Hearing the Nation: Low-Barrier Information and Rural Education in Socialist China

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Abstract

How can states extend the reach of public institutions where literacy is low and information is costly to acquire? This paper studies rural wired broadcasting in socialist China, a state-funded loudspeaker network built after 1952 that carried Communist Party propaganda alongside literacy instruction, agricultural guidance, and practical information about state policies. Combining newly digitised establishment dates for 1,175 county stations with individual census records on approximately 540,000 household-head–child pairs, I find that children exposed before age twelve completed about 0.4 additional years of schooling. The response is concentrated at the transition to junior high school and is similar in within-family comparisons. Broadcasting did not increase parents' use of patriotic or revolutionary vocabulary in children's names. It also did not affect fertility at the parity thresholds examined under the one-child policy. Schooling effects are larger where alternative information was limited and are non-monotonic in linguistic distance, peaking in fringe-Mandarin areas. The evidence is consistent with broadcasting increasing schooling by making state institutions more legible to rural households, but not by changing their political beliefs.

Keywords: Wired broadcasting, Information diffusion, State-building, Legibility, Human capital

JEL Codes: I25, O33, D83, P26

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1 Introduction

Authoritarian regimes invest heavily in mass media to consolidate power and secure compliance (Edmond, 2013; Guriev and Treisman, 2019). A large literature shows that broadcast media can shift electoral support (DellaVigna and Kaplan, 2007), strengthen support for authoritarian regimes and shape political beliefs (Adena et al., 2015; Ou and Xiong, 2021), mobilise resistance against authoritarian rule (Gagliarducci et al., 2020), and incite participation in mass violence (Yanagizawa-Drott, 2014). Yet this literature largely treats it as an instrument of political persuasion. Broadcasting can also shape how citizens respond to public policies and institutions. Qian (2024), for example, shows that Communist broadcasting during the Cultural Revolution reduced gender inequality in education in China. Authoritarian states also use broadcasting as an instrument of state-building: to promote literacy, explain public institutions, and connect households to schools and the administrative systems that support them.

This function matters for nation-building. States do not extend their reach merely by establishing schools, clinics, or administrative programmes. Households must know what these institutions offer, how to access them, and whether their benefits justify the associated costs. These constraints are especially important where literacy is low and local administrative contact is limited. Broadcasting can transmit political messages, but it can also provide a direct and low-cost source of information about public institutions. I refer to this latter function as institutional legibility: households' ability to understand and navigate state institutions. The term extends the idea of legibility in Scott (1998). Whereas Scott emphasises how states render society legible for administration, the question here is whether communication infrastructure can render state institutions legible to households.

Schools, for example, provide skills and human capital, but states have also used them to standardise language, transmit political values, and cultivate national identity (Weber, 1976; Cantoni et al., 2017; Bandiera et al., 2019; Blanc and Kubo, 2024). Participation in

a state school may reflect political incorporation, but it may also reflect a household using information to take up an institution whose value does not depend on accepting the regime's political message. The two accounts generate different implications. If broadcasting worked mainly through persuasion, its effects should extend beyond schooling to other margins of political alignment. If it reduced information and comprehension constraints, the schooling response should be concentrated where households had limited access to alternative information and where broadcasts were understandable but the language of national institutions was not already familiar.

This paper assesses these implications in rural socialist China. Beginning in 1952, county broadcasting stations transmitted centrally and provincially produced programmes over telephone lines to loudspeakers installed in village squares, fields, and production brigades (*Wired Broadcasting in China*, 1988). Because listening required neither a private receiver nor literacy, the system reached rural populations that other forms of mass communication could not. Its programming deliberately combined political content with literacy instruction, agricultural guidance, scientific and technical knowledge, and practical information about state policies, including appeals for families to send their children to school. The loudspeaker network thus pursued two objectives simultaneously: cultivating support for the Chinese Communist Party and reducing the informational and linguistic barriers that separated rural households from the institutions of the new state. The setting therefore permits the persuasive and informational functions of authoritarian media to be considered together.

I digitise the establishment year of the first wired broadcasting station in 1,175 counties from approximately 2,000 archival county entries in the *Compendium of County Conditions in China*, and harmonise these historical units with census geography. I link the rollout to approximately 540,000 household-head–child pairs from the 1990 Population Census, 239,265 given names from the 2005 Population Mini-Census, and fertility outcomes from the 2000 Population Census. The analysis centres on children's educational attainment. Naming and the one-child-policy analysis assess whether the schooling response was part of a broader

change in political expression or policy compliance.

Children's educational attainment measures the reach of the state school system on a costly margin. For rural households during the period I study, keeping a child in school imposed substantial costs, including tuition and boarding expenses and the loss of children's labour during agricultural seasons (Lardy, 1983; Pepper, 1996; Thøgersen, 2002). Continuing beyond village primary school often required families to send children to township-level secondary schools and to engage with an institution with which many rural parents had little prior experience (Fei, 1992; Suryanarayan, 2021). Before the introduction of nationwide compulsory education in 1986, continued attendance depended substantially on households' willingness and ability to bear these costs. Educational attainment therefore captures a consequential form of participation in the state school system, not a low-cost public display of compliance.

The empirical design exploits staggered station establishment across counties. Treatment indicates whether a county had established a network by the time a child reached age twelve, approximately the transition from primary to junior high school. The difference-in-differences specification compares children within a county who reached that transition before and after establishment. Because adoption timing was not random, earlier-adopting counties tended to have better communications infrastructure and higher baseline education. To address this, I absorb county baseline primary- and junior-high completion rates interacted with child-birth-year indicators, allowing initially more and less educated counties to follow different cohort trajectories. Event-study estimates show no systematic pre-treatment trend after accounting for this differential educational convergence.

I complement the county-cohort design with household fixed-effects models that compare siblings whose treatment status differs because the county established its network before one child reached age twelve but only after the other had crossed that threshold. These specifications absorb all time-invariant family-level characteristics shared by siblings, including persistent household resources, parental background, and stable preferences for education.

They do not rule out shocks that coincided with station establishment and affected siblings differently by age, but they substantially narrow the set of plausible alternative explanations.

Broadcasting increased participation in the state school system. Children whose county network was established by age twelve completed approximately 0.4 additional years of schooling, or about 6 percent of the control-group mean. The within-family estimate is similar. Effects appear at both primary and junior-high completion, with the larger within-family response at the transition to junior high school. This transition was relatively costly and often required rural families to engage with a township institution that was less familiar than the local primary school.

The political outcomes do not show a parallel response. Parents in counties reached by broadcasting at birth did not use patriotic or revolutionary vocabulary in their children's names at higher rates. In the one-child-policy analysis, exposure has no detectable effect on the parity thresholds studied, although completed fertility declines modestly in the rural sample. Neither result establishes that broadcasting had no political effects. Together, however, they provide little evidence that the schooling response reflected a broad shift in political vocabulary or policy compliance.

The heterogeneity patterns are consistent with an informational account. Schooling effects are concentrated among rural households and are larger among children of illiterate household heads, for whom written information was a less useful substitute. Effects are smaller in counties with higher surname concentration, an indirect proxy for stronger local lineage networks that could mediate access to information (Duara, 1988). The clearest pattern concerns language. Effects are largest in fringe-Mandarin regions, where local speech differed from standard Mandarin but broadcasts remained intelligible. These subgroup estimates do not identify mediation, but their non-monotonic shape is more consistent with a role for comprehension than with a simple persuasion account.

These findings contribute to three literatures. First, this paper speaks to research on

authoritarian rule, state-building, and political incorporation. Existing accounts often treat participation in state institutions and convergence toward a national identity as mutually reinforcing elements of the same project (Weber, 1976; Scott, 1998; Alesina and Reich, 2015; Cantoni et al., 2017; Bazzi, Fiszbein and Gebresilasse, 2019; Blanc and Kubo, 2024). I show that a state can raise participation without securing identification and that the two are separable. Participation requires that a household can perceive an institution well enough to use it, a condition that is prior to belief and can be relaxed on its own. The same broadcasts that did not move political naming or fertility at the thresholds the one-child policy enforced, but raised schooling most where households had no alternative source of information. Participation in state institutions therefore measures a state's success in becoming intelligible, which is a different achievement from becoming believed.

Second, the paper contributes to research on mass media and authoritarian politics. While existing work primarily studies media as a vehicle of persuasion and mobilisation (DellaVigna and Kaplan, 2007; Yanagizawa-Drott, 2014; Adena et al., 2015; Gagliarducci et al., 2020; Ou and Xiong, 2021; Qian, 2024), I emphasise its underappreciated capacity to function as public communication infrastructure. Wired broadcasting reached rural populations that could not read the state's newspapers and did not speak its language. The findings show that such infrastructure can expand participation in public institutions.

Third, this paper contributes to research on human capital formation in developing economies. Existing studies show that schooling responds to school construction, teacher supply, and targeted information about educational quality or returns (Duflo, 2001; Chen et al., 2020; Jensen, 2010; Hastings and Weinstein, 2008; Dinkelman and A., 2014). I identify a complementary demand-side channel operating through mass communication infrastructure. The results suggest that communication infrastructure can complement the expansion of schools by reducing the informational and linguistic barriers households face when deciding whether to continue children's education. Where institutions exist but are difficult to understand or access, communication infrastructure may increase their use at scale.

2 Historical Context

When the Chinese Communist Party came to power in 1949, it inherited a predominantly rural country with limited mass education and an uneven state presence outside cities and county seats. The Sino-Japanese War had generated an important wave of nationalist mobilisation (Johnson, 1962), but the new regime pursued a more extensive transformation. Drawing partly on the Soviet model of mass organisation, it sought to extend political and administrative authority into the countryside, mobilise rural communities for national development, expand literacy and schooling, and cultivate identification with a common socialist project (Schurmann, 1968; Kenez, 1985). Schools, literacy campaigns, mass organisations, and political communication were central components of this state-building programme.

The rural society into which the new state sought to expand had historically been governed at considerable distance from the political centre. The county magistrate was generally the lowest regularly appointed representative of the imperial state. Below the county seat, governance relied on local gentry, lineage heads, village notables, and institutions such as the *baojia* system (Wu and Fei, 1988; Chü, 1962; Hsiao, 1960).[1] The same intermediaries shaped access to information. Information from outside the village likewise circulated through local intermediaries and periodic markets. Although land reform weakened the traditional gentry after 1949, early Communist campaigns still depended on work teams, village meetings, and local cadres to communicate policies face to face (Hinton, 1966). The political position of local intermediaries had changed, but direct communication between the national state and dispersed rural households remained costly.

The expansion of rural schooling began from a low base. During the Republican period (1912-1949), fewer than 20 percent of school-age children were enrolled nationally, and junior-secondary enrolment stood at approximately 6 percent (Pepper, 1996). Educational provision was concentrated in cities, county seats, and larger market towns. Primary schools

[1] A single Qing magistrate was often responsible for a population of between 100,000 and 300,000 (Skinner, 1977).

became increasingly available within or near villages, but junior-high schools served a much smaller population and were generally located farther from rural households. Most rural parents had received little or no formal schooling themselves. The Communist government expanded rural schools rapidly after 1949, but the presence of a school did not ensure household participation. Children contributed labour to farming, livestock care, household production, and the supervision of younger siblings. These opportunity costs generally increased as children grew older. Continuing beyond primary school could also require additional fees, travel to a township school, or boarding away from the village (Pepper, 1996; Thøgersen, 2002). The transition to junior high therefore represented a particularly consequential decision for rural households. The expected economic returns to further education were also uncertain. Agricultural procurement and pricing policies transferred resources from the countryside to urban industrialisation (Lardy, 1983), while the *hukou* (household-registration) system restricted permanent rural-to-urban migration and access to urban employment. For most rural children, education did not provide an obvious route out of agriculture. Before the nationwide introduction of compulsory education in 1986, continuing in school depended substantially on whether households considered the prospective benefits worth the immediate financial and labour costs.

Limited access to written information further separated rural households from the institutions of the new state. The Accelerated Literacy Method (*suchen shizi fa*), launched nationally in 1952, sought to teach adults a restricted set of commonly used characters through intensive classes (Peterson, 1997; Seeberg, 1990). Yet literacy classes competed with agricultural work, and gains were often difficult to sustain when adults made little subsequent use of written materials (Hayford, 1987; Thøgersen, 2002). Despite the expansion of literacy and schooling, the 1964 census still classified approximately 52 percent of the population aged fifteen and above as illiterate (Peterson, 1997). Newspapers, written notices, textbooks, and administrative documents consequently remained inaccessible to much of the rural population. Spoken language created an additional divide. China contained extensive variation

across Sinitic dialect groups and minority languages (Ramsey, 1987). Local speech remained the language of the household and village, while the Communist government increasingly promoted standard Mandarin (*putonghua*) in schools, administration, and national communication. For many rural children, entering school therefore involved exposure not only to an unfamiliar institution but also to a different linguistic environment.

