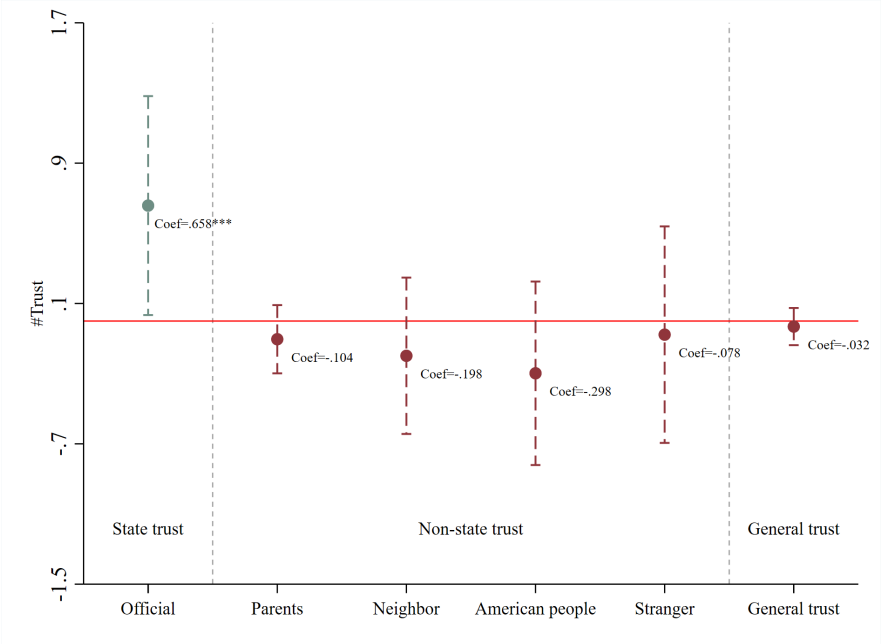


(A) **Punish corruption.** (B) **Government transparency.**

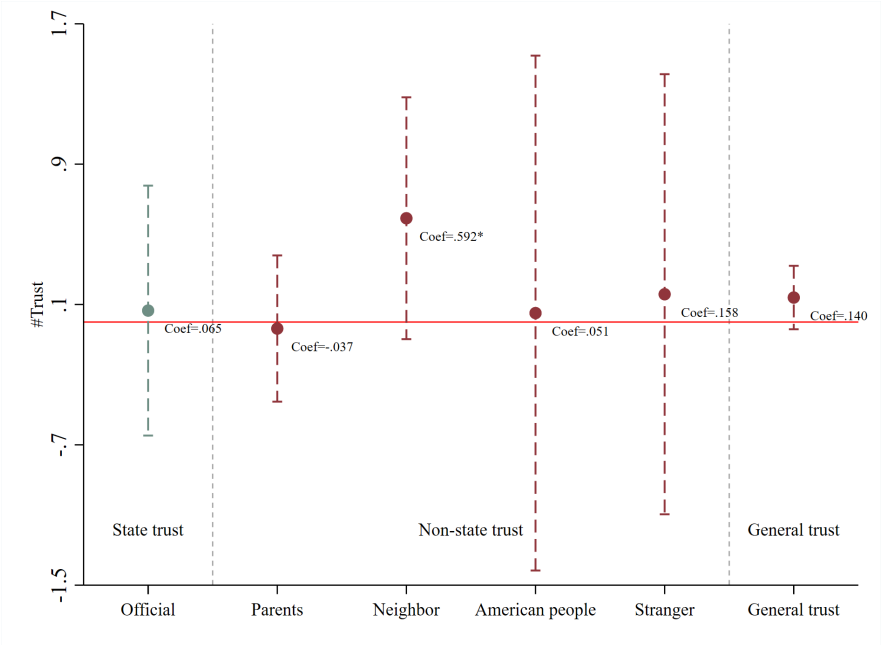
(C) **Human rights protection.** (D) **Enforcement fairness.**

FIGURE B4 More on Baseline: Long-term Effects of HAGC on Satisfaction with Government.

Notes: Figure B4 presents the event study results of cohort-level differential exposure to HAGC on government support evaluated in long-term. The data is derived from CSS, consisting of respondents with rural *hukou* during HAGC policy. All the regressions use event study specification in Equation 4. The dependent variables are the satisfaction with different government work, including anti-corruption, government transparency, human rights protection, and enforcement fairness, ranked scores 1-4. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The gray shaded area represents the 90% confidence interval.



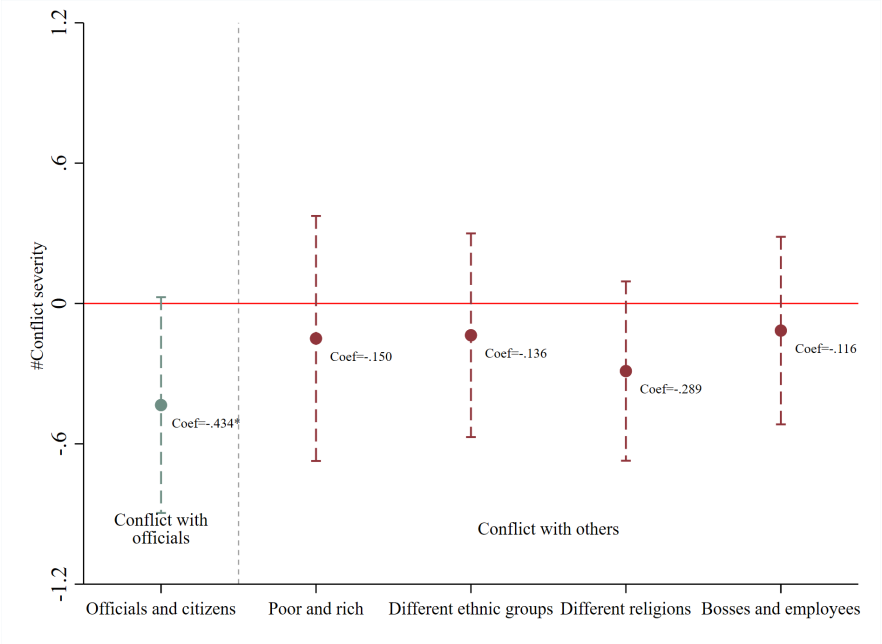
(A) **Rural.**



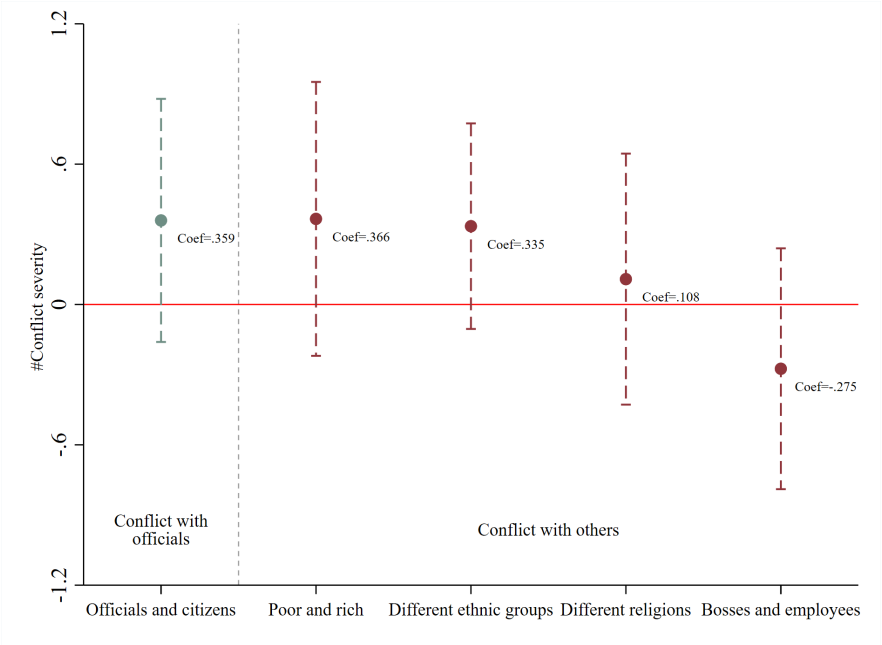
(B) **Urban.**

FIGURE B5 More on Baseline: Long-term Effects of HAGC on Trust.

Notes: Figure B5 presents the effect of cohort-level differential exposure to HAGC on trust evaluated in long-term. The data is drawn from CFPS. All the regressions use DID specification in Equation 2. The dependent variables are trust with officials, parents, neighbors, American people, strangers, and general trust respectively. The specific trusts are ranked scores 0-10 while the general trust is a binary variable. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The dashed line represents the 95% confidence interval.



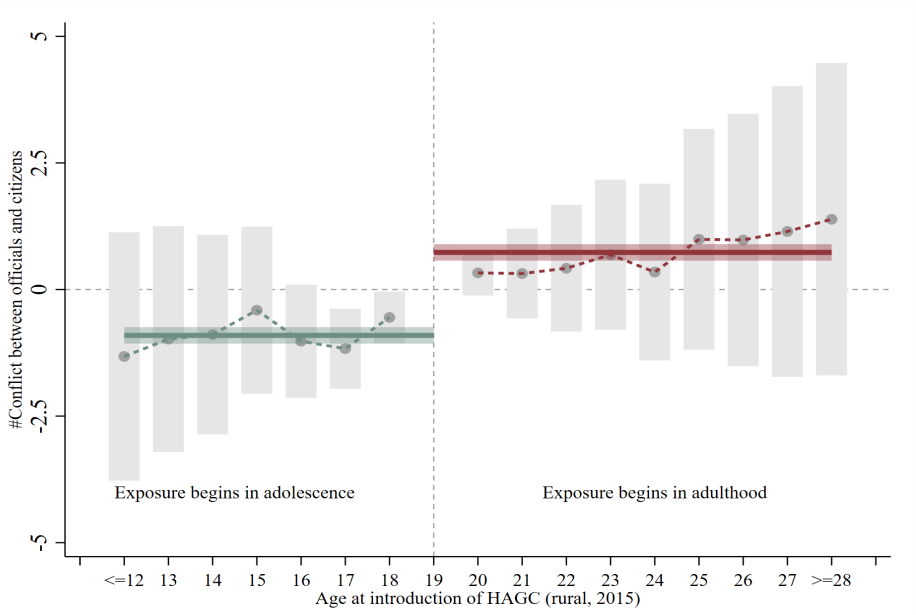
(A) **Rural.**



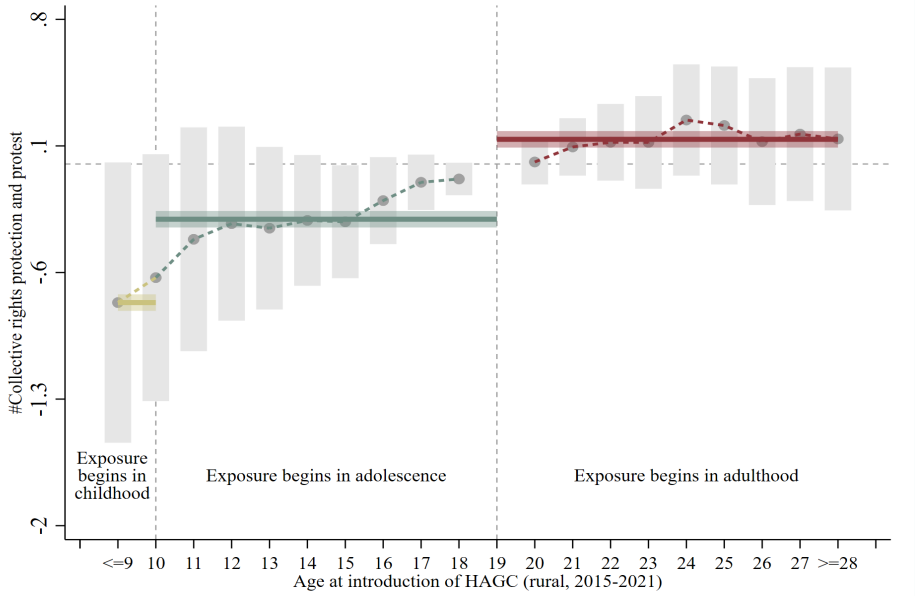
(B) **Urban.**

FIGURE B6 More on Baseline: Long-term Effects of HAGC on Perceived Conflict.

Notes: Figure B6 presents the effect of cohort-level differential exposure to HAGC on respondents' perceived conflict severity in long-term. The data is derived from CSS dataset. All the regressions use DID specification in Equation 2. The dependent variables are the self-reported conflict severity between officials and citizens, poor and rich, different ethnic groups, different religious groups, and bosses and employees respectively. All the conflict severity are ranked scores 1-4. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The dashed line represents the 95% confidence interval.



(A) **Perceived Conflict Severity with Officials.**



(B) **Collective Protest.**

FIGURE B7 More on Baseline: Long-term Effects of HAGC on Conflict and Protest.

Notes: Figure B7 presents the event study results of cohort-level differential exposure to HAGC on the perceived conflict severity with officials and collective protest. The data is derived from the CSS dataset. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use event study specification in Equation 4. The dependent variables in panel A and B are respectively self-reported conflict severity between officials and citizens ranked scores 1-4 and whether respondent participated in collective protest last year. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The gray shaded area represents the 90% confidence interval.

TABLE B1 More on Baseline: Satisfaction with Government Work

Dependent variable:	Satisfaction with government work							
	Abstract criteria for regime legitimacy				Specific criteria for individual welfare			
	Anti-corruption	Government trans-parency	Human rights protection	Enforcement fairness	Economic growth	Increase employ-ment	Health care	Environment governance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure to HAGC	0.299**	0.287**	0.350***	0.360**	-0.053	0.117	0.121	0.101
	(0.124)	(0.134)	(0.118)	(0.143)	(0.118)	(0.114)	(0.111)	(0.105)
Inference robustness (Exposure to HAGC)								
Robust p-value	0.01590**	0.03221**	0.00302***	0.01182**	0.65332	0.30474	0.27567	0.33610
Holm-adjusted p-value	0.03546**	0.03546**	0.01206**	0.03546**	1.00000	1.00000	1.00000	1.00000
Cauchy-combined p-value	0.00784***				0.37905			
Controls	✓	✓	✓	✓	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓	✓	✓	✓
Dependent-variable mean	2.450	2.436	2.494	2.476	2.488	2.474	2.540	2.438
Observations	4,879	4,817	4,937	4,955	5,053	4,983	5,116	5,184

Notes: Table B1 reports the effect of HAGC on long-term satisfaction with government across different issues. The data is derived from CSS. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The dependent variables are a series of satisfaction with government work in different aspects, ranked with scores 1-4. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The Holm-adjusted p-values control the family-wise error rate (Holm, 1979). The Cauchy-combined p-value provides a joint test across outcomes (Liu and Xie, 2020). ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE B2 More on Baseline: Conflict and Protest

Dependent variable:	Anti-government					
	Collective Protest	Conflict with officials				
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure to HAGC	-0.250**	-0.402***	-0.057***	-0.056***	-0.060***	-0.060***
	(0.126)	(0.110)	(0.016)	(0.019)	(0.017)	(0.017)
Governing ability			0.073***	0.055***	0.050***	0.035***
			(0.006)	(0.005)	(0.006)	(0.004)
Controls	✓	✓	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓	✓
Observations	5,228	4,801	4,796	4,798	4,794	4,792
Dataset	CSS	CFPS	CFPS	CFPS	CFPS	CFPS

Notes: Table B2 reports the results of cohort-level differential exposure to HAGC on anti government protests, conflict with officials, and governing ability in long-term. CSS dataset is used in column (1) and CFPS dataset is used in columns (2) to (6). The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The dependent variables whether respondents participated in collective protest or experienced conflict with officials last year, with standardization. Columns (3) to (6) additionally control a series of dummies indicating respondents suffered unfair treatment, shirking responsibility, unreasonable charge, or income discrimination, respectively. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Appendix C More on Robustness Checks

Heterogeneous Treatment Effects We address potential heterogeneous treatment effects across different policy waves. In Panel A of Figure C1, we replace the baseline policy exposure variable with three dummy variables, each representing whether an individual was aged 7-18 during a particular policy wave. The effects on government support, political trust, and conflict with officials are all consistent with our baseline findings. In addition, Table C1 reports estimates using HTE-robust DiD estimators. Specifically, we employ the group-time average treatment effect estimator of Sun and Abraham (2021) and the two-stage imputation estimator of Gardner (2022). These approaches address potential biases arising from HTE in staggered treatment settings. The results remain highly consistent with our baseline estimates in both statistical significance and economic magnitude.

Alternative Settings We then explore the robustness of our results under alternative specifications in Figure C1.

First, our baseline specification does not account for age-specific differences of policy exposure. Following Huang and Liu (2023), we redefine the exposure variable as a continuous measure, capturing the number of years each cohort g in county c overlaps with the policy during the ages of 7-18. We first assign exposure values on a quarterly scale from 0 to 1, reflecting the overlap between an individual's age (7-18) and the four-year TV subsidy policy duration. Alternatively, as TV's effect might persist beyond the policy implementation periods, we measure individual's exposure as the time between the policy's starting year and the year when he/she reaches age 18. As shown in Panel B, these adjustments do not alter our conclusions.

Second, we redefine the age windows for the treatment and control groups. In Panel C, we let the treatment windows to start at the age of 7, 10, 13, and 16 (and end at 18). In Panel D, we shorten the control windows to end at ages 22, 25, 28, and 31. All results remain robust, similar to the baseline.

Placebo Tests We also perform a series of placebo tests in Figure C1. Panel E reports the effects of HAGC when using ages over 18, such as 19-21, as the treated windows. As expected, changes in values for this age range are insignificant. In Panel F, we estimate the pseudo effect when shifting the HAGC policy forward by 1-9 years, which again produce insignificant results. These findings suggest that our results are robust against omitted variables.

In Figure C2, we randomly assign HAGC policy start years and conduct permutation tests, repeated by 1,000 times. Our baseline point estimates are shown with dotted blue lines, permutation outcomes with red dots, and the placebo trial averages with solid blue lines. These results confirm that our baseline findings are not driven by other random factors or omitted variables.

Possible Confounders In this subsection, we address potential confounding factors that could threaten the validity of our baseline estimates, with results presented in Figure C3.

In Panel A, we account for six policies in China that may influence public support and political trust during periods similar to the HAGC. These policies include the expansion of college enrollment (from 1999), China's accession to the World Trade Organization (WTO, from 2001), agricultural tax reform (from 2003), the New Rural Pension Scheme (NRPS, from 2003), the anti-corruption campaign, and the poverty alleviation campaign. First, to control for the impact of college enrollment expansion, we introduce a variable capturing the logarithm of the number of individuals holding a university or vocational college degree at the prefectural level. This variable is interacted with an exposure dummy, which equals 1 if an individual was under 18 when the policy was implemented, reflecting the cohort most likely affected by the policy. Second, we account for China's accession to the WTO by including prefectural-level net exports, which proxy for the economic changes driven by increasing trade integration. Third, for the agricultural tax reform, we use the county-level agricultural tax amount, capturing the tax burden on farmers. Fourth, we control for the NRPS by incorporating data on rural pension coverage at the county level, indicating the extent to which individuals benefited from the scheme. Fifth, we control for the anti-corruption campaign by including a variable that captures whether respondents had experienced the campaign, proxied by the number of officials arrested in their region. Sixth, we control for the poverty alleviation campaign by including a dummy variable indicating whether the campaign was implemented in the respondent's county. Each of these variables is interacted with the exposure dummy to reflect the differential impact on individuals of varying ages during the HAGC policy. Our findings remain highly robust, indicating that these concurrent policies are unlikely to drive our baseline results.

In Panel B, we exclude the influence of China's infrastructure development since 2000. The simultaneous expansion of infrastructural programs might confound with the implementation of HAGC. We first control for adolescence exposure to electricity, cable radio, television, postal services, telephone, mobile phone signal, roads, railways, water, and gas in the respondents' village, using dummy variables for each infrastructure type based on

their availability during adolescence (<18 age). Second, to alleviate the interference from infrastructure construction with the HAGC policy, we add a series of interaction terms between villages' availability to infrastructure above during HAGC policy (2010 proxy) with cohort dummy. We find similar results after these additional controls, which means the effect of HAGC is not driven by infrastructural improvement.

In Panel C, we consider the influence of local conditions by controlling for the interaction terms between the predetermined (2007) variables and the birth cohort. These variables include socioeconomic factors (per capita GDP, per capita wage, industry outputs, population size, technical and educational personnel, entertainment personnel), educational factors (number of colleges, senior high schools, college teachers, college students), infrastructure factors (unemployment figures, environmental investments, cinemas, hospitals, road area, bus availability, library collections, green areas), mass media and information technology conditions (number of mobile/Internet users, and telecommunication incomes), all logged where applicable. This comprehensive control strategy helps ensure that our findings are not driven by path-dependence effect, highlighting the unique role of HAGC policy.