Wired broadcasting (*youxian guangbo*) created a new means of reaching this population. Beginning in the early 1950s, county stations transmitted programmes through wired networks to loudspeakers installed in village squares, production brigades, fields, and other collective spaces. The timing and organisation of the county-level rollout are described in Section 3. Unlike newspapers and wireless radio, wired broadcasting required neither literacy nor household investment in a receiver. The network remained costly for local governments to construct and maintain, and the establishment of a county station did not imply immediate coverage of every village. Once local loudspeakers were installed, however, households could receive regular state-produced information without purchasing equipment, acquiring literacy, or relying on a cadre to repeat each message face to face.

The technology's arrival registered immediately in village life. When the first loudspeaker was installed in Taojia Village, Jiutai County, a sixty-year-old woman reportedly stood holding a lamp for two hours, listening until the broadcast ended (*Renmin Ribao*, 1953). For households whose knowledge of the world beyond the village had passed through intermediaries and periodic markets, the loudspeaker was the first direct channel to the nation. And unlike wireless receivers, which the price-scissors regime placed beyond most rural families' means (Lardy, 1983), the wired network was collectively funded and collectively heard: peasants could listen while they worked, at no cost beyond being within earshot.

Broadcast schedules combined political instruction with practical and educational material. County stations relayed Party directives, government announcements, political news, and ideological education (*Compilation of Historical Materials of the Central People's Broadcasting Station (1949–1984)*, 1984). Wired broadcasting was therefore an important compo-

nent of the regime's apparatus of political communication and mobilisation. The same schedules included agricultural, public-health, educational, and cultural programming directed at rural audiences (*Radio and Television in Contemporary China*, 1987). Local announcers also read newspaper articles and official documents aloud, converting written material into speech for audiences who could not read. Broadcasting formed part of the government's language policy as well. In 1956, the State Council instructed stations in dialect-speaking regions to include Mandarin programming so that listeners could gradually understand and speak *putonghua* (State Council of the People's Republic of China, 1956).

Schools embodied a similar combination of objectives. They provided literacy and formal knowledge but also exposed children to standard Mandarin, national history, political instruction, and the authority of the socialist state. Wired broadcasting thus brought political, educational, and practical content to households facing limited access to print, substantial linguistic variation, and little prior experience of formal state institutions. This setting makes it possible to examine whether the arrival of the network increased participation in the state school system and whether it was accompanied by greater parental use of the regime's political vocabulary in children's names.

3 Data

I combine newly digitised records on the establishment of county wired broadcasting stations with individual-level data from the 1990 Population Census, the 2000 Population Census and the 2005 Population Mini-Census. The broadcasting records provide the timing of county-level network establishment.

3.1 Wired Broadcasting

I digitise the year in which each county established its first wired broadcasting station from the *Compendium of County Conditions in China* (*Zhongguo Xianqing Daquan*). The *Compendium* is a national survey of county conditions compiled under the Ministry of Civil Affairs in the late 1980s and early 1990s. Drawing on local archival records, county entries describe administrative histories, public infrastructure, and major local events. Their broadcasting sections generally report the year, and occasionally the month, in which the first county-wired station began operation.

I review approximately 2,000 county entries and recover establishment years for 1,175 counties. The earliest stations opened in the early 1950s, and the largest wave of establishment followed the 1956 *National Agricultural Development Plan*. Adoption nevertheless remained staggered through the following decades. Figure 1 maps the geographic distribution of establishment dates.

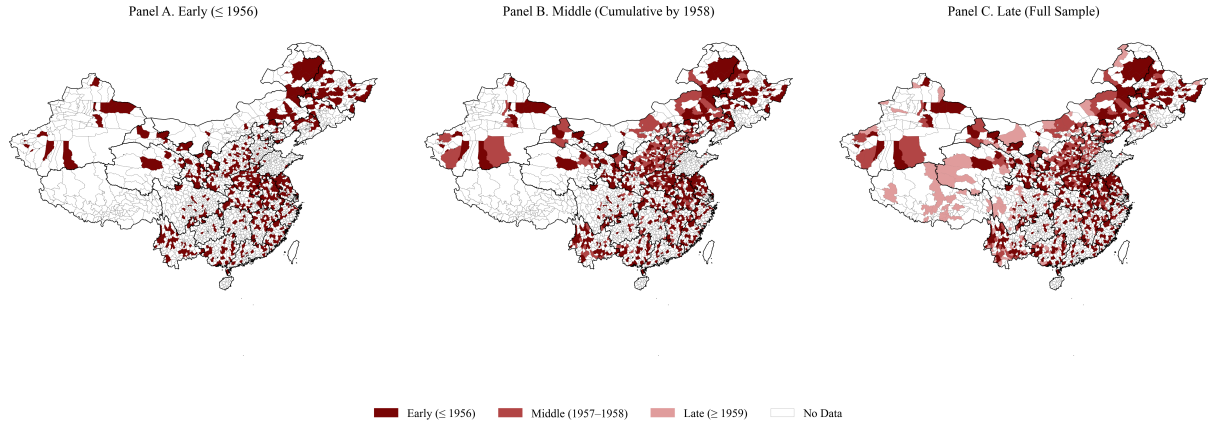


Figure 1: Wired Broadcasting Rollout by County

Source: Compendium of County Conditions in China.

Notes: Counties are coloured by tertiles of first establishment year: *Early* (≤1956), *Middle* (1956–1958), and *Late* (≥1958). Blank areas indicate missing data or unmatched geometry. Base map uses 1990 boundaries.

The historical county units in the *Compendium* do not always correspond directly to the administrative geography reported in later censuses. I harmonise historical county names and

boundary changes with the county identifiers used in the 1990 Population Census. Counties for which the historical administrative unit cannot be reconciled with the census geography, or for which no establishment date can be recovered, are excluded from the analysis[2]. The establishment date refers to the opening of the first county station. It does not record when each village loudspeaker was connected or whether a particular household listened. The measure should accordingly be read as the timing of county-level network establishment, not as verified village coverage or individual exposure.

3.2 Educational Attainment

My main source for educational outcomes is the 1% sample of the 1990 Population Census. The census records individuals' educational attainment, household-registration status, county of residence, relationship to the household head, and a range of demographic characteristics. It also identifies the education, literacy, birth year, and ethnicity of the household head.

I restrict the educational sample to individuals who reside in their county of registration and were born between 1930 and 1972. The local-*hukou* restriction makes the observed county more likely to correspond to the county in which the individual spent their school-age years. The cohort bounds retain individuals whose schooling could plausibly have overlapped with the wired broadcasting rollout and ensure that the main educational transitions are observed by 1990. They also limit the analysis to cohorts whose key schooling decisions largely preceded the widespread diffusion of television and other newer forms of mass media, particularly in rural areas, thereby reducing contamination from subsequent changes in the information environment. The baseline educational analysis includes both rural- and urban-*hukou* individuals. I subsequently estimate the effects separately by household-registration status to examine whether the response was concentrated in rural areas, where wired broad-

[2]I also compare the dates recorded in the *Compendium* with independently compiled county gazetteers to do the validation. The two sources align closely.

casting was primarily deployed and alternative sources of information were more limited. The heterogeneity analyses focus on the rural-*hukou* sample, which most closely corresponds to the historical population targeted by the wired broadcasting network.

I link each person recorded as a child of the household to the household head. The resulting data contain approximately 540,000 household-head-child pairs. Households contribute one observation for each co-resident child, allowing me to measure the timing of county broadcasting relative to each child's schooling and to examine heterogeneity by characteristics of the household head. The linked data exclude children who had left the parental household by 1990. Because co-residence may depend on education, migration, marriage, and household formation, the linked sample may be selected on outcomes that were themselves affected by broadcasting. A plausible channel would attenuate the estimated schooling effect if more educated children were more likely to leave the household, although the direction of selection is not unambiguous.

The principal outcome is completed years of schooling. I convert the census education categories into years of attainment, assigning zero years to no schooling, six to primary school, nine to junior high, twelve to senior high or vocational school, fifteen to college, and sixteen to university or above. I use reported completion status to distinguish completed levels from partial attendance. I also construct indicators for completing primary, junior-high, and senior-high education. Junior-high completion captures the transition beyond primary schooling, when rural children generally faced higher direct and opportunity costs and often had to attend schools outside their village.

3.3 Political Naming

I measure political naming using given names recorded in the 20% sample of the 2005 Population Mini-Census. The sample comprises 239,265 rural Han individuals born between 1940 and 1972 in counties matched to the broadcasting registry; I focus on the rural population

because the network was built to reach villages, and on Han individuals because minority naming conventions follow different traditions and are frequently transliterated. Names were typically assigned at or shortly after birth, so I date each naming decision to the individual's birth year. Chinese given names consist of one or two characters chosen freely by parents rather than drawn from a fixed stock of names, which makes them unusually responsive to their moment: a parent in 1952 could compose *jianguo*("found the nation") as readily as any traditional combination.

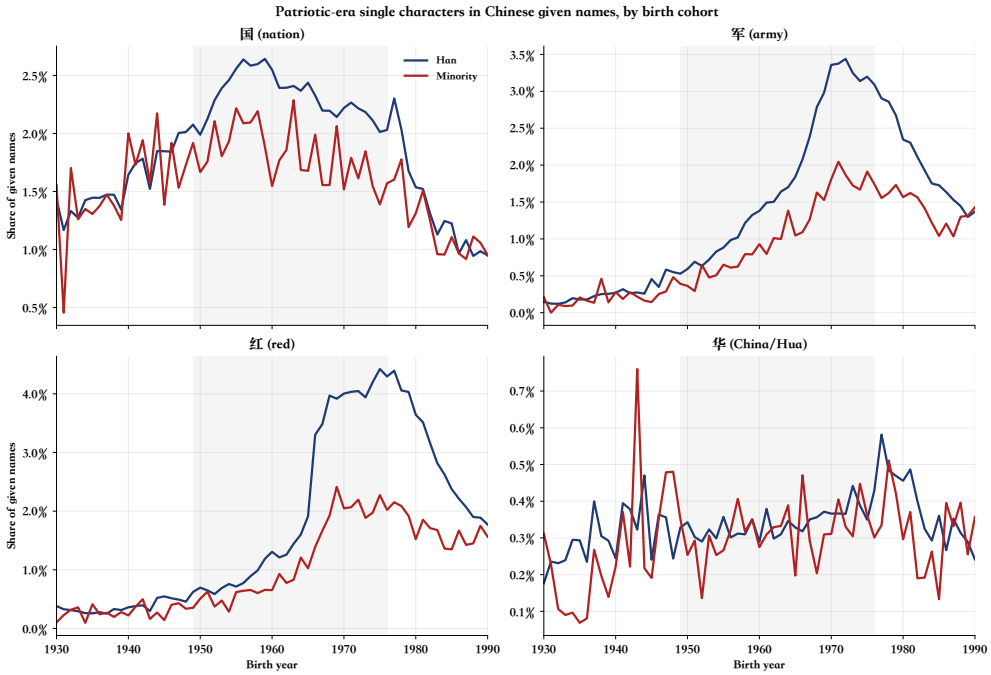


Figure 2: Patriotic Characters in Given Names by Birth Cohort (2005 Mini-Census)

Source: Author's analysis of 2005 20% population census microdata. **Notes:** Each panel plots the share of rural Han given names containing the indicated character across birth cohorts. Shaded band: Mao era (1949–1976). The character *Hua* (华) is shown as a descriptive comparison: it has patriotic connotations but did not undergo a comparable Mao-era spike.

Figure 2 shows what the measure is picking up. The share of rural Han given names containing *guo* (nation), *jun* (army), and *hong* (red) rises sharply after 1949, peaks during the Mao era, and recedes among later cohorts. *Hua*, which carries patriotic connotations but was not part of the era's mobilisational vocabulary, shows no comparable spike, which indicates that the measure tracks the specific political register of the period rather than a

general drift toward patriotic-sounding characters.