In Panels D and E, we deal with the migration issue by excluding samples that involve changes in *hukou* status or migration. We define "migrants" as respondents who migrated from age 3 to 2010, from age 12 to 2010, from 2010 to the survey year, and the union of the above three. The obtained effects are highly robust. Additionally, in Table C2, we directly examine the relationship between HAGC exposure and *hukou* changes or migration. The results demonstrate that HAGC exposure cannot drive migration or changes in *hukou* status, which reinforces the robustness of our findings.

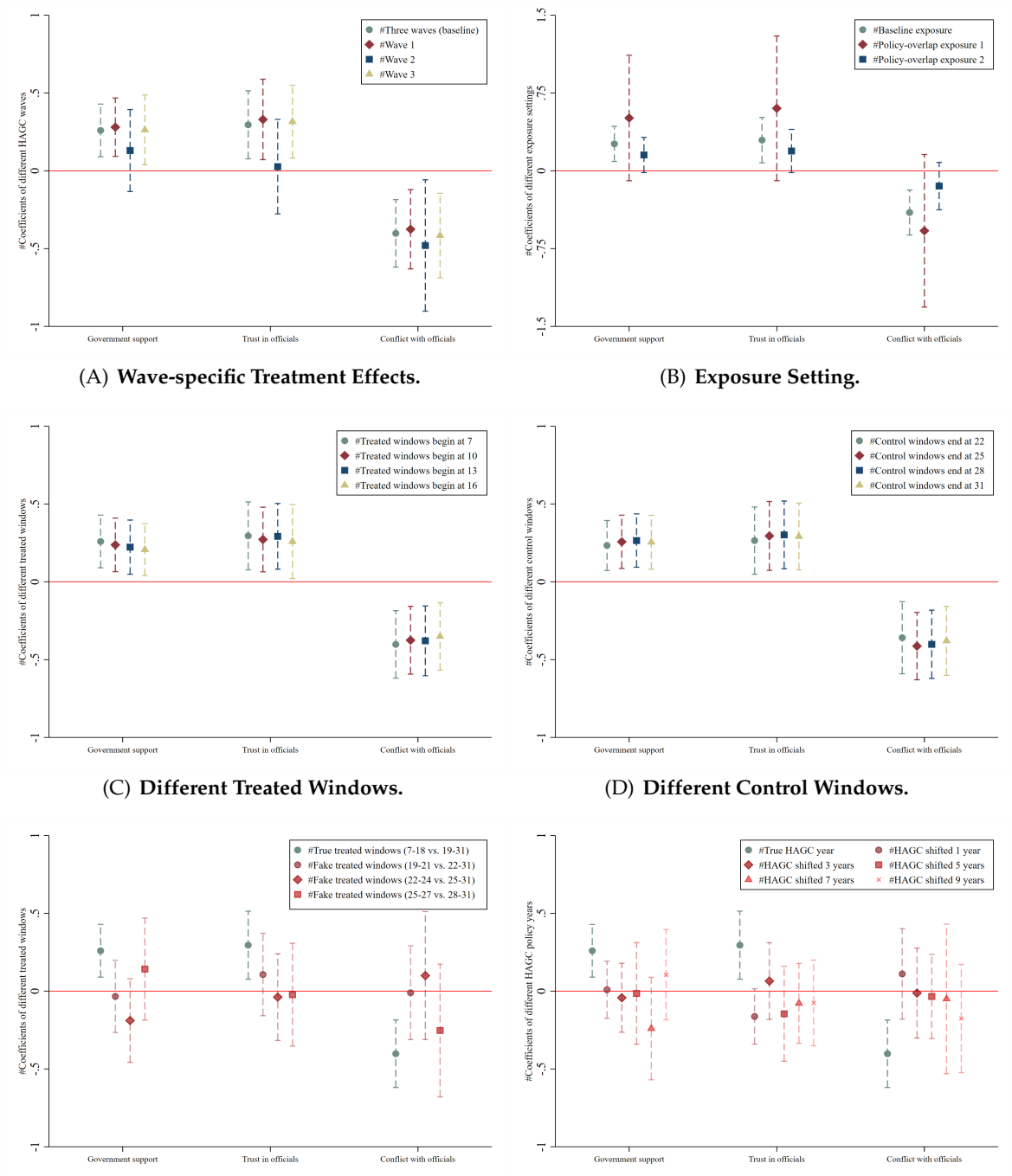
Finally, to account for potential attrition problem in longitudinal surveys, we re-estimate the results using the cross-sectional dataset (CFPS 2016) and restrict our analysis to samples that participated in at least five survey waves. Results in Panel F confirm that neither attrition nor sample selection threatens the integrity of the baseline results.

Other Robustness Checks In Figure C4, we conduct a series of technical analyses.

In Panel A, we use cross-sectional datasets (CFPS 2016 and 2018) to re-estimate our results. To address potential sampling bias, we perform a series of weighted regressions in Panel B. These regressions utilize the inverse of survey participation frequency, cross-sectional sampling weights, and panel sampling weights. The effects are similar to the baseline, showing that our findings are not sensitive to bias in surveys.

Panels C-E assess how sensitive our baseline results are to various specification changes.

Panel C incrementally adds control variables, confirming the stability of our baseline estimates. Panel D introduces trends at multiple levels, from province-cohort linear and quadratic trends to cubic trends, and prefecture-cohort linear trends. In Panel E, we adjust the clustering of standard errors from the county level to include county-year, prefecture-year, province-year, prefecture, and province levels. Across all these changes, our results remain consistently robust.



(A) **Wave-specific Treatment Effects.**

(B) **Exposure Setting.**

(C) **Different Treated Windows.**

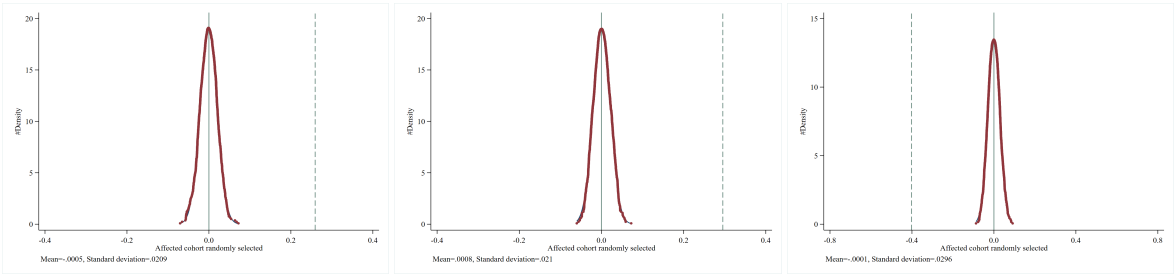
(D) **Different Control Windows.**

(E) **Fake Treated Windows.**

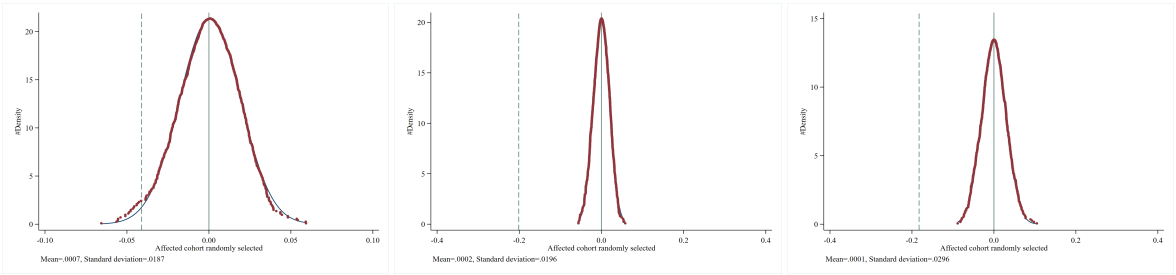
(F) **Fake Policy Years.**

FIGURE C1 More on Robustness: Heterogeneous Treatment Effects and Alternative Settings.

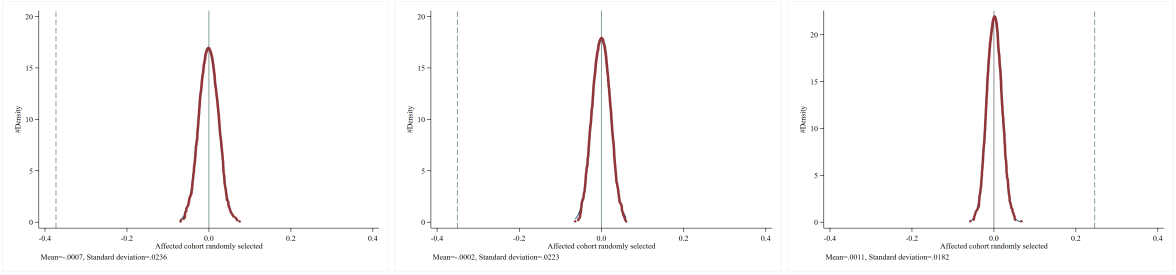
Notes: Figure C1 shows that our baseline findings are robust to heterogeneous treatment effects and different settings. The data is drawn from CFPS. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. The dashed line represents the 95% confidence interval. For more details see Appendix C.



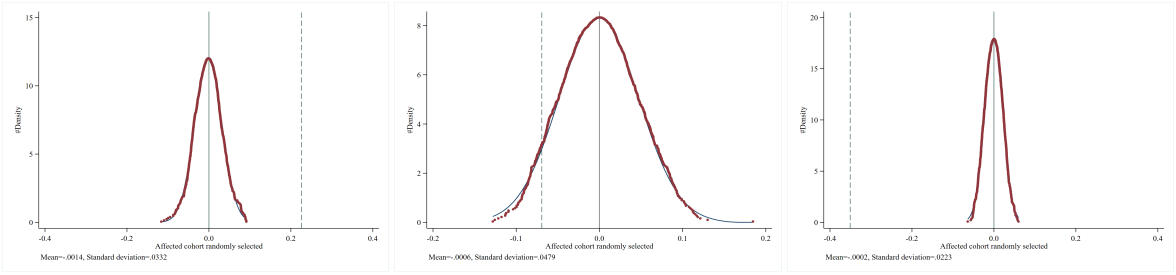
(A) **Government support.** (B) **Trust in officials.** (C) **Conflict with officials.**



(D) **Math test score.** (E) **Word test score.** (F) **Memory test score.**



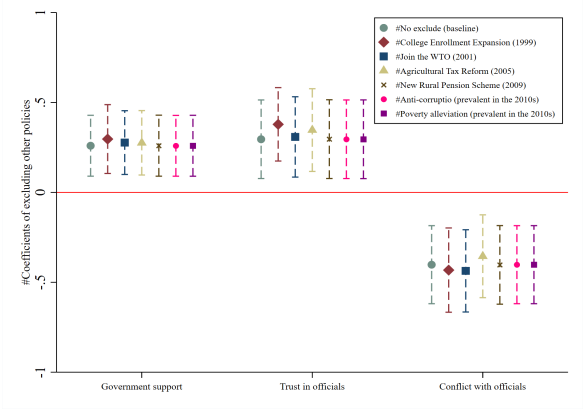
(G) **CES-D score (20-question, 80-scale).** (H) **CES-D score (8-question, 32-scale).** (I) **Emotion index (weekly).**



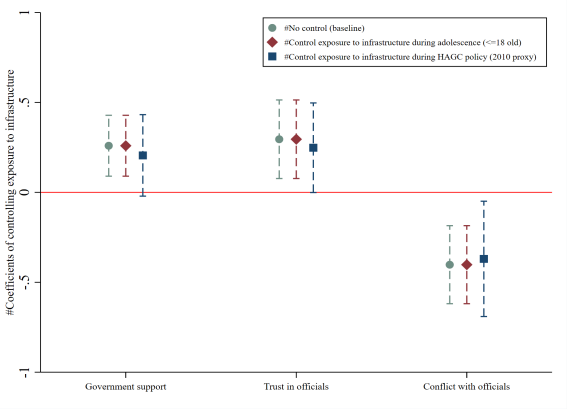
(J) **Emotion index (monthly).** (K) **Education level.** (L) **College and above.**

FIGURE C2 More on Robustness: Permutation-test Coefficients for "pseudo HAGC".

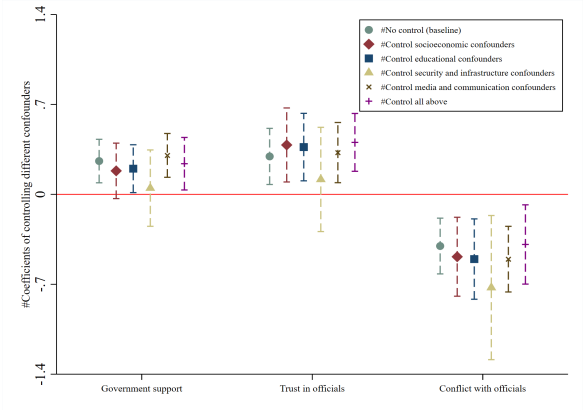
Notes: Figure C2 demonstrates the estimates for the pseudo policy results. The data is drawn from CFPS. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The dashed line represents the true coefficient. For more details see Appendix C.



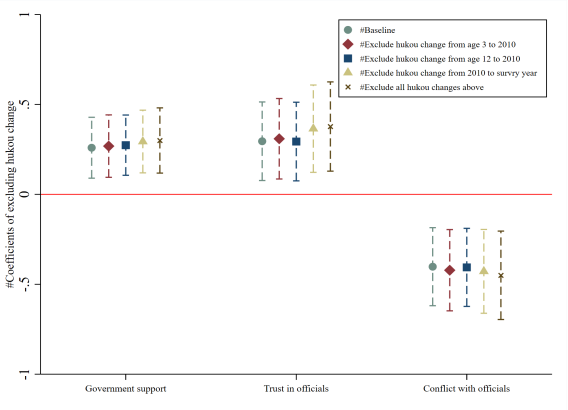
(A) **Interference Policy.**



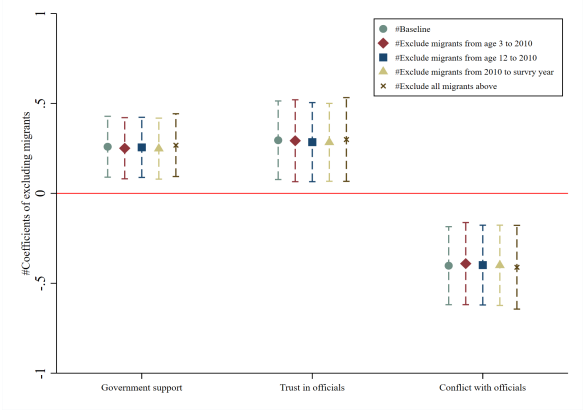
(B) **Exposure to Other Improving Infrastructure.**



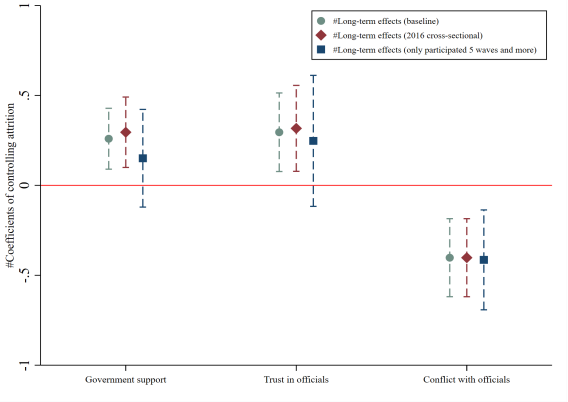
(C) **Predetermined Confounders.**



(D) ***Hukou* Change.**



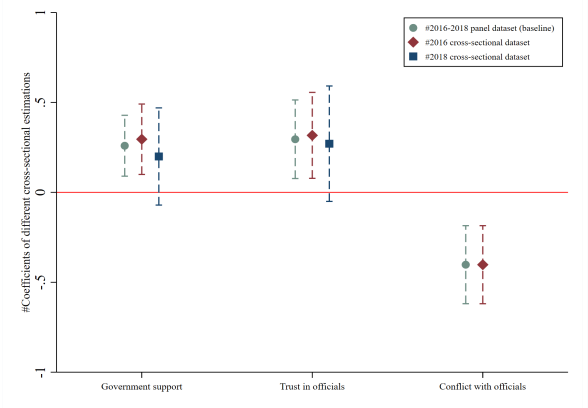
(E) **Migration.**



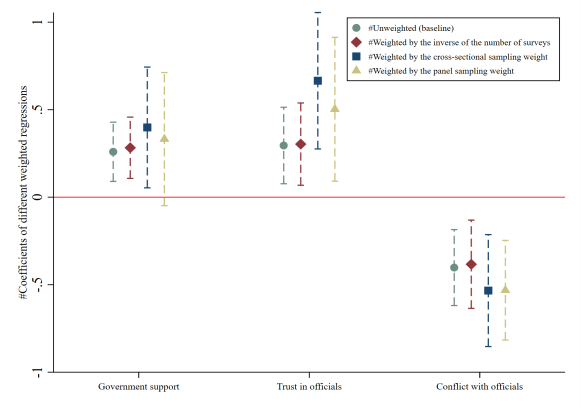
(F) **Attrition.**

FIGURE C3 More on Robustness: Potential Confounders.

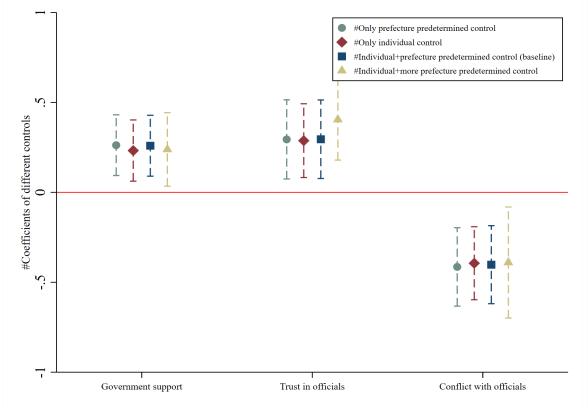
Notes: Figure C3 shows that our baseline findings are robust to a battery of potential confounders. The data is drawn from CFPS. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. The dashed line represents the 95% confidence interval. For more details see Appendix C.



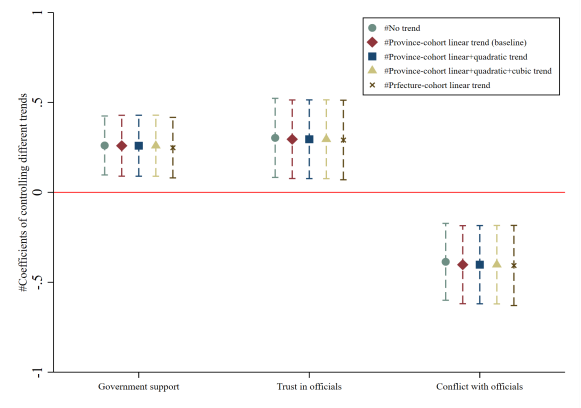
(A) **Different Cross-Sectional Estimations.**



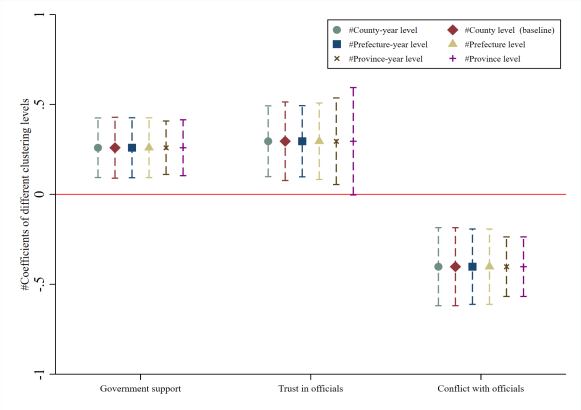
(B) **Different Weighted Regressions.**



(C) **Different Controls.**



(D) **Different Trends.**



(E) **Different Cluster Levels.**

FIGURE C4 More on Robustness: Others.

Notes: Figure C4 shows that our baseline results are robust under cross-sectional settings, weighted regressions, different levels of controls, different trends, and different cluster levels. The data is drawn from CFPS. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. The dashed line represents the 95% confidence interval. For more details see Appendix C.