I classify names into four registers, specified before estimating any regression and reported in full in Appendix A. The patriotic and revolutionary measure is an indicator for whether a given name contains characters or compounds drawn from the political vocabulary of the era*guo* (nation), *jun*(army), *jian*(to build), *wei* (to defend), *hong*(red), *ge*(revolution), and compounds such as *weidong*("defend Mao"), *yuanchao*("aid North Korea"), and *yuejin*("great leap"). Three further registers provide comparison categories. The Confucian measure captures classical virtues and literate-elite vocabulary; the aspirational measure captures wealth, longevity, and good fortune; the concrete measure captures animals, plants, objects, birth-order prefixes, and son-soliciting compounds, expressions such as *Zhaodi* literally menas "beckon a younger brother" [3]. The comparison registers matter because rural naming was changing along several dimensions at once during this period, becoming more literate and less concrete, and without them, a general movement away from *Dog* and toward any two-character literary name could be mistaken for a political response.

I complement the classified measures with a data-driven index that lets the data identify the vocabulary of the period. For each given-name character q appearing at least 200 times in the sample, I calculate

$$Lift_q = \log\left(\frac{p_q^{post} + \epsilon}{p_q^{pre} + \epsilon}\right), \tag{3.1}$$

where p_q^{post} is the character's frequency among given-name characters for cohorts born between 1950 and 1976, p_q^{pre} is its corresponding frequency among cohorts born between 1925 and 1948, and $\epsilon = 10^{-6}$ prevents undefined values in empty cells. The individual-level index averages the lifts of the characters in each name:

$$RevolutionaryIndex_i = \frac{1}{L_i} \sum_{q \in Name_i} Lift_q, \tag{3.2}$$

[3]A few coding decisions deserve mention. Characters are counted independently of position, and a name containing characters from more than one register is coded as one in each corresponding category, so the categories are not mutually exclusive. Some characters carry political meaning only in context, Red (*Hong*) can mean revolutionary red or simply the colour.

where L_i is the number of characters in the given name. Characters that became disproportionately common during the Mao era receive high weight whether or not any coder would have labelled them political, so the index captures combinations the classifications miss and provides a check on where their boundaries are drawn.

The index has a corresponding weakness: it identifies characters that rose in frequency over the period, which is not necessarily the same as characters with political content. Province-by-cohort fixed effects absorb nationally and provincially common changes in naming fashion, but the index remains a measure of prevalence rather than political meaning. I therefore treat the dictionary-based patriotic measure as the primary outcome and the index as a supplementary check.

3.4 Fertility and the One-Child Policy

I use the 2000 Population Census to study fertility under the one-child policy. The sample includes ever-married Han women born between 1930 and 1975 who reside in their county of registration. The census records children ever born, from which I construct indicators for two, three, and four or more children. This analysis compares women born before 1955 with those born in 1955 or later, whose principal childbearing years overlapped with the policy.

4 Empirical Design

4.1 Baseline Specification

The empirical design exploits variation in the timing of wired broadcasting across counties and birth cohorts. Let R_c denote the year in which county c established its first wired broadcasting station. For each household-head-child pair, where child i was born in year t, I assign treatment according to whether the county broadcasting network had been established

by the time the child reached age twelve:

$$D_{ct}^{12} = \mathbf{1}[R_c \leq t + 12]\,. \tag{4.1}$$

The indicator captures county-level network availability at a child-specific schooling decision point, approximately the transition from primary to junior-high school. Although treatment is indexed by the child's birth cohort, the relevant household decision-maker is the household head. I therefore interpret D_{ct}^{12} as whether the county network had been established before the household head faced that child's schooling transition. Treatment status can consequently differ across children linked to the same household head if the county station was established between their twelfth birthdays. The measure records when the county network was established and serves as a proxy for broadcasting availability at the relevant decision point.

This is a difference-in-differences design with staggered timing. Counties are the units, child birth cohorts index time, and a county switches on when its station opens. In a county whose station opened in 1956, a child born in 1943 reached age twelve in 1955 and is untreated, while a child born in 1945 reached age twelve in 1957 and is treated; β compares that within-county cohort difference with the corresponding difference in counties that adopted at other dates.

For educational outcomes, I estimate

$$Y_{ijcpt} = \beta D_{ct}^{12} + X_{ij}'\gamma + \alpha_c + \delta_{pt} + \varepsilon_{ijcpt}, \tag{4.2}$$

where Y_{ijcpt} is an educational outcome for child i, linked to household head j, born in year t, and observed in county c and province p. County fixed effects α_c absorb persistent differences across counties, while province-by-child-birth-year fixed effects δ_{pt} absorb shocks shared by children born in the same province and year. These include regional differences in

educational policy and cohort-specific exposure to the famine, the Cultural Revolution, and other national disruptions. The baseline educational sample includes both rural- and urban-*hukou* individuals who reside in their county of registration. The vector X_{ij} includes child sex, Han ethnicity, rural-*hukou* status, and the educational attainment of the household head and spouse. Standard errors are clustered at the county level, where broadcasting establishment is measured.

A major concern in my setting is that adoption timing was not random. Earlier-adopting counties tended to have better communications infrastructure and higher initial educational attainment, and they may have followed different educational trajectories even in the absence of broadcasting. I account for this convergence using county baseline primary- and junior-high completion rates. These measures are calculated among cohorts aged 16–25 when the local station was established. Because these cohorts had already crossed the primary-to-junior-high transition, their attainment captures the county's educational position around the time of adoption. In all preferred specifications, I absorb interactions between each baseline education measure and a full set of child-birth-year indicators. I omit these interactions from Equation (4.2) for ease of presentation. They allow initially more and less educated counties to follow different cohort trajectories, instead of imposing a common pattern of educational convergence across counties.

The identifying assumption is that, conditional on county fixed effects, province-specific cohort shocks, and these differential educational trajectories, counties with different broadcasting dates would otherwise have experienced similar changes in schooling across child cohorts. This assumption does not require early- and late-adopting counties to have had the same initial levels of education. It requires that any remaining differences in their cohort trajectories were not systematically aligned with station establishment timing.

I assess this assumption using an event-study version of Equation (4.2). The binary treatment indicator is replaced by indicators for the timing of the child's twelfth birthday relative to county station establishment, defined as $k = (t + 12) - R_c$. Negative values indicate that

the station was established after the child reached age twelve, while non-negative values indicate that it was operating by that age. I omit $k = -1$, corresponding to a station established one year after the child reached twelve, and group observations in the tails of the event-time distribution. The pre-treatment coefficients examine whether educational outcomes were already evolving differently among cohorts whose schooling transition preceded broadcasting. The subsequent coefficients trace how the schooling difference developed as broadcasting became available earlier in childhood. The event study includes the same county fixed effects, province-by-child-birth-year fixed effects, and absorbed baseline-education-by-cohort interactions as the preferred specification. The estimates show no systematic pre-treatment trend after differential educational convergence is taken into account, although they cannot rule out all shocks correlated with station establishment.

I subsequently estimate Equation (4.2) separately for rural- and urban-*hukou* individuals. Wired broadcasting was designed primarily to reach villages, while urban households had greater access to newspapers, wireless receivers, cinemas, and work-unit communication. The rural-urban comparison therefore provides evidence on whether the schooling response was concentrated in the population for whom wired broadcasting was most likely to add a new source of information. It is treated as a diagnostic of the historical scope of the intervention. The subsequent heterogeneity analyses focus on the rural-*hukou* sample.

For the political-naming analysis, the relevant parental decision occurs at birth. I therefore define

$$D_{ct}^{N} = \mathbf{1}[R_c \leq t]\,,$$

which equals one when the county station was operating in the individual's birth year. I estimate a county-cohort specification analogous to Equation (4.2) using the rural Han naming sample described in Section 3.3. The naming regressions include county fixed effects and province-by-birth-year fixed effects. County fixed effects absorb persistent local naming

practices, while province-specific cohort effects absorb broader political and cultural changes in naming across the Mao era. The schooling and naming specifications do not identify parental motives directly. They allow me to compare the effect of broadcasting on costly participation in the state school system with its effect on parents' use of political vocabulary in children's names.

4.2 Within-Household Specification

In additional specifications I replace the county fixed effect with a household fixed effect, comparing siblings who reached age twelve on opposite sides of their county's broadcasting date. The sample is households with at least two co-resident children whose treatment status differs. I estimate

$$Y_{ijcpt} = \beta^{HH} D_{ct}^{12} + X_i' \gamma^{HH} + \mu_j + \delta_{pt} + \varepsilon_{ijcpt}, \tag{4.3}$$

where μ_j is a household fixed effect, which also absorbs the county fixed effect. As in the county-cohort specification I include the baseline-education-by-cohort interactions, omitted from Equation (4.3) for ease of presentation. Because parental and household characteristics are constant across siblings, the remaining individual control is child sex.

Identification now comes from comparing siblings in the same household who reached age twelve on opposite sides of the county's broadcasting date. Household fixed effects absorb all time-invariant characteristics shared by siblings, including parental background, persistent household resources, residential location, and stable preferences for education. Because siblings in the same household are observed in the same county, the household fixed effect also absorbs the county fixed effect.

The within-household comparison absorbs persistent differences between early- and late-adopting counties and stable differences across families as explanations for the estimated

effect. It does not eliminate shocks that coincided with station establishment and affected siblings differently according to their age, birth order, or stage of schooling. Moreover, the sibling sample is selected on multiple children remaining co-resident with the household head in 1990. For this reason, the household fixed-effects estimate serves as a complementary within-family comparison.

5 Main Results

Table 1 reports the main estimates for children's educational attainment. Columns (1), (3), and (5) use the county-cohort specification, while Columns (2), (4), and (6) replace county fixed effects with household fixed effects. All specifications include province-by-child-birth-year fixed effects and county baseline primary- and junior-high completion rates interacted with child-birth-year indicators.

Table 1: Wired Broadcasting and Children's Educational Attainment

	Years of Education		Primary Completion		Junior-High Completion	
	County FE	Household FE	County FE	Household FE	County FE	Household FE
	(1)	(2)	(3)	(4)	(5)	(6)
Broadcast Exposure before Age 12	0.401***	0.415**	0.063***	0.034*	0.053**	0.066**
	(0.101)	(0.158)	(0.013)	(0.022)	(0.023)	(0.032)
Observations	540,488	377,064	540,488	377,064	540,488	377,064
R-squared	0.273	0.622	0.212	0.607	0.213	0.613
Control mean	6.735	6.943	0.773	0.792	0.432	0.401
County FE	Yes	–	Yes	–	Yes	–
Household FE	–	Yes	–	Yes	–	Yes
Province × Child Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Education × Child Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Child Controls	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	–	Yes	–	Yes	–

Notes: This table reports estimates of the effect of county-network establishment by age twelve on children's educational attainment. Columns (1), (3), and (5) include county fixed effects; Columns (2), (4), and (6) include household fixed effects. All specifications include province-by-child-birth-year fixed effects and county baseline primary- and junior-high completion rates interacted with child-birth-year indicators. The county-cohort specifications control for child sex and ethnicity, rural-*hukou* status, and the educational attainment of the household head and spouse. The household fixed-effects specifications control for child sex; household-level controls and other characteristics without within-household variation are absorbed by the household fixed effects. Standard errors, clustered at the county level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Column (1) shows that children whose county network had been established by age twelve completed 0.401 additional years of schooling, approximately 6 percent of the control-group mean. The household fixed-effects estimate in Column (2) is 0.415 years and is statistically significant at the 5 percent level. The similarity of the estimates suggests that stable differences in household background are not the sole source of the county-cohort result[4].

The remaining columns examine completion at different levels of schooling. In the county-cohort specification, broadcasting increases primary completion by 6.3 percentage points and junior-high completion by 5.3 percentage points. The corresponding household fixed-effects estimates are 3.4 and 6.6 percentage points. Continuing to junior high generally involved higher direct costs, greater demands on children's time, and, in many places, travel to or residence near a township school. The estimates are therefore consistent with broadcasting affecting both initial participation and the decision to continue beyond primary school.

The larger within-family response at this transition is consistent with the information account developed below. Village primary schools were local and familiar. Junior-high schools were commonly located in townships, used standard Mandarin, charged fees, and required families to navigate admission procedures. Broadcasting may have been more useful where these features made the institution less familiar to rural households.