TABLE C1 More on Robustness Check: Heterogeneous Robust DiD Estimator

Dependent variable:	Government support			
Sample:	Long-term (2016–2018)		Short-term (2012–2014)	
	DiD	DDD	DiD	DDD
	(1)	(2)	(3)	(4)
Panel A. *Sun and Abraham (2021)*				
Exposure to HAGC	0.282***	0.008	0.044	0.109
	(0.072)	(0.089)	(0.130)	(0.129)
Exposure to HAGC × Rural		0.188***		-0.013
		(0.045)		(0.025)
Panel B. *Gardner (2022)*				
Exposure to HAGC	0.325***	0.102	0.066	0.084
	(0.080)	(0.099)	(0.099)	(0.109)
Exposure to HAGC × Rural		0.203***		0.057
		(0.051)		(0.077)
Dependent-variable mean	3.406	3.428	3.375	3.381
Observations	9,034	14,483	8,647	14,274

Notes: Table C1 reports the effect of cohort-level differential exposure to HAGC on government support using heterogeneous-treatment-robust DiD estimators. Panel A employs the group-time average treatment effect estimator of Sun and Abraham (2021), and Panel B employs the two-stage imputation estimator of Gardner (2022). The data is derived from the CFPS dataset. Columns (1) and (3) use DiD specification in Equation 2 while columns (2) and (4) apply DDD specification in Equation 3. Columns (1) and (2) present the long-term effects (2016–2018) while columns (3) and (4) focus on short-term effects (2012–2014). The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

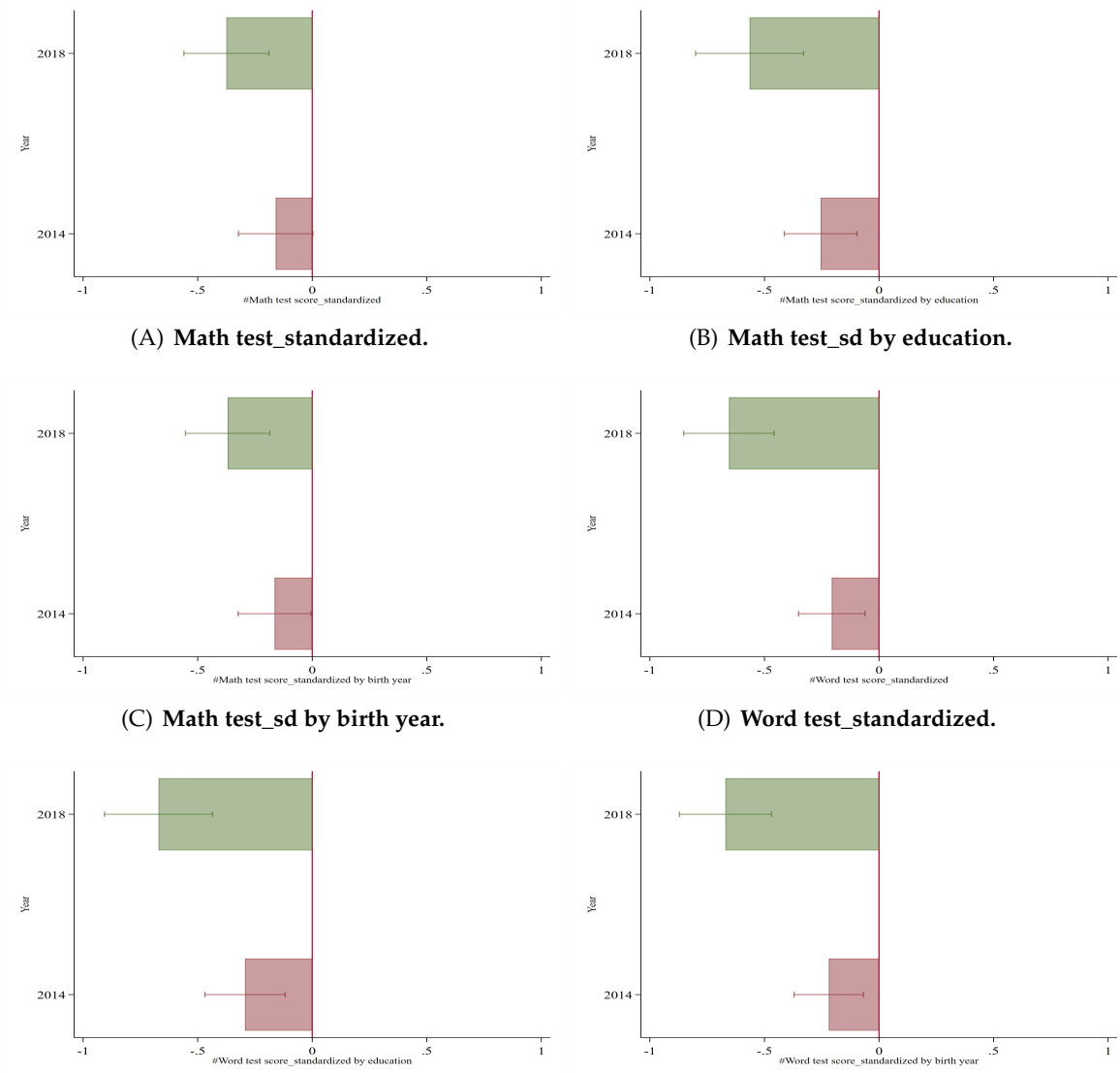
TABLE C2 More on Robustness Check: *Hukou* Change and Migration

Dependent variable:	Change between aged 3 and 2010	Change between aged 12 and 2010	Change between 2010 and now	Change all above
	(1)	(2)	(3)	(4)
Panel A. *Hukou change*				
Exposure to HAGC	-0.018	0.006	-0.009	0.004
	(0.025)	(0.007)	(0.017)	(0.005)
Dependent-variable mean	0.063	0.007	0.076	0.003
Panel B. *Migration (county level)*				
Exposure to HAGC	-0.001	0.017	0.015*	0.005
	(0.026)	(0.012)	(0.009)	(0.006)
Dependent-variable mean	0.373	0.338	0.335	0.326
Controls	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓
FEs	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓
Observations	33,786	33,786	33,786	33,786

Notes: Table C2 reports the results of cohort-level differential exposure to HAGC on *hukou* status change and county-level migration. The data is drawn from CFPS. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The dependent variables are dummy variables indicating whether respondents' *hukou* status or county change between aged 3 and policy year (2010 proxy), between aged 12 and policy year, between policy year and survey year, and all mentioned above. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Appendix D More on Mechanism

D.1 Reduced Ability to Demand Accountability



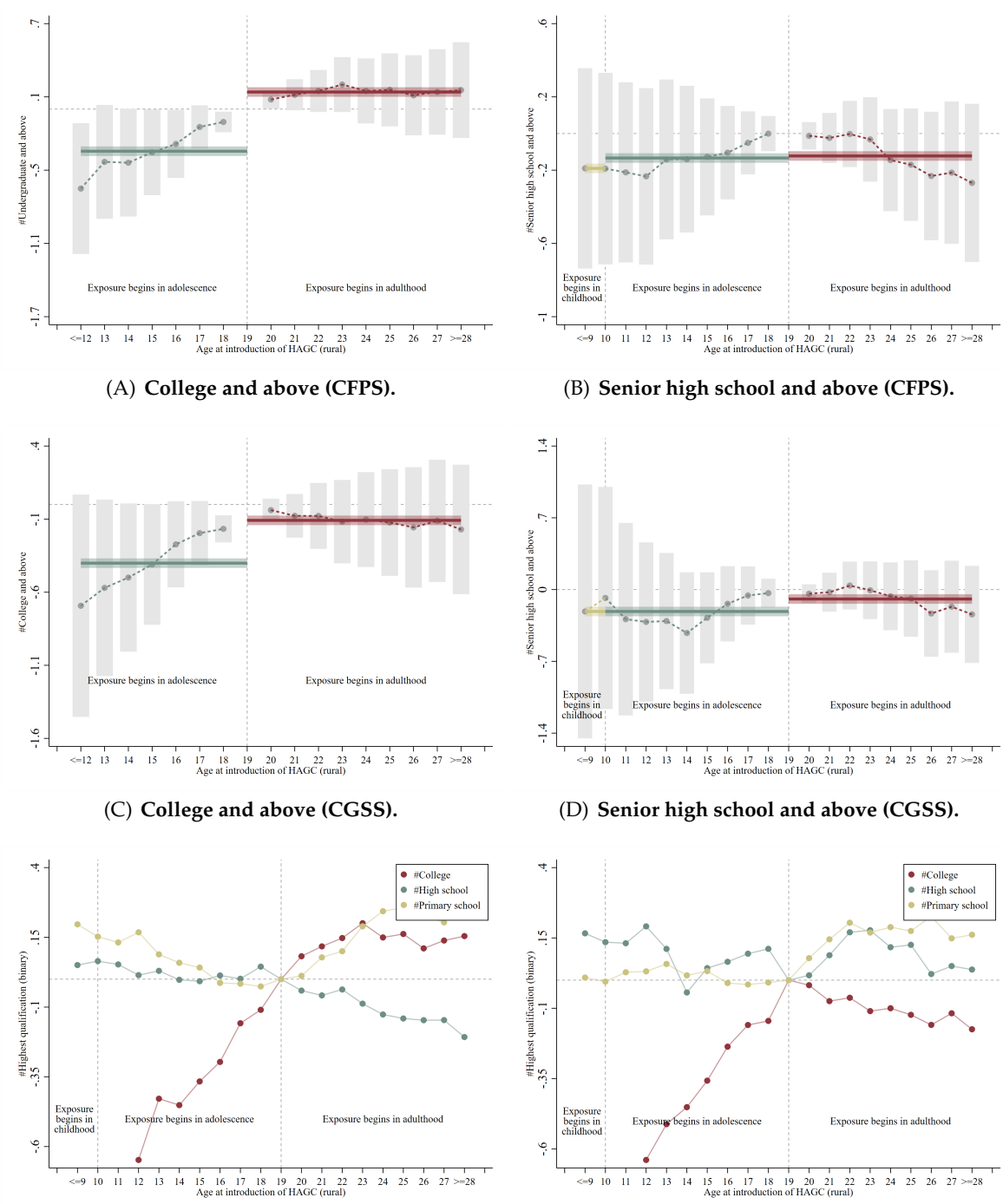
(A) **Math test_standardized.** (B) **Math test_sd by education.**

(C) **Math test_sd by birth year.** (D) **Word test_standardized.**

(E) **Word test_sd by education.** (F) **Word test_sd by birth year.**

FIGURE D1 More on Mechanism: Mid-term and Long-term Effects of HAGC on Cognitive Outcomes.