Figure B.5 plots coefficients from the event-study version of the preferred specification. Event time is defined as $k = (t+12) - R_c$, with $k = -1$ omitted. The reference group therefore consists of children whose county station was established one year after they reached age twelve. The estimates before treatment fluctuate around zero and display no systematic trend after accounting for differential educational convergence across counties. The coefficients become positive among cohorts for whom the network was operating by age twelve and

[4]For comparison, Chen et al. (2020) estimates that the Send-Down Movement increased rural schooling by approximately 0.07 years for a one-standard-deviation increase in sent-down-youth density, while Duflo (2001) finds that Indonesia's mass school construction programme generated an additional 0.12–0.19 years of schooling per school built. Although the treatment units differ across these studies, the broadcasting effect documented here is substantial in magnitude, particularly given that wired broadcasting did not require a direct expansion of school infrastructure.

remain broadly positive for cohorts with earlier access. The estimates are not characterised by a perfectly sharp break, but they provide no indication that the schooling differences were already increasing systematically before the relevant exposure window[5].

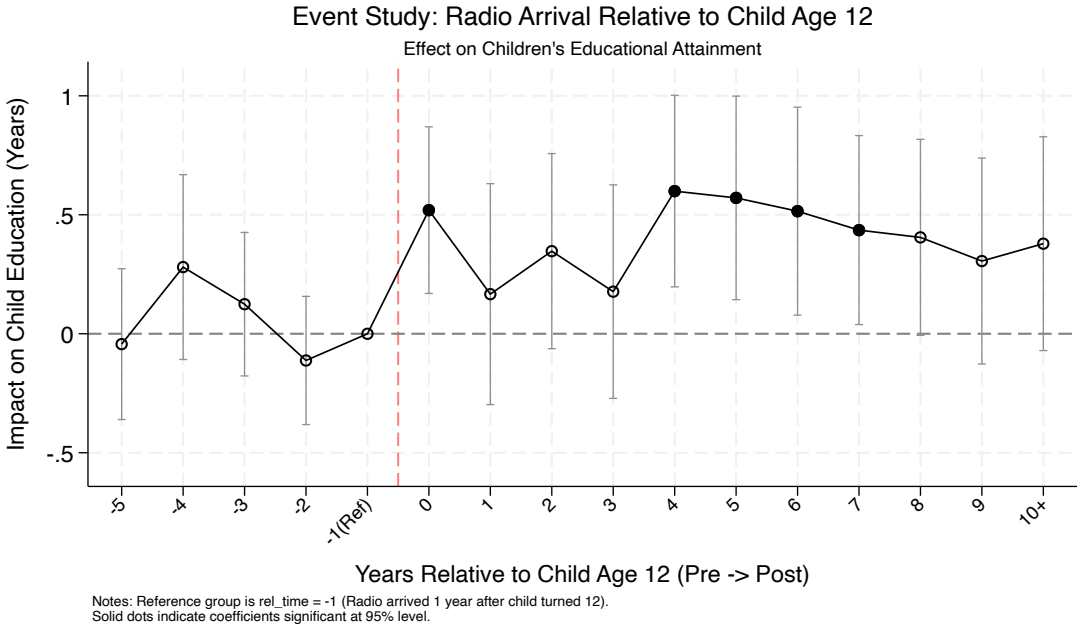


Figure 3: Event Study: Effect of Broadcast Arrival on Child's Years of Schooling

Notes: The figure plots event-study coefficients, with event time defined as $k = (t+12) - R_c$, where t is the child's birth year and R_c is the year in which the county broadcasting station was established. The omitted category is $k = -1$, corresponding to station establishment one year after the child reached age twelve. The specification includes county fixed effects, province-by-child-birth-year fixed effects, and absorbed interactions between county baseline primary- and junior-high completion rates and child-birth-year indicators. Vertical bars report 95% confidence intervals based on standard errors clustered at the county level.

6 Testing the Political Persuasion

The educational estimates establish a response in school participation, but not its mechanism. A persuasion account implies that broadcasting should also affect other margins on which households could express political alignment. This section examines political vocabulary in children's names and fertility under the one-child policy.

[5]Appendix C reports robustness to school-supply expansion, matched-county samples, and alternative treatment definitions.

6.1 Political Naming

Names provide a low-cost symbolic margin on which parents could adopt the regime's political vocabulary. They were assigned at birth, required no interaction with an institution, and persisted over the life course. As Figure 2 shows, patriotic and revolutionary names became substantially more common during the Mao era. The question is whether local network establishment contributed to this broader political change. For this analysis, treatment is defined at birth ($R_c \leq t$), when names were assigned. County fixed effects absorb persistent local naming customs, and province-by-birth-year fixed effects absorb the national and provincial political wave, so identification comes from comparing cohorts within the same county born before and after the local network opened, relative to provincial peers.

Table 2 reports the estimates. The effect of broadcasting on patriotic naming is 0.000, with a standard error of 0.004. The 95-percent confidence interval rules out effects larger than roughly 0.8 percentage points in either direction: a small fraction of the Mao-era change visible in Figure 2.

Table 2: Wired Broadcasting and Children's Naming Practices

	Patriotic (1)	Confucian (2)	Aspir. (3)	Concrete (4)	Rev. Index (5)
Network Established by Birth	0.000	0.008**	−0.007*	0.003	−0.014***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Observations	239,265	239,265	239,265	239,265	239,265
County FE	Yes	Yes	Yes	Yes	Yes
Prov × Cohort FE	Yes	Yes	Yes	Yes	Yes

Notes: Sample: Han rural individuals born 1940–1972, 2005 Population Mini-Census linked to the broadcasting registry. Birth county is proxied by hukou registration county. Network Established by Birth = 1 if broadcasting arrived in the birth county before the individual's birth year. All specifications include county fixed effects and province × birth-cohort fixed effects, which absorb the national political wave. Outcomes are binary indicators for character categories; Rev. Index is a data-driven log-lift index. Standard errors clustered at the county level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The remaining outcomes provide no evidence of a general shift toward the regime's vo-

cabulary. The data-driven revolutionary index declines by 0.014 standardised units, concrete naming is close to zero, and aspirational naming falls by 0.7 percentage points. Confucian naming increases by 0.8 percentage points. Because the index includes characters whose popularity may have changed for non-political reasons, I treat the pre-specified patriotic measure as the primary outcome and attach little weight to the sign of the index estimate.

However, the null effect is not easily explained by an absence of exposure. Contemporaneous reports describe villagers in remote areas learning of Stalin's death through wired loudspeakers in March 1953 and responding emotionally to the announcement (Renmin Ribao, 1953; Li, 2006). Such accounts do not establish average listening or political persuasion, but they show that the network could carry national political events to rural audiences. In the naming data, however, local network establishment did not increase parents' use of the regime's political vocabulary. Naming captures only one symbolic margin and cannot rule out political effects on attitudes that leave no trace in names. The results nevertheless provide no evidence that the schooling response was accompanied by a broader shift toward the regime's political language.

6.2 Fertility under the One-Child Policy

The one-child policy provides a supplementary behavioural outcome. Introduced in 1979 and enforced through the 1980s, it placed substantial constraints on fertility and was also communicated through the wired network. I use the childhood-exposure indicator from the education analysis and interact it with an indicator for women whose childbearing years fell under the policy. The specification compares the exposure gradient in fertility before and after the policy across women in the same county, with county and province-by-birth-year fixed effects. The analysis uses the 2000 Population Census and reports rural and urban samples separately.

Table 3 reports estimates for children ever born and indicators for two, three, and four

or more children. In the rural sample, the interaction is negative but statistically indistinguishable from zero for each parity threshold. Urban estimates are also small and imprecise. Completed fertility in the rural sample declines by 0.060 children and is significant at the 10-percent level. This isolated estimate is modest relative to the control mean and is not accompanied by changes at the parity thresholds. It may also reflect the effect of childhood exposure on education, which can affect fertility independently of policy compliance. I therefore treat the fertility analysis as supplementary evidence, not as a stand-alone test of persuasion.

Table 3: Wired Broadcasting and Fertility under the One-Child Policy

	≥ 2 children		≥ 3 children		≥ 4 children		Children ever born	
	Rural (1)	Urban (2)	Rural (3)	Urban (4)	Rural (5)	Urban (6)	Rural (7)	Urban (8)
Broadcast before Age 12 × OCP Cohort	−0.018	0.012	−0.012	0.007	−0.014	0.009	−0.060*	0.029
	(0.017)	(0.022)	(0.020)	(0.020)	(0.012)	(0.010)	(0.032)	(0.047)
Observations	494,800	81,119	494,800	81,119	494,800	81,119	494,800	81,119
R-squared	0.448	0.365	0.314	0.182	0.177	0.076	0.463	0.378
Control mean	0.658	0.546	0.410	0.172	0.152	0.048	1.969	1.494
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Birth-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample: ever-married Han women born 1930–1975 in the 2000 Population Census who reside in their county of registration. Columns are split by household-registration status. The reported coefficient is the interaction between childhood broadcast exposure, $D_{ct}^{12} = \mathbf{1}[R_c \leq t + 12]$, and an indicator for women born in 1955 or later, whose principal childbearing years overlapped with the one-child policy. Dependent variables are indicators for having borne at least two, three, and four children, and the count of children ever born. All specifications include county fixed effects and province-by-birth-year fixed effects. Standard errors clustered at the county level in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Taken together, the political outcomes do not support an interpretation of the schooling effect as part of a broad shift in political alignment or policy compliance. They do not establish that broadcasting left attitudes unchanged. They instead help distinguish an account based primarily on persuasion from one in which broadcasting changed what households knew about, and could do with, state institutions. The next section examines whether the schooling response was concentrated where low-barrier audio communication was likely to add the most: among households facing limited access to written information, linguistic

distance from standard Mandarin, and few alternative information channels.

7 Information Endowments and Language

If broadcasting reduced information and comprehension constraints, effects should be larger where households had few alternative sources of information and where broadcasts were sufficiently understandable. This section examines heterogeneity by access to information, linguistic distance from standard Mandarin, household circumstances, and the local institutional environment. These patterns are consistent with particular mechanisms but do not constitute formal tests of mediation.

7.1 Access to Alternative Information

Wired broadcasting was designed primarily to reach villages. Rural households also had more limited access to newspapers, wireless receivers, cinemas, and work-unit communication than urban households. If the network added value by providing a new source of information, its educational effect should therefore be more visible in rural areas.

Table 4 reports estimates separately by *hukou* status. The contrast is clearest for completed years of schooling. Broadcasting increases rural schooling by 0.397 years, while the urban estimate is -0.104 and statistically indistinguishable from zero. The rural estimates are also positive for primary and junior-high completion. The urban completion estimates are imprecise, and the point estimate for junior-high completion is not smaller than the rural estimate. I interpret the comparison as evidence about the historical scope of the intervention, but not as a uniform rural-urban difference across every educational outcome [6].

[6] Statistical significance in the rural sample and insignificance in the urban sample do not by themselves establish that the two effects differ. A pooled interaction specification provides the appropriate test of the rural-urban contrast.

Table 4: Heterogeneity by Hukou Status: Rural vs. Urban Sample

	Years of Education		Primary Graduate		Junior High Graduate	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Broadcast Exposure before Age 12	0.397***	−0.104	0.045***	0.033	0.052**	0.072
	(0.121)	(0.199)	(0.016)	(0.024)	(0.026)	(0.047)
Observations	394,431	45,537	394,431	45,537	394,431	45,537
Control Mean	6.595	9.250	0.763	0.959	0.412	0.796
R-squared	0.226	0.273	0.210	0.163	0.166	0.212
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × Child Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × Head Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Edu × Cohort Trend	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the effect of radio exposure on children's educational attainment, separately for rural and urban hukou holders. Broadcast Exposure before Age 12 equals one if wired broadcasting arrived in the county before the child turned 12. Demographic controls include ethnicity, head's education, and spouse's education. Standard errors clustered at the county level in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A similar pattern appears across households with different access to written information. Figure 4 shows that the estimated schooling effect is larger among children of illiterate household heads than among children whose household heads could read. Printed notices, newspapers, and administrative documents provided a less usable alternative for illiterate heads, whereas loudspeaker broadcasts could be received without literacy.

The subgroup estimates are consistent with broadcasting relaxing an information constraint. For households with limited literacy, the relevant difficulty was not simply receiving a political message. It was learning that schools existed beyond the village, what enrolment required, what continued attendance would cost, and what benefits it might offer. In the absence of accessible print, such information had to be obtained through cadres, relatives, or occasional travel to a market town. Wired broadcasts could lower these search and comprehension costs by making official notices, educational guidance, and information about state programmes audible in the village. Literate households had more usable substitutes, like newspapers, written notices, and administrative documents, and therefore had less to gain

from the same network.

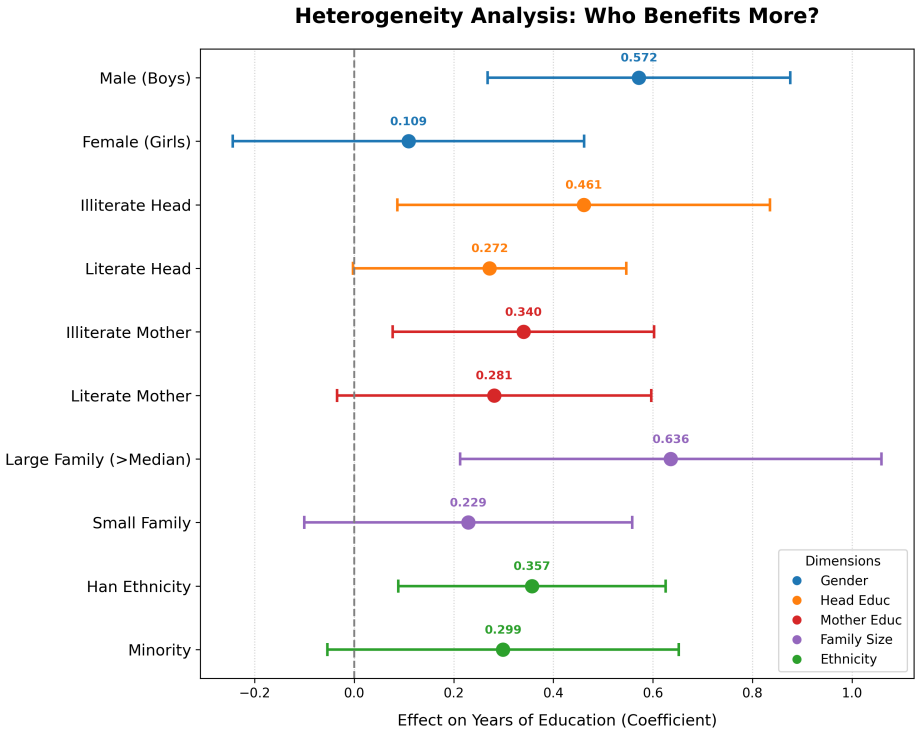


Figure 4: Heterogeneous Effects of Broadcasting on Years of Schooling

This interpretation does not require that households accepted the political content of the broadcasts. Rather, broadcasting could change a schooling decision by making an unfamiliar institution easier to evaluate and navigate. The larger estimates among children of illiterate household heads are consistent with this account, although the subgroup comparisons do not by themselves identify the specific information that mattered. The point estimates are also larger for boys, suggesting that the response remained shaped by existing gender differences in household educational investment.

7.2 Linguistic Distance

The value of broadcasting also depended on whether listeners could understand the language of programming. Central and provincial broadcasts generally used standard Mandarin, whereas everyday speech varied considerably across rural China. Effects need not be mono-

tonic in linguistic distance: the network may have added little where standard Mandarin was already familiar, but have been difficult to understand at very high levels of distance.

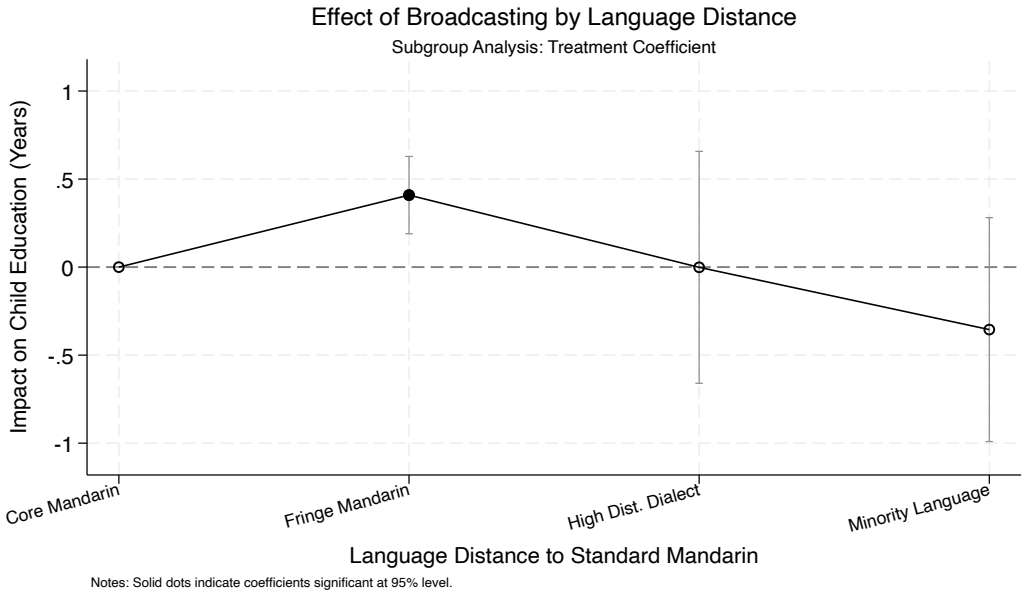


Figure 5: Effect of Broadcasting by Language Distance

Notes: Estimated treatment coefficients on years of schooling, separately for four subsamples defined by linguistic distance from standard Mandarin: *Core Mandarin* (Beijing-area dialects); *Fringe Mandarin* (Mandarin-family dialects with intelligibility frictions); *High Distance Dialect* (Wu, Yue, Min, Hakka, etc.); *Minority Language* (non-Sinitic languages). Solid dots indicate coefficients significant at the 95% level. The pattern is non-monotonic: broadcasting has its largest effect in fringe Mandarin areas, where the linguistic distance is binding for schooling but bridgeable by audio in standard Mandarin. Effects are statistically zero in core Mandarin and high-distance dialect regions, and negative (though imprecisely estimated) in minority-language regions.

Figure 5 reports estimates across four linguistic groups. The schooling response is largest in fringe-Mandarin regions. Estimates are close to zero in core Mandarin and high-distance Sinitic dialect regions, and negative but imprecise in minority-language counties. The resulting pattern is non-monotonic. It is consistent with broadcasting being most useful where the language of national institutions differed from local speech but remained sufficiently intelligible. The subgroup estimates do not establish that language comprehension is the mechanism, and formal comparisons across groups depend on the corresponding interaction tests.

The pattern also helps distinguish institutional legibility from a simple persuasion account. If broadcasts changed schooling primarily by persuading households of the regime's political message, their effect should, other things equal, be greatest where that message was easiest to understand: in core Mandarin areas. Linguistic distance should attenuate persuasion because it makes the message harder to follow. The estimates do not display this ordering. Effects are close to zero in core Mandarin areas and largest in fringe-Mandarin regions.

An institutional-legibility account instead predicts a non-monotonic relationship. In core Mandarin areas, households could already understand the language used by schools and national communication, so broadcasts may have added relatively little information. At very high levels of linguistic distance, Mandarin broadcasts may have been too difficult to follow. Fringe-Mandarin regions lie between these cases: the language of national institutions was sufficiently unfamiliar to create a barrier, but broadcasts remained intelligible enough to reduce it. This interpretation is not dispositive, because linguistic regions differ along many historical dimensions. Together with the rural and literacy results, however, the pattern is more consistent with institutional legibility than with a simple persuasion account.

7.3 Household Decision-Making

The response to broadcasting also varied with household structure. Table 5 reports household fixed-effects estimates by family size. In Column (1), the interaction between treatment and the number of children is positive. In Column (2), the treatment effect is 1.190 years larger for children in relatively large families, implying an effect of 0.773 years for this group. This pattern indicates differential educational allocation within larger and smaller households, but does not distinguish among possible explanations, including resource constraints and children's labour allocation.

Table 5: Heterogeneous Effects by Family Size (Household Fixed Effects)

	Years of Education	
	(1) Continuous	(2) Relative Dummy
Broadcast Exposure before Age 12	−0.868***	−0.417**
	(0.310)	(0.170)
Treat × Number of Children	0.324***	
	(0.092)	
Treat × Relative Large Family		1.190***
		(0.248)
Observations	417,548	417,548
R-squared	0.627	0.627
Household FE	Yes	Yes
Province × Child Birth Year FE	Yes	Yes
Province × Head Birth Year FE	Yes	Yes

Notes: Household fixed-effects estimates testing the resource-dilution hypothesis. Column (1) interacts broadcast exposure with the continuous total number of children in the family. Column (2) uses a dummy indicating whether family size is above the median of the household head's birth cohort, netting out macroeconomic demographic transitions. The main effects of family size are perfectly collinear with household fixed effects and thus omitted. The results show that the educational benefits of broadcasting are heavily concentrated among children in larger families. In Column (2), the net effect of broadcast exposure for a relatively large family is highly positive and substantial ($-0.417 + 1.190 = 0.773$ years). Standard errors clustered at the county level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

I also examine whether the schooling response varies with the household head's age when the county station was established. Figure 6 plots marginal effects for each age bin. Positive estimates are concentrated among children whose household heads encountered broadcasting at younger ages. This pattern is consistent with earlier familiarity shaping later schooling decisions, but it may also reflect parental cohort, life-cycle position, or family-formation differences.

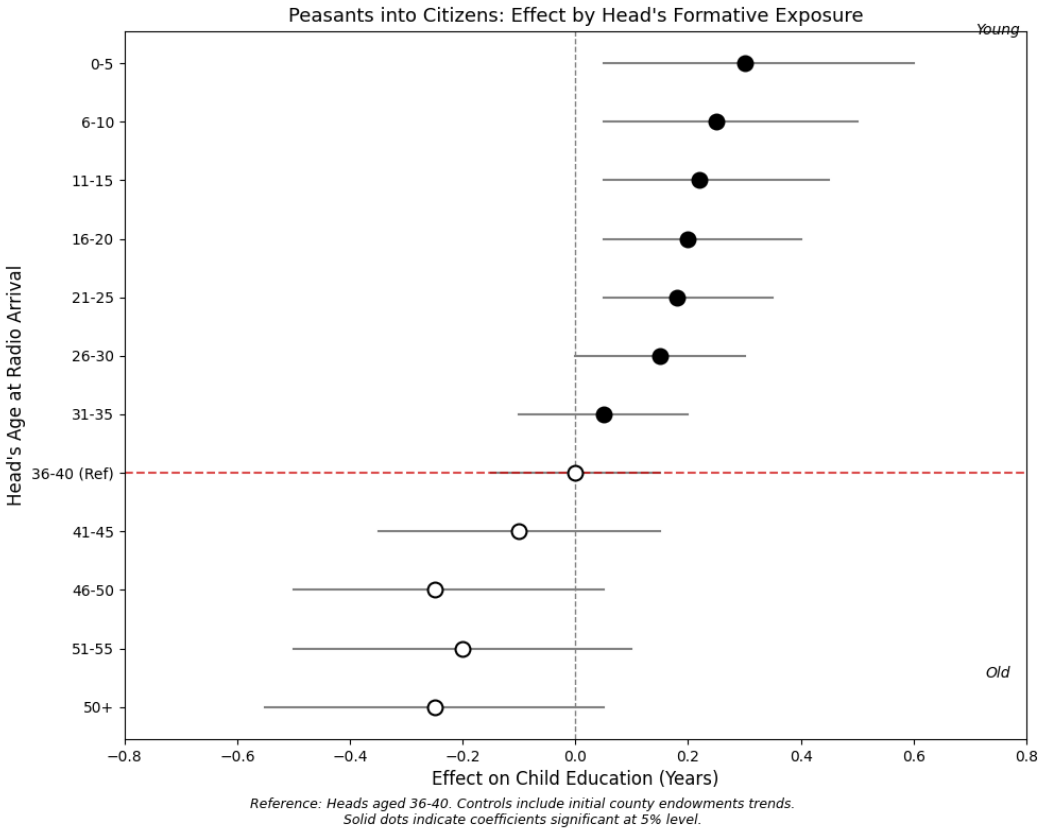


Figure 6: Effect Heterogeneity by Parent's Age at Broadcast Arrival

Notes: This figure plots marginal effects by bin, not interaction coefficients: each point is the estimated effect of broadcasting on years of schooling for children whose household head was in the indicated age range when the county station was established, obtained as the sum of the main treatment coefficient and the relevant interaction term. The reference group is parents aged 36–40 at broadcast arrival. All specifications include county fixed effects and birth year fixed effects. Vertical bars represent 95% confidence intervals based on standard errors clustered at the county level.