Notes: Figure D1 demonstrates the mid-term (2014 or 2016), and long-term (2018 or 2020) effects of HAGC exposure on cognitive outcomes. The data is drawn from CFPS. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions include two interaction terms between survey time dummies and HAGC exposure (interaction terms between short-term dummy (2010/2012) and HAGC exposure are omitted). Dependent variables in Panels A-C, D-F, are math and word test scores with different standardization, respectively. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. Binned bar shows a 95% confidence interval.



(A) **College and above (CFPS).**

(B) **Senior high school and above (CFPS).**

(C) **College and above (CGSS).**

(D) **Senior high school and above (CGSS).**

(E) **Three levels in one graph (CFPS).**

(F) **Three levels in one graph (CGSS).**

FIGURE D2 Mechanism: Effects of HAGC on Education by Age at First Exposure.

Notes: Figure D2 demonstrates the event study estimates of cohort-level differential exposure to HAGC on educational achievement. The data is derived from the CFPS and CGSS dataset. The sample consists of respondents with rural *hukou* during HAGC policy. All the panels present rural samples with DID specification in Equation 4. The dependent variable in Panels A and C in whether one gets college education (and above), while that in Panels B and D is whether one gets senior high school education (and above). Panel E and F plot college, senior high school, and primary school education in one graph. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. Bin shows the 90% confidence interval.

TABLE D1 More on Mechanism: Education-based (Acquired) Cognitive Outcomes

Dependent variable:	Educational-based cognitive outcomes							
	Math test				Word test			
Standardized by:	No	All	Education	Cohort	No	All	Education	Cohort
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. *With baseline controls*								
Exposure to HAGC	-0.500	-0.087	-0.102	-0.083	-1.602***	-0.200***	-0.251***	-0.207***
	(0.449)	(0.078)	(0.079)	(0.076)	(0.590)	(0.074)	(0.088)	(0.077)
Panel B. *With baseline controls*								
Exposure to HAGC	-0.918*	-0.160*	-0.255***	-0.164**	-1.652***	-0.206***	-0.293***	-0.219***
# mid-term (2014)	(0.473)	(0.082)	(0.080)	(0.080)	(0.588)	(0.073)	(0.089)	(0.077)
Exposure to HAGC	-2.156***	-0.375***	-0.565***	-0.369***	-5.247***	-0.655***	-0.671***	-0.670***
# long-term (2018)	(0.541)	(0.094)	(0.119)	(0.093)	(0.800)	(0.100)	(0.119)	(0.102)
Panel C. *Control interference policies*								
Exposure to HAGC	-0.506	-0.088	-0.096	-0.084	-1.590***	-0.199***	-0.244***	-0.208***
	(0.464)	(0.081)	(0.080)	(0.078)	(0.589)	(0.074)	(0.089)	(0.077)
Panel D. *Control interference policies*								
Exposure to HAGC	-0.943*	-0.164*	-0.254***	-0.169**	-1.664***	-0.208***	-0.290***	-0.224***
# mid-term (2014)	(0.489)	(0.085)	(0.082)	(0.083)	(0.592)	(0.074)	(0.090)	(0.078)
Exposure to HAGC	-2.182***	-0.379***	-0.561***	-0.374***	-5.267***	-0.658***	-0.667***	-0.676***
# long-term (2018)	(0.554)	(0.096)	(0.120)	(0.095)	(0.802)	(0.100)	(0.120)	(0.102)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓	✓	✓	✓
Dependent-variable mean	13.157	-0.074	-0.007	-0.154	23.460	-0.045	-0.001	-0.118
Observations	13,899	13,899	13,899	13,899	13,899	13,899	13,899	13,899

Notes: Table D1 reports the results of cohort-level differential exposure to HAGC on acquired cognitive outcomes in long-term. The data is drawn from CFPS. The sample consists of respondents with rural *hukou* during HAGC policy. Panel A and C use DID specification in Equation 2 while panel B and D use the interaction terms between exposure and wave dummy to estimate. Panel A and C pool test results from different terms. Panel B and D evaluate dynamic treatment effects of HAGC by middle-term (2014), and long-term (2018) (short-term (2010) as base group). The dependent variables are test scores with different standardization. Math test score is evaluated in columns (1)-(4), and word test score is evaluated in columns (5)-(8). For more about the cognitive tests, see subsubsection A.2.1. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D2 More on Mechanism: Educational Achievements

Dependent variables:	General level	Highest qualification			
	Education year (logged)	Junior high school and above	Senior high school and above	College and above	
				No control	Control policies
	(1)	(2)	(3)	(4)	(5)
Panel A. *CGSS dataset*					
Exposure to HAGC	-0.028	-0.008	-0.025	-0.144***	-0.144***
	(0.074)	(0.035)	(0.066)	(0.038)	(0.038)
Dependent-variable mean	2.371	0.879	0.478	0.224	0.224
Observations	5,302	6,630	6,630	6,622	6,622
Panel B. *CFPS dataset*					
Exposure to HAGC	-0.033	-0.012	0.088	-0.104*	-0.110*
	(0.062)	(0.051)	(0.060)	(0.062)	(0.063)
Dependent-variable mean	2.215	0.819	0.438	0.193	0.194
Observations	2,752	4,628	4,257	3,417	3,402
Controls	✓	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓

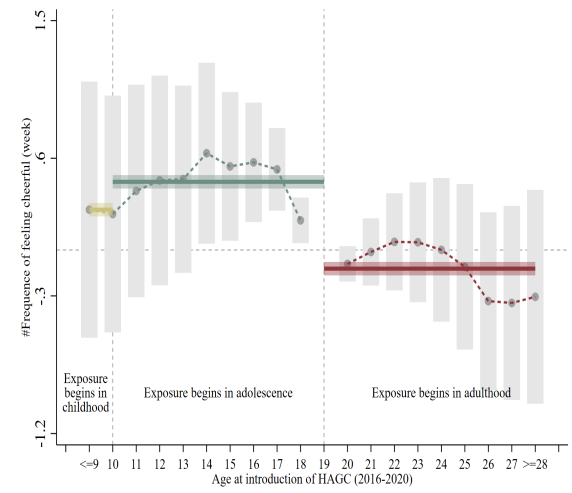
Notes: Table D2 demonstrates the results of cohort-level differential exposure to HAGC on educational achievement. Panels A, B, and C uses data from CGSS and CFPS. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The dependent variables from column (1) to (4) are education year (logged) and different qualification dummy variables. The column (5) additionally controls the density of prefectural primary school density when the respondent entered primary school and the dummy variable of whether the respondent was affected by the Curriculum Reform to eliminate interference. In Panel A, samples are restricted to respondents aged not less than 22/12/15/18 years old respectively to ensure that they had gone through the initial stage of relevant academic qualifications (respondents answer the highest degree they have completed), while in Panel B and C, samples are restricted to respondents aged not less than 25/15/18/22 years old respectively to ensure that they had gone through the completion stage of relevant academic qualifications (respondents answer the highest degree they have studies). All regressions include individual-level controls, predetermined prefectural-level characteristics interacted with cohort FE, same FEs as Table 1, and province-cohort-specific linear trend. Standard errors are clustered at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D3 More on Mechanism: Effects of HAGC on Political Knowledge

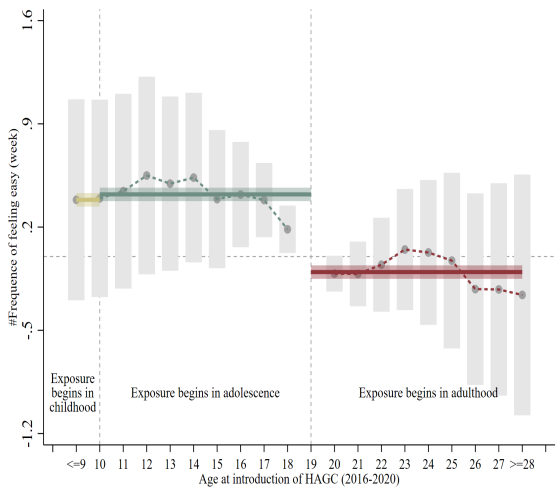
Dependent variables:	Political knowledge					
	All Questions	Question 1	Question 2	Question 3	Question 4	Question 5
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure to HAGC	-1.687***	-1.448***	-0.781*	-1.546**	-0.895**	-1.036**
	(0.475)	(0.546)	(0.403)	(0.596)	(0.431)	(0.487)
Inference robustness (Exposure to HAGC)						
Robust p-value		0.00800***	0.05263*	0.00949***	0.03784**	0.03339**
Holm-adjusted p-value		0.04001**	0.10018	0.04001**	0.10018	0.10018
Cauchy-combined p-value				0.01636**		
Controls	✓	✓	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓	✓
Observations	2,277	2,277	2,277	2,277	2,277	2,277

Notes: Table D3 reports the long-term effects of HAGC on individual political knowledge. The Data is drawn from ISCIU. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. Questions 1-5 are different questions to assess one's political knowledge level. The dependent variables are a series of dummies with standardization indicating whether respondents answer the question correctly. Question 1 is "How many members are there currently on the Standing Committee of the Political Bureau of the CPC Central Committee?"; Question 2 is "In which year did China officially announce the implementation of the socialist market economic system?"; Question 3 is "In what year was the 18th National Congress of the Communist Party of China held?"; Question 4 is "Who is the current British Prime Minister?"; Question 5 is "China's agricultural tax was abolished during which prime minister's term?"; Index is the average scores of 5 questions. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The Holm-adjusted p-values control the family-wise error rate (Holm, 1979). The Cauchy-combined p-value provides a joint test across outcomes (Liu and Xie, 2020). ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

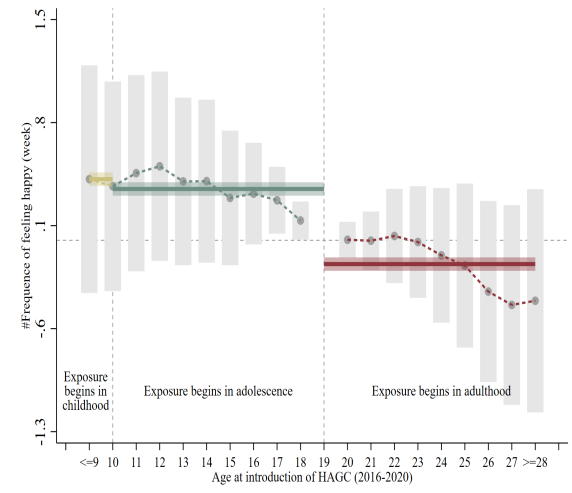
D.2 Reduced Willingness to Demand Accountability



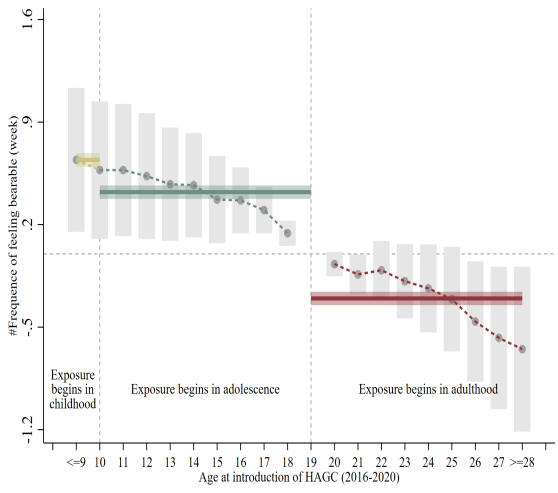
(A) **Cheerful.**



(B) **Easy.**



(C) **Happy.**



(D) **Resilient.**

FIGURE D3 More on Mechanism: Long-term Effects of HAGC on Specific Emotions by Age at First Exposure.