7.4 Local Institutional Environment

Finally, I examine heterogeneity by the local institutional environment. I use county surname concentration as an indirect proxy for the persistence of lineage organisation. Chinese lineages (*zongzu*) historically coordinated mutual aid and dealings with outside authorities (Freedman, 1958; Duara, 1988). Where these institutions remained strong, they may have provided an alternative intermediary between rural households and the state. The proxy is imperfect, particularly outside the regions in which corporate lineages were most developed,

so this analysis is suggestive rather than a direct measure of local gatekeeping.[7]

The measure is the Herfindahl index of surname shares in the 2005 Mini-Census:

$$\text{HHI}_c = \sum_s \left(\frac{n_{sc}}{N_c} \right)^2 . \tag{7.1}$$

A higher value indicates that a smaller number of surnames account for a larger share of the county population. Surname concentration is an indirect measure of lineage organisation and need not capture the political influence of clans in every county. Table 6 shows that the interaction between broadcasting and surname concentration is negative. The estimated coefficient is -2.416 with a standard error of 1.329 and is statistically significant at the 10 percent level. The educational effect was therefore smaller in counties with greater surname concentration.

Table 6: Local Power Constraints: Clan Networks and Broadcasting Effects

	Years of Education
Radio Before Age 12	0.498***
	(0.148)
Radio × Surname HHI	−2.416*
	(1.329)
Observations	438,659
R-squared	0.272
County FE	Yes
Prov × Cohort FE	Yes
Baseline Edu × Cohort FE	Yes

Notes: The dependent variable is years of education. Surname HHI is computed at the county level from the 2005 Mini-Census. A higher HHI indicates greater surname concentration and stronger lineage networks. The negative interaction indicates that broadcasting's educational effect is attenuated where clan networks are stronger. Standard errors clustered at the county level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

If surname concentration captures the persistence of lineage institutions, this result is consistent with local social structures moderating the effect of broadcasting. Given the

[7]The mapping is stronger in South China, where corporate lineages held property and maintained halls, than in the North, where shared surnames less often implied functioning lineage institutions (Freedman, 1958).

indirect proxy and limited precision of the interaction estimate, it is best interpreted as suggestive evidence of a boundary condition.

8 Conclusion

This paper studies rural wired broadcasting as both a political medium and a source of information about state institutions. Using the staggered rollout of county networks, I find that children exposed before age twelve completed approximately 0.4 additional years of schooling. The result is similar in within-family comparisons and is concentrated at the transition beyond primary school.

The evidence provides less support for a broad persuasion account. Local network establishment did not increase patriotic or revolutionary naming, a low-cost margin that changed substantially during the Mao era. The one-child-policy analysis shows no detectable effect at the parity thresholds examined, although completed fertility declines modestly in the rural sample. Neither result rules out political effects that leave no trace in names or fertility.

The heterogeneity patterns are more consistent with an informational account. Effects are larger in rural households and among children of illiterate household heads. They are also smaller in counties with greater surname concentration, an indirect proxy for stronger local lineage networks. The clearest pattern concerns language. Effects peak in fringe-Mandarin regions and are close to zero in core Mandarin areas, where the language of schools and national communication was already more familiar. If persuasion were the primary channel, one would expect larger effects where broadcasts were easiest to understand. The observed non-monotonic pattern is instead consistent with broadcasts being most useful where standard Mandarin was sufficiently unfamiliar to create a barrier, but sufficiently intelligible to reduce it.

These results distinguish participation in state institutions from political identification

with the state. When a communication system bundles political messages with practical information, greater use of an institution need not imply acceptance of the political message. Communication infrastructure may therefore expand the effective reach of public institutions where households face substantial informational and linguistic barriers.

The evidence has important limits. County-station establishment measures local availability, not village-level coverage or listening. The linked schooling sample includes children co-residing with a parent in 1990, and names capture only one symbolic political margin. Future work could examine programme content and test whether similar communication infrastructure affected participation in other public institutions.

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A Appendix

Trends in School

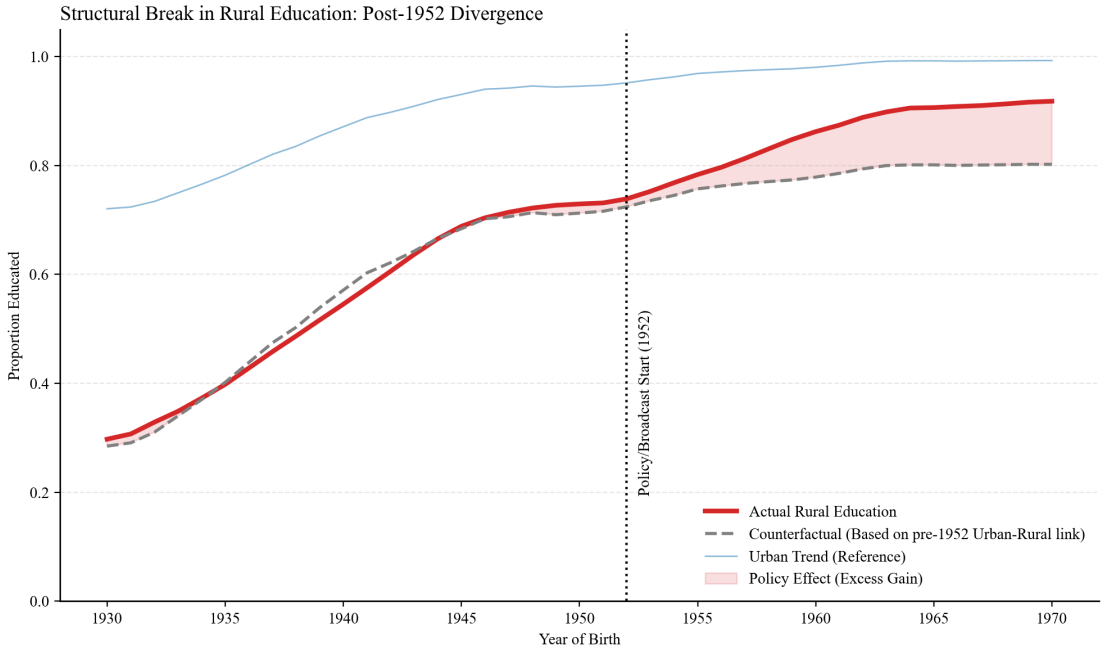


Figure A.1: Trends in School Entry by Urban-Rural

Source: 1990 Population Census microdata.

Note: This figure illustrates the observed versus predicted trends in rural school entry. The predicted (counterfactual) trend is generated using a linear projection based on the pre-1952 structural relationship between rural and urban outcomes ($Rural_t = \alpha + \beta \times Urban_t$). By feeding the observed post-1952 urban school entry rates into this model, we isolate the expected rural trend absent the wired broadcasting rollout. Urban areas serve as a donor pool to control for aggregate macro shocks affecting both sectors (such as the Cultural Revolution). The resulting gap represents the idiosyncratic increase in rural schooling.

Broadcasting Contents

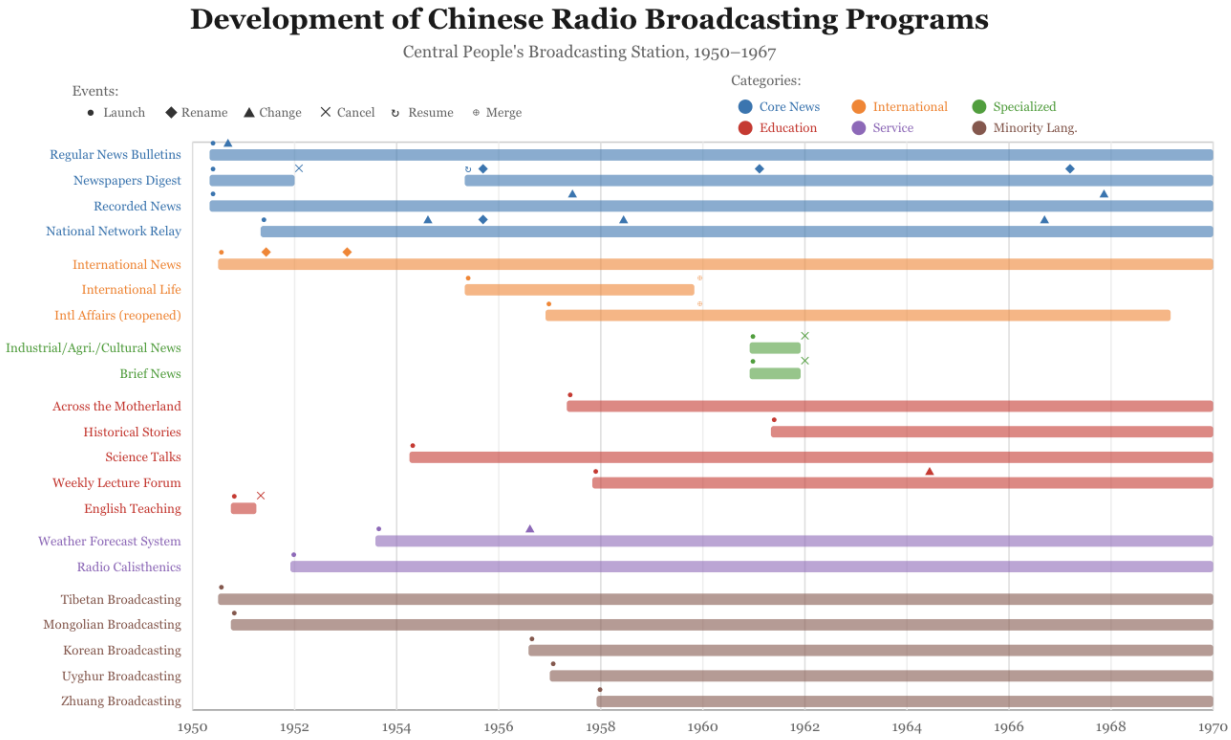


Figure A.2: Composition of Daily Broadcast Content

Source:Compilation of Historical Materials of the Central People's Broadcasting Station (1949–1984)

Note: Data compiled from the author's digitization of Compilation of Historical Materials of the Central People's Broadcasting Station (1949–1984). The chart displays the average share of airtime allocated to political news, agricultural/developmental knowledge, and cultural/educational programming. All content(except minority language broadcasting) was delivered in Standard Mandarin.

Radio line density

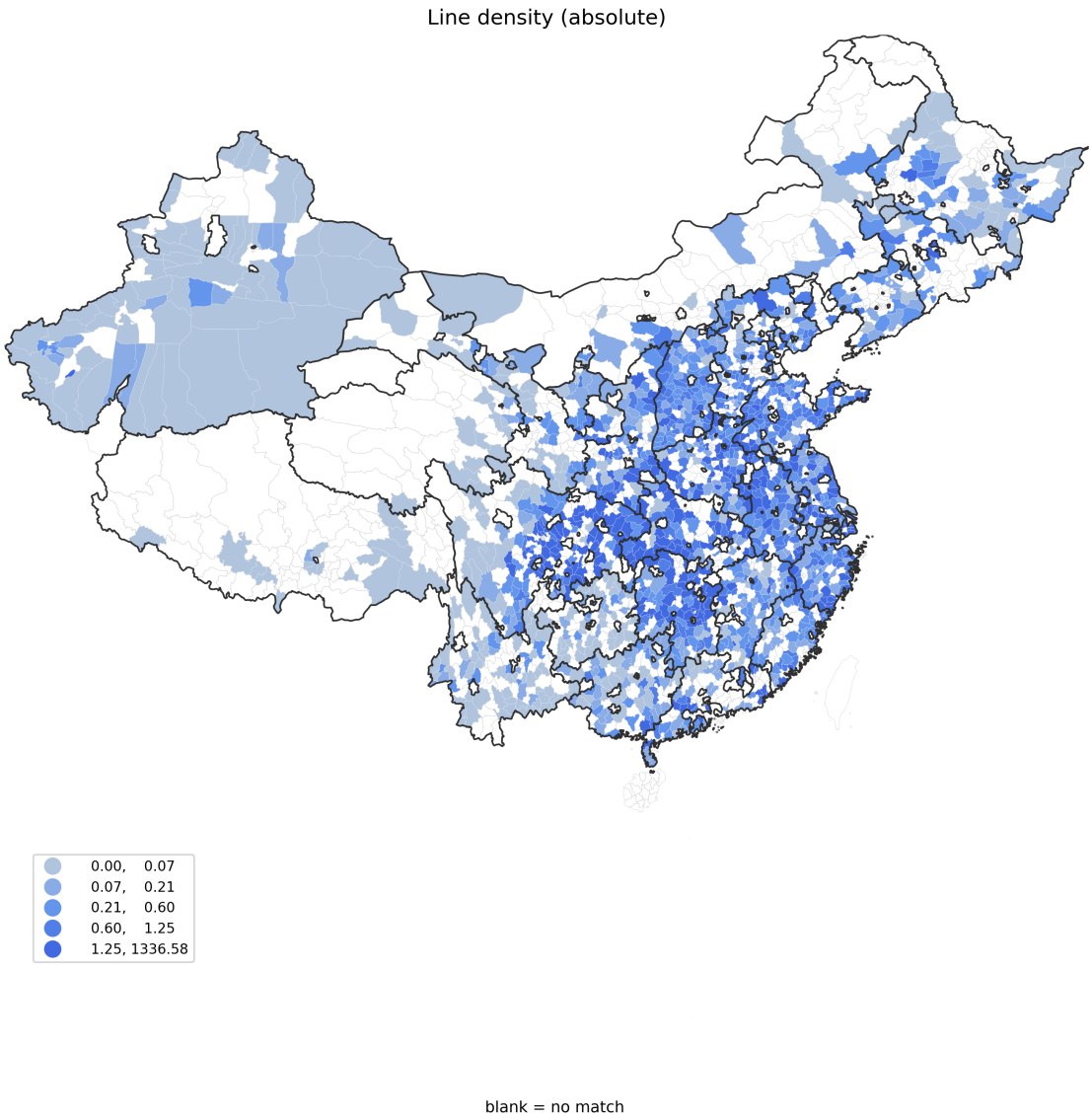


Figure A.3: Radio line density (county level)

Source:Compendium of County Conditions in China

Notes. Counties are shaded by wired-radio *line density* (total line length in pole-km divided by county area in km^2). Shading uses quintiles across matched counties (darker = higher density). White polygons indicate missing line length/area (no density) or unmatched geometry. Base map uses 1990 administrative boundaries; province borders are bold and county borders are light. No top-coding; the legend reports bin cut points. Density is available for about 1,175 counties.