Notes: Figure D3 reports the event-study coefficients of HAGC on long-term specific emotional indicators. The data is drawn from CFPS. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use event study specification in Equation 4. The dependent variable is the frequency of feeling this emotion weekly, respectively ranked as 1-4 and standardized by birth year. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. Bin shows the 90% confidence interval.

TABLE D4 More on Mechanism: Effects of HAGC on General Emotion Indicators

Dependent variable:	CES-D score and positive emotion index					
Standardized by:	Raw Score	Sd Score (all)	Sd Score (cohort)	Raw Score	Sd Score (all)	Sd Score (cohort)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. *CES-D score*						
	8-question, 32-scale			20-question, 80-scale		
Exposure to HAGC	-2.454***	-0.356***	-0.356***	-1.241***	-0.351***	-0.367***
	(0.865)	(0.126)	(0.125)	(0.438)	(0.124)	(0.128)
Dependent-variable mean	32.742	0.081	0.089	13.339	0.020	0.022
Observations	7,671	7,671	7,671	7,666	7,666	7,666
Panel B. *Positive emotion index*						
	Weekly			Monthly		
Exposure to HAGC	0.543***	0.246***	0.251***	0.643**	0.226**	0.246**
	(0.203)	(0.092)	(0.093)	(0.259)	(0.091)	(0.096)
Dependent-variable mean	17.602	-0.077	-0.079	22.306	-0.029	-0.013
Observations	12,485	12,485	12,485	4,125	4,125	4,125
Controls	✓	✓	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓	✓

Notes: Table D4 illustrates the long-term effect of HAGC exposure on general emotions. The data is drawn from CFPS. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The dependent variable is CES-D score in Panel A and positive emotion index in Panel B. Columns (1) and (4) use raw scores; columns (2) and (5) use standardized scores; columns (3) and (6) use within-cohort standardized scores. For more about the measurement and construction of CES-D scores, see subsubsection A.2.2. The positive emotion index is derived by summing the frequency (weekly or monthly) of a series of positive emotions. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D5 More on Mechanism: Effects of HAGC on Specific Emotions

Dependent variables:	Frequency of feeling specific emotions				
	(1)	(2)	(3)	(4)	(5)
Panel A. *Weekly*					
	Cheerful	Relaxed	Happy	Resilient	Supported
Exposure to HAGC	0.147**	0.142**	0.092*	0.076**	0.083
	(0.073)	(0.065)	(0.055)	(0.034)	(0.067)
Dependent-variable mean	3.307	3.417	3.479	3.850	3.549
Observations	12,492	12,493	12,492	12,490	12,493
Inference robustness (Exposure to HAGC)					
Robust p-value	0.04404**	0.02892**	0.09438*	0.02540**	0.21542
Holm-adjusted p-value	0.13212	0.12699	0.18876	0.12699	0.21542
Cauchy-combined p-value			0.04494**		
Panel B. *Monthly*					
	Cheerful	Calm	Relaxed	Engaged	Hopeful
Exposure to HAGC	0.172**	0.225**	0.089	0.071	0.075
	(0.084)	(0.099)	(0.080)	(0.088)	(0.063)
Dependent-variable mean	4.206	4.380	4.493	4.513	4.712
Observations	4,128	4,129	4,128	4,129	4,127
Inference robustness (Exposure to HAGC)					
Robust p-value	0.04060**	0.02304**	0.26592	0.41977	0.23386
Holm-adjusted p-value	0.16239	0.11521	0.70158	0.70158	0.70158
Cauchy-combined p-value			0.06576*		
Controls	✓	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓

Notes: Table D5 reports the long-term effects of HAGC exposure on specific emotions. The data is drawn from CFPS. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The dependent variables are the frequency of feeling different specific emotions (weekly and monthly, ranked with scores 1-4 and 1-5, respectively). The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The Holm-adjusted p-values control the family-wise error rate (Holm, 1979). The Cauchy-combined p-value provides a joint test across outcomes (Liu and Xie, 2020). ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D6 More on Mechanism: Effects of HAGC on Social Network

Dependent variables:	Social network							
	Contact activity				Social quality: agreeability and openness			
	Family		Relative	Friend				
	Have dinner together	Do housework	Contact with relatives	Contact with friends	No distance from others	Tolerate others	Cheerful and sociable	Consider others
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure to HAGC	-0.073	-0.017	-0.210**	-0.017	-0.108	-0.104	-0.095	-0.157
	(0.069)	(0.065)	(0.101)	(0.180)	(0.147)	(0.188)	(0.177)	(0.184)
Inference robustness (Exposure to HAGC)								
Robust p-value	0.29007	0.79368	0.03760**	0.92476	0.46253	0.58013	0.59146	0.39351
Holm-adjusted p-value	0.87021	1.00000	0.15039	1.00000	1.00000	1.00000	1.00000	1.00000
Cauchy-combined p-value	0.26110				0.50691			
Controls	✓	✓	✓	✓	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓	✓	✓	✓
Observations	16,899	16,384	6,476	2,285	4,203	4,200	4,200	4,199

Notes: Table D6 reports the effects of HAGC on contact. The Data is drawn from CFPS and CGSS (CGSS for column (4) and CFPS for others). The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The dependent variables at column (1), (2) and (4) are the frequencies of having dinner with family members , doing housework, and meeting with friends. The dependent variable in the column (3) is an index composed of the average frequency of four contact behaviors including entertaining, giving gifts, providing help, and chatting with relatives. The dependent variables from column (5) to (8) are a series of self-reported personality concerning social contact: feel no distance from others, love to tolerate others, feel cheerful and sociable, and love to consider others. All dependent variables are standardized. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. The Holm-adjusted p-values control the family-wise error rate (Holm, 1979). The Cauchy-combined p-value provides a joint test across outcomes (Liu and Xie, 2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

D.3 Political Apathy, and Authoritarian Stability

TABLE D7 More on Mechanism: Authoritarian Personality

Dependent variables:	State authority	Other authority		
	Government's authority	Parents' authority	Teachers' authority	Bosses' authority
	(1)	(2)	(3)	(4)
Exposure to HAGC	0.806***	-0.046	-0.108	-0.843
	(0.223)	(0.614)	(0.585)	(0.560)
Controls	✓	✓	✓	✓
2007 predetermined cohort	✓	✓	✓	✓
FEs	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓
Observations	1,696	1,764	1,764	1,764

Notes: Table D7 reports the effect of cohort-level exposure to the HAGC policy on authoritarian personality, measured by standardized respect for the authority of government, parents, teachers, and bosses. Data are drawn from the ISCIU dataset. All regressions use the DID specification in Equation 2, with standard controls, fixed effects, and province-specific trends as in Table 1. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE D8 Mechanism: Involvement in Politics

Dependent variable:	Involvement in Politics				
	(1)	(2)	(3)	(4)	(5)
Panel A. *Political involvement*	Political involvement index	Discuss political issues	Report social issues to the media	Voice over government policy	Attend government hearings
Exposure to HAGC	-0.108**	-0.088	-0.026	-0.020	-0.018
	(0.050)	(0.064)	(0.017)	(0.025)	(0.022)
Dependent-variable mean	0.493	0.224	0.348	0.024	0.018
Observations	5,150	3,463	5,150	1,760	1,760
Panel B. *Vote and election*	Vote & election index	Focus on election	Vote for grassroots election	Vote for officials election	Willingness of keep voting
Exposure to HAGC	-0.117*	-0.105*	-0.098	0.025	-0.272***
	(0.061)	(0.061)	(0.070)	(0.076)	(0.102)
Dependent-variable mean	0.861	0.919	0.309	0.071	0.565
Observations	5,220	3,401	4,884	2,938	3,316
Controls	✓	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓

Notes: Table D8 reports the results of cohort-level differential exposure to HAGC on individual participation in politics. Data is derived from the CSS dataset. The sample consists of respondents with rural *hukou* during HAGC policy. All the regressions use DID specification in Equation 2. The dependent variables in panel A and B are a series of dummies measuring respondents' political participation. The "political involvement index" and "vote and election index" are binary variables indicating the respondent participation in at least one of the four related activities last year. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D9 More on Mechanism: Engagement in Politics

Dependent variable:	Political engagement				
	(1) Political engagement index	(2) Focus on political issues	(3) Go online for political information	(4) Read political books	(5) Watch political programs
Exposure to HAGC	-1.080***	-0.204***	-1.647**	-1.363***	-1.292**
	(0.189)	(0.063)	(0.640)	(0.515)	(0.635)
Inference robustness (Exposure to HAGC)					
Robust p-value	0.00000***	0.00120***	0.01007**	0.00813***	0.04189**
Holm-adjusted p-value	0.00000***	0.00481***	0.02439**	0.02439**	0.04189**
Cauchy-combined p-value			0.00000***		
Controls	✓	✓	✓	✓	✓
2007 predetermined cohort	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓
Dependent-variable mean	3.261	3.138	3.752	3.546	2.531
Observations	2,277	4,062	2,277	2,277	2,277