Province Coverage of Radio Station

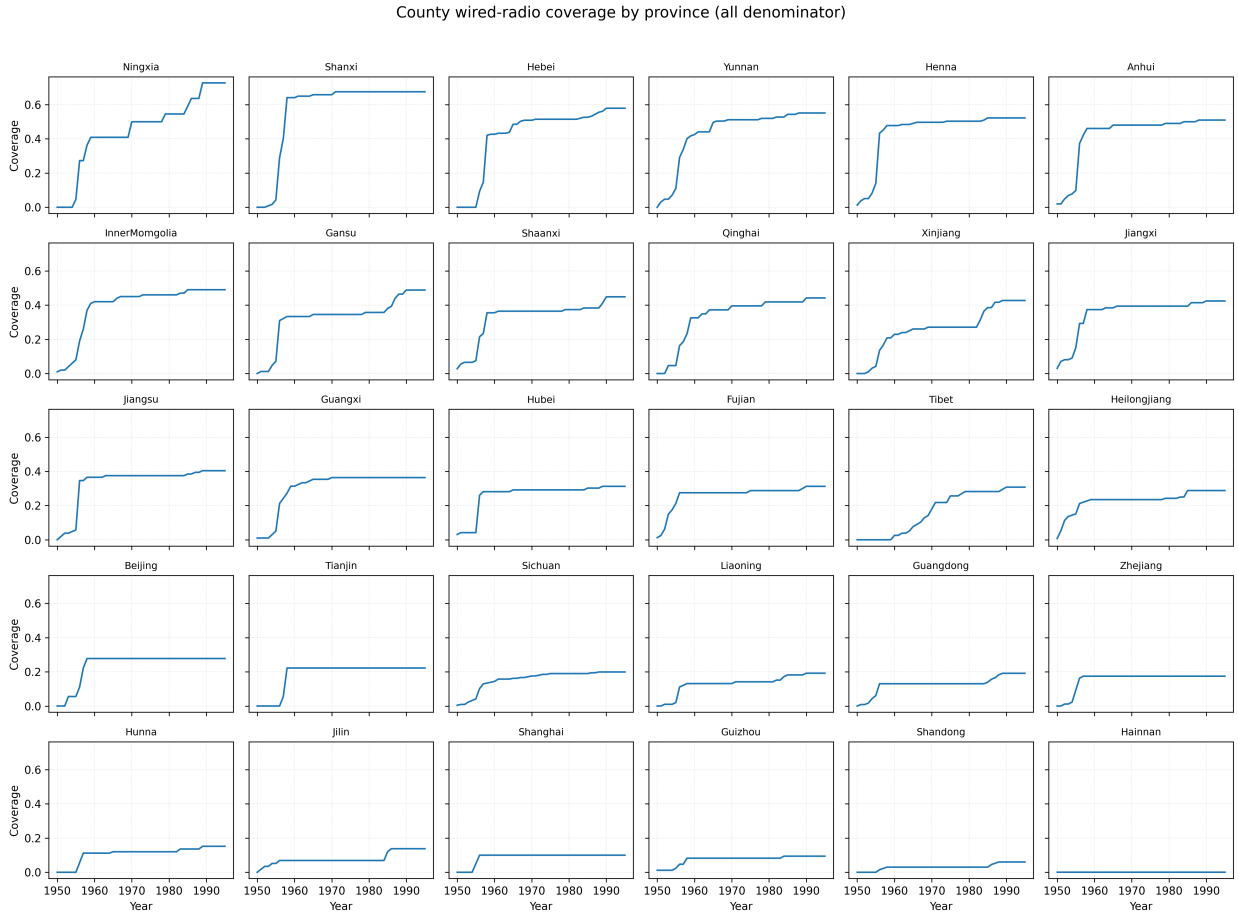


Figure A.4: County wired-radio coverage by province (all denominator)

Source:Compendium of County Conditions in China

Notes. Curves may plateau below 1 because some counties lack an observed establishment year; The establishment year is the earliest year a county reports a wired-radio transmitting/broadcasting station in provincial yearbooks and local gazetteers; out-of-range values are coded as missing.

Naming Classification and Validation

This appendix reports the full construction of the naming measures used in Section 6.1. Table A.1 lists the complete character and compound dictionaries underlying the four registers described in Section 3.3.

Table A.1: Construction of Dictionary-Based Naming Measures

Index	Construction	Interpretation
Patriotic / revolutionary	Indicator equal to one if the given name contains any of {国, 军, 红, 华, 建, 卫, 民, 党, 东, 兵, 忠, 革, 朝, 跃, 援, 解, 放} or compounds including {建国, 卫东, 红军, 解放, 国庆, 援朝, 跃进, 红卫, 忠国, 向东, 卫国, 爱国, 拥军, 学军, 学雷, 学锋, 抗美, 反帝}.	Mao-era national and political vocabulary
Confucian / classical	Indicator equal to one if the given name contains any of {仁, 义, 礼, 智, 信, 德, 孝, 廉, 悌, 贤, 杰, 雅, 文, 学, 哲, 君, 诚, 谦, 逊, 慎, 节, 守, 端, 敬, 恭, 良, 善, 圣, 道, 儒, 章, 颂, 雯, 经, 史, 书, 册, 卷, 翰, 墨, 简, 笔}.	Classical virtues and literate-elite vocabulary
Aspirational / folk	Indicator equal to one if the given name contains any of {富, 贵, 财, 福, 寿, 旺, 安, 平, 顺, 兴, 茂, 盛, 发, 喜, 利, 长, 永, 万, 春, 庆, 禄, 祥, 瑞, 吉, 好, 美, 优, 强, 胜, 立, 来, 招, 望, 盼, 念, 思}.	Wealth, longevity, fortune, and aspiration
Concrete / earthy	Indicator equal to one if the given name contains an animal, plant, object, birth-order prefix, or son-soliciting expression. Animal and rustic examples include {狗, 猫, 牛, 羊, 猪, 兔, 鸡, 鸭, 鱼, 虎, 鼠, 蛇, 蛋, 娃, 崽}; objects and plants include {柱, 石, 山, 田, 林, 树, 草, 花, 苗, 麦, 米, 谷, 土, 砖, 瓦, 锁}; birth-order prefixes include {二, 三, 四, 五, 六, 七, 八, 九, 十, 大, 小, 老}; and son-soliciting compounds include {招弟, 来弟, 引弟, 招男, 望男, 唤男, 招娣, 来娣, 改弟, 盼弟}.	Concrete, low-literacy, and pre-modern rural register
Single-character name	Indicator equal to one if the given name consists of one Chinese character.	Name structure

Notes: The dictionaries are specified before estimating the broadcasting regressions. Robustness exercises remove individually ambiguous characters, restrict the political measure to explicit multi-character compounds, and use alternative machine- and human-coded classifications.

The patriotic and revolutionary register is the main symbolic-alignment outcome. The other registers serve two purposes. First, they show whether any estimated effect is specific to the regime's political vocabulary and not part of a general change in naming styles. Second,

they provide comparison registers associated with classical literacy, household aspiration, and concrete rural naming traditions, the registers from which Mao-era naming drew families away.

Supplementary Variables

Linguistic Distance from Standard Mandarin

I use the county-level language-distance measure constructed by Qin (2022) from the *Language Atlas of China*. Chinese language varieties are organised in a hierarchical tree consisting of supergroups, groups, subgroups, and local clusters. Each county is assigned a distance from the Beijing dialect, the principal phonological basis of standard Mandarin.

The original measure ranges from zero to four:

- 0: the local variety is itself a Beijing-area variety;
- 1: it shares a subgroup with Beijing;
- 2: it shares a broader dialect group with Beijing;
- 3: it shares only the Mandarin supergroup;
- 4: it belongs to another Sinitic supergroup or to a non-Sinitic minority language family.

For the heterogeneity analysis, I group counties into:

(i) core Mandarin areas, where broadcasting added relatively little linguistic exposure;

(ii) fringe Mandarin areas, where the local dialect differed from standard Mandarin but remained sufficiently close for broadcasts to be intelligible; and

(iii) high-distance or minority-language areas, where linguistic distance may have limited comprehension.

The empirical grouping and alternative continuous specifications are reported together in the mechanism section.

Clan-Network Strength

I proxy for local lineage strength using the concentration of surnames in each county. Let s_{kc} denote the share of individuals in county c carrying surname k. The surname Herfindahl–Hirschman Index is:

$$ClanHHI_c = \sum_k s_{kc}^2. \tag{A.1}$$

The index is computed from rural Han individuals in the 2005 Mini-Census. Higher values indicate that a smaller number of surnames account for a larger share of the county population, consistent with a greater local concentration of extended lineage groups. Because surname concentration may also reflect migration, ethnic composition, or population size, the mechanism analysis restricts the construction sample to Han residents and examines robustness to the effective number of surnames, the largest-surname share, and controls for county population and migration.

The index is a proxy for the potential organisational strength of lineage networks. The interpretation therefore relies on the interaction pattern: broadcasting effects are expected to be weaker where local lineage organisations retained greater capacity to mediate information and access to public institutions.

Summary Statistics

Appendix Table A.2 reports descriptive statistics for the education-analysis sample, divided by treatment status and household registration status. Treatment is defined as $D_{ct}^{12} = \mathbf{1}[R_c \leq t + 12]$: a child is treated when the county wired-broadcasting network was established by the time that child reached age twelve. The table is descriptive. Treated children are me-

chanically more likely to belong to later birth cohorts, and later cohorts had more education for many reasons unrelated to broadcasting. The raw differences therefore do not have a causal interpretation. They motivate the province-by-child-cohort fixed effects, the county baseline-education-by-cohort interactions, and the within-household comparison used in the main analysis.

Two patterns are notable. First, treated rural children have higher raw years of schooling and higher completion rates than untreated rural children, while the treated group is also younger on average. Second, rural household heads have substantially lower educational attainment than urban heads, underlining the information and institutional-access constraints that motivate the paper.