Notes: Table D9 reports the effect of cohort-level exposure to the HAGC policy on one's attention in politics. Data are drawn from the ISCIU dataset. The dependent variables in panel A are a series of ordinal variables ranked with scores 1-5 that indicate engagement in politics. The "political engagement index" is the average of four related political concerns. All regressions use the DID specification in Equation 2, with controls, fixed effects, and provincespecific linear trends as in Table 1. Standard errors are clustered at the county level. The Holm-adjusted p-values control the family-wise error rate (Holm, 1979). The Cauchy-combined p-value provides a joint test across outcomes (Liu and Xie, 2020). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

D.4 More on Alternative Explanations

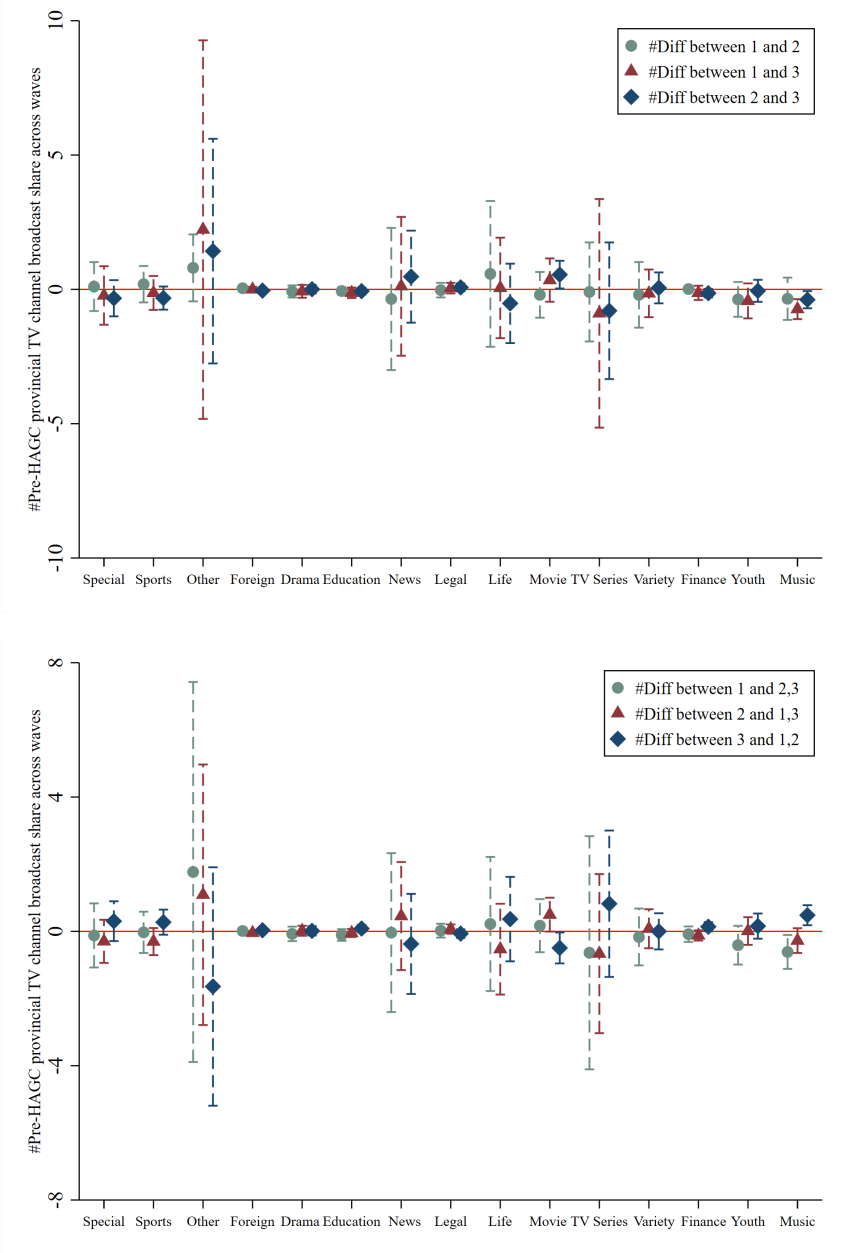


FIGURE D4 Pre-HAGC Provincial TV Broadcast Shares Across Waves.

Notes: Figure D4 shows the pre-HAGC (2006 proxy) provincial TV broadcast differences between different HAGC-waves. The data is derived from China TV Rating Yearbook (2006). The circles, triangles, diamonds in each panel respectively show the differences of wave 1 and 2 (or wave 1 and waves 2 & 3), wave 1 and 3 (or wave 2 and waves 1& 3), wave 2 and 3 (or wave 3 and waves 1 & 2). The dependent variables are the broadcast shares of different types of TV programs. The dashed line represents the 95% confidence interval.

TABLE D10 More on Alternative Explanations: TV Program Broadcast Supply

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Knowledge	Sports	Foreign	Drama	Education	News	Legal
Post HAGC	-0.179	-0.345	0.061	0.008	0.017	0.374	0.211
	(0.373)	(0.341)	(0.071)	(0.040)	(0.069)	(0.825)	(0.123)
Dependent-variable mean	12.029	2.690	0.105	1.019	0.394	13.475	1.086
Observations	367	367	367	367	367	367	367
	Life	Movie	TV Series	Variety	Finance	Youth	Music
Post HAGC	1.154*	-0.110	0.288	0.188	0.357**	-0.050	-0.446
	(0.436)	(0.710)	(1.746)	(0.790)	(0.069)	(0.287)	(0.590)
Dependent-variable mean	8.612	4.000	23.195	8.519	1.871	6.284	2.211
Observations	367	367	367	367	367	367	367
2007 Predetermined # Year	✓	✓	✓	✓	✓	✓	✓
Region-year FE	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓

Notes: Table D10 reports the impact of the HAGC reform on the broadcast shares of different types of television programs. The data is drawn from *China TV Rating Yearbook* (2006-2018). Dependent variables are the shares of television programs broadcast by topic type, measured at the province-year level. All regressions include predetermined provincial controls, region-year FE and province FE. Standard errors are clustered at the region (including east, west, central and northeast) level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D11 More on Alternative Explanations: Material Gratitude

Dependent variables:	Government support			
Sample:	Weekly TV viewing time		Perceived TV Importance	
	Positive	Zero	Higher	Lowest
	(1)	(2)	(3)	(4)
Exposure to HAGC	0.229***	0.087	0.268***	-0.166
	(0.076)	(0.565)	(0.090)	(0.320)
Controls	✓	✓	✓	✓
2007 predetermined # cohort	✓	✓	✓	✓
FEs	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓
Observations	8,401	576	7,943	1062

Notes: Table D11 reports heterogeneous effects of cohort-level exposure to HAGC on long-term government support by post-treatment television usage. The data are drawn from CFPS. The sample consists of respondents with rural *hukou* during the HAGC policy period. Columns (1)-(2) split the sample by weekly TV viewing time, comparing individuals who report positive viewing time with those reporting zero usage. Columns (3)-(4) further partition the sample by respondents' reported frequency and importance of television use in daily life. All regressions follow the DID specification in Equation 2. The control variables, fixed effects, and province-specific linear trends are the same as in Table 1. Standard errors are clustered at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D12 More on Alternative Explanations: Labor Market Performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. *Objective outcomes*	Labor force participation	Earnings	Employment status	Occupation prestige			Job Tier	
		Wage	Entrepreneurship	ISEI	SIOPS	Leader	Management	Hierarchical rank
Exposure to HAGC	-0.008	-0.025	-0.019	-2.368*	-2.305*	-0.032	-0.034	-0.048
	(0.031)	(0.110)	(0.033)	(1.305)	(1.238)	(0.042)	(0.037)	(0.045)
Dependent-variable mean	0.841	7.927	0.128	37.525	39.056	0.132	0.101	0.108
Observations	10,444	4,574	9,582	8,998	8,998	5,770	4,840	4,811
Panel B. *Subjective satisfaction*	Job satisfaction about						Self-reported status	
	Overall job	Income	Work environment	Promotion	Job safety	Work time	Income status	Social status
Exposure to HAGC	0.237***	0.354***	0.242***	0.257*	0.146	0.164	0.276***	0.240**
	(0.081)	(0.103)	(0.092)	(0.144)	(0.109)	(0.112)	(0.088)	(0.112)
Dependent-variable mean	3.392	3.217	3.461	3.159	3.654	3.396	2.699	2.779
Observations	9,149	9,146	9,146	5,902	9,145	9,146	9,667	9,950
Controls	✓	✓	✓	✓	✓	✓	✓	✓
2007 Predetermined # cohort	✓	✓	✓	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Table D12 reports the long-term effects of HAGC exposure on individual labor-market performance. The data is drawn from CFPS. The sample consists of respondents with rural *hukou* during HAGC policy and remain in the labor market now (except for column (1)). All the regressions use DID specification in Equation 2. The dependent variables in panel A include a labor force participation dummy, wage (logged), an entrepreneurship dummy, International Socio-Economic Index (ISEI) score, Standard International Occupational Prestige Scale (SIOPS) score, a dummy for having subordinates (*Leader*), a dummy for being in a management position(*Management*), and the management level of the current job(*Management Hierarchy*) (with higher values indicating higher positions). The dependent variables in panel B consist of self-reported job satisfaction measures, including general satisfaction, satisfaction for income, work environment, promotion, job safety, and work time, with higher values indicating greater satisfaction. Columns (7) and (8) in panel B focus on self-reported income status and social status. All controls and fixed effects are consistent with those in Table 1. Standard errors are clustered at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D13 More on Alternative Explanation: Physical Health

Dependent variable:	Physical health			
	(1) Illness episode	(2) Hospitalization	(3) Chronic	(4) BMI abnormal
Exposure to HAGC	0.180**	0.247***	0.409***	0.162
	(0.079)	(0.077)	(0.079)	(0.121)
Observations	14,295	14,295	12,676	10,717
Controls	✓	✓	✓	✓
2007 predetermined cohort	✓	✓	✓	✓
FEs	✓	✓	✓	✓
Province-cohort trend	✓	✓	✓	✓

Notes: Table D13 reports the effect of cohort-level exposure to the HAGC policy on one's future physical health. Data are drawn from the CFPS dataset. The dependent variables are the number of illness episodes and hospitalization last year, dummy for chronic illness, and dummy for abnormal BMI, all of which are normalized. All regressions use the DID specification in Equation 2, with controls, fixed effects, and provincespecific linear trends as in Table 1. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.