Table A.2: Summary Statistics by Treatment Status and Hukou

	Control Group		Treatment Group	
Hukou	Rural (1)	Urban (2)	Rural (3)	Urban (4)
Panel A: Child Characteristics				
Years of education	6.145	6.071	7.124	6.901
	(3.619)	(4.493)	(3.545)	(4.284)
Primary school graduate	0.715	0.889	0.818	0.962
	(0.452)	(0.314)	(0.386)	(0.192)
Junior high graduate	0.373	0.713	0.426	0.836
	(0.484)	(0.453)	(0.495)	(0.370)
Senior high graduate	0.049	0.166	0.050	0.254
	(0.215)	(0.372)	(0.218)	(0.435)
Male	0.581	0.509	0.552	0.510
	(0.493)	(0.500)	(0.497)	(0.500)
Han ethnicity	0.820	0.864	0.917	0.903
	(0.384)	(0.343)	(0.276)	(0.296)
Birth year	1958.72	1957.00	1967.13	1966.80
	(12.70)	(12.98)	(4.94)	(4.91)
Panel B: Household Head Characteristics				
Years of education	3.456	8.661	2.220	9.520
	(3.604)	(4.075)	(2.832)	(2.411)
Spouse years of education	2.091	4.153	4.191	5.058
	(3.118)	(3.171)	(3.149)	(4.259)
Male	0.846	0.762	0.879	0.788
	(0.361)	(0.426)	(0.326)	(0.409)
Han ethnicity	0.836	0.892	0.921	0.915
	(0.370)	(0.311)	(0.270)	(0.279)
Birth year	1930.30	1929.38	1936.10	1935.45
	(11.49)	(11.14)	(8.41)	(7.86)
Observations	10,347	676	473,816	55,726

Notes: Standard deviations in parentheses. The sample is divided by treatment status and *hukou*. Treatment equals one when the county broadcasting network was established by the time the child reached age twelve, $\mathbf{1}[R_c \leq \text{Child Birth Year}_i + 12]$.

B Individual-level Outcome

B.1 Baseline

Table B.3: Baseline Regressions: Radio Exposure and Educational Attainment

	Years of Education				
	(1)	(2)	(3)	(4)	(5)
Radio Exposure (Treat)	0.084	0.066	0.040	0.366***	0.147***
	(0.068)	(0.070)	(0.064)	(0.056)	(0.052)
Male		1.507***	1.507***	1.508***	1.509***
		(0.029)	(0.029)	(0.029)	(0.029)
Rural		−2.810***	−2.816***	−2.788***	−2.784***
		(0.080)	(0.081)	(0.080)	(0.079)
Han Ethnicity		0.123	0.115	0.103	0.107
		(0.077)	(0.079)	(0.078)	(0.077)
Observations	2,700,527	2,700,527	2,700,527	2,700,527	2,700,527
Adj. R-squared	0.344	0.433	0.443	0.448	0.451
County FE	YES	YES	YES	YES	YES
Birth Year FE	YES	YES			
Province × Cohort FE			YES	YES	YES
School Baseline × Cohort				YES	YES
County Linear Trend					YES

Notes: The dependent variable is years of education. "Radio Exposure" is a binary indicator equal to one if radio arrived in the county before the individual turned 12. Standard errors clustered at the county level are reported in parentheses. Province × Cohort FE denotes province-by-birth-year fixed effects. School Expansion × Cohort controls interact baseline primary and junior secondary school density with birth year. County Linear Trend includes county-specific linear birth-year trends.
Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.2 Event Study

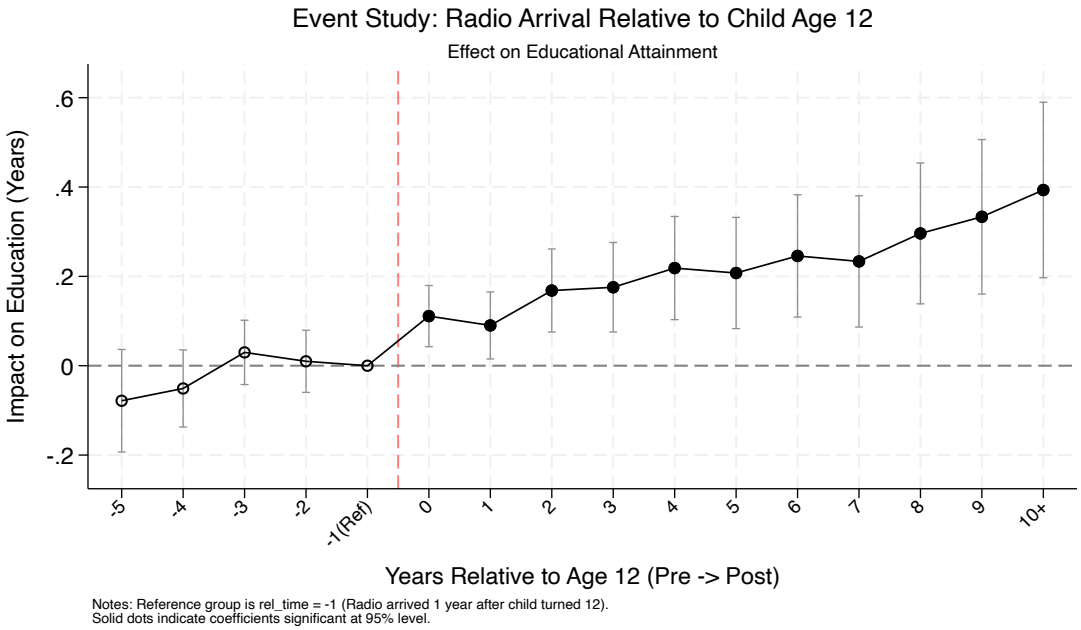


Figure B.5: Event Study: Effect of Broadcast Arrival on Years of Schooling

Notes: This figure plots the coefficients β_k. The outcome variable is years of schooling. The reference group ($k = -1$) is the cohort aged 13 when broadcasting was established in their county. The shaded area represents 95% confidence intervals based on standard errors clustered at the county level.

C Robustness

I report three sets of robustness exercises. First, I examine whether station establishment coincided with the expansion of local school supply. Second, I restrict the analysis to counties with comparable initial educational conditions. Third, Section C.1 reports alternative treatment definitions, age cutoffs, samples that do not require a household-head–child link, and progressive specifications.

School supply A potential concern is that counties expanded broadcasting and schools at the same time. Using school-construction data digitised from local gazetteers, Table C.4 relates station-establishment timing to changes in the number of primary and secondary schools. The unconditional relationship for primary schools is weakly positive, but it disappears after province fixed effects are included. The estimates for secondary-school expansion are also small and statistically insignificant. These results show no detectable within-province relationship between broadcasting timing and the expansion of school supply, although the available school data cover a smaller set of counties.

Table C.4: Falsification Test: Does Radio Timing Predict School Expansion?

	Primary School Expansion			Secondary School Expansion		
	(1)	(2)	(3)	(4)	(5)	(6)
Radio Year	0.146*	−0.034	0.110	0.016	0.021	0.026
	(0.084)	(0.080)	(0.093)	(0.020)	(0.015)	(0.018)
Baseline Primary Enrolment			−9.670**			−0.261
			(4.465)			(1.327)
Baseline Junior Enrolment			−0.629			−0.159
			(6.794)			(2.019)
Observations	368	366	366	368	366	366
R-squared	0.008	0.272	0.295	0.001	0.394	0.394
Province FE		Yes	Yes		Yes	Yes
Baseline Education Controls			Yes			Yes

Notes: The dependent variable is the number of new schools built per 1,000 population (primary in columns 1–3, secondary in columns 4–6). The unit of observation is a county. The weak positive correlation in column 1 disappears entirely after controlling for province fixed effects (column 2), indicating that it is driven by cross-province differences. Across all specifications with province fixed effects, radio adoption year bears no significant relationship with school expansion, ruling out the concern that broadcasting effects operate through concurrent school supply investments. Heteroskedasticity-robust standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Matched counties I also restrict the sample to counties with comparable baseline educational conditions within provinces. The matched and CEM-weighted estimates remain positive and are similar to the preferred rural-sample estimate. This reduces the concern that the result is driven by comparisons between counties with little overlap in their initial characteristics.

Table C.5: Robustness: CEM-DID

	Years of Education		Primary Completion		Junior High Completion	
	(1) Matched	(2) CEM-Weighted	(3) Matched	(4) CEM-Weighted	(5) Matched	(6) CEM-Weighted
Radio Exposure (Treat)	0.381***	0.400***	0.034***	0.038***	0.032***	0.030**
	(0.060)	(0.068)	(0.007)	(0.007)	(0.010)	(0.013)
Observations	2,300,295	2,300,295	2,300,295	2,300,295	2,300,295	2,300,295
R-squared	0.451	0.442	0.505	0.494	0.345	0.333
Control Mean	3.872	3.872	0.516	0.516	0.182	0.182
Matched Counties	700	700	700	700	700	700
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Edu × Cohort	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates from a coarsened exact matching (CEM) difference-in-differences design. Counties are classified as early adopters(radio ≤ 1956) or late adopters (radio > 1956) based on the median adoption year. CEM matches counties exactly on province and coarsely on baseline primary and junior secondary school enrolment rates (quartile bins). Of the original sample, 700 counties (286 control, 414 treated) are successfully matched across 57 strata, with a multivariate L_1 distance of 0.167. Odd columns restrict the sample to matched counties; even columns additionally apply CEM weights. All regressions include county fixed effects, province × birth year fixed effects, baseline education ×cohort controls, and individual controls (gender, rural status, ethnicity). Standard errors clustered at the county level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

C.1 Alternative Treatment Definitions and Data Robustness

This section reports results using alternative definitions of broadcasting exposure, including arrival before ages six through eighteen, excluding counties with ambiguous administrative histories, using alternative station dates where county gazetteers disagree with the *Compendium*, and restricting the analysis to the main 1952–1965 rollout period.

Education Attainment Trajectory

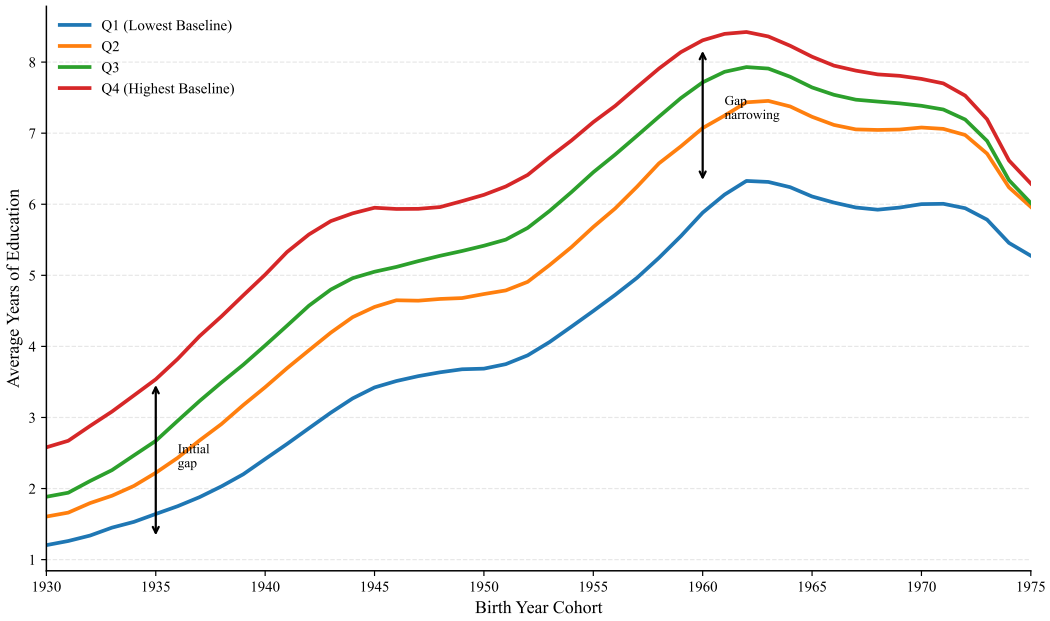


Figure C.6: Average Years of Education by Birth Cohort and County Baseline Education Quartile

Notes: Counties are divided into quartiles based on pre-treatment primary school graduation rates (calculated from definitively untreated cohorts using the rolling window described in Section 4.1). Each line plots the mean years of education for rural individuals born in a given year, smoothed with a 3-year rolling average. The convergence pattern—high-baseline counties growing slower, low-baseline counties catching up—motivates the inclusion of baseline education × cohort trend interactions in the main specification. Without this control, the convergence pattern attenuates the estimated treatment effect because early-adopting counties (which tend to have higher baseline education) exhibit slower educational growth for reasons unrelated to broadcasting. Source: 1% sample of the 1990 Population Census.

Why Age 12?

The baseline specification defines treatment by broadcasting arrival before the child's age 12. To validate this choice, I examine effects across grade levels and across alternative age cutoffs.

Figure C.7 plots the effect of broadcasting on the probability of completing each grade from 1 to 12. Effects are small and statistically insignificant for grades 1–3, modest and marginally significant for grades 4–6 (the primary years), and *peak sharply* at grades 7–9—precisely the junior-high years that begin at age 12. Effects beyond grade 9 attenuate,

consistent with senior high being a relatively rare outcome in rural China during this period.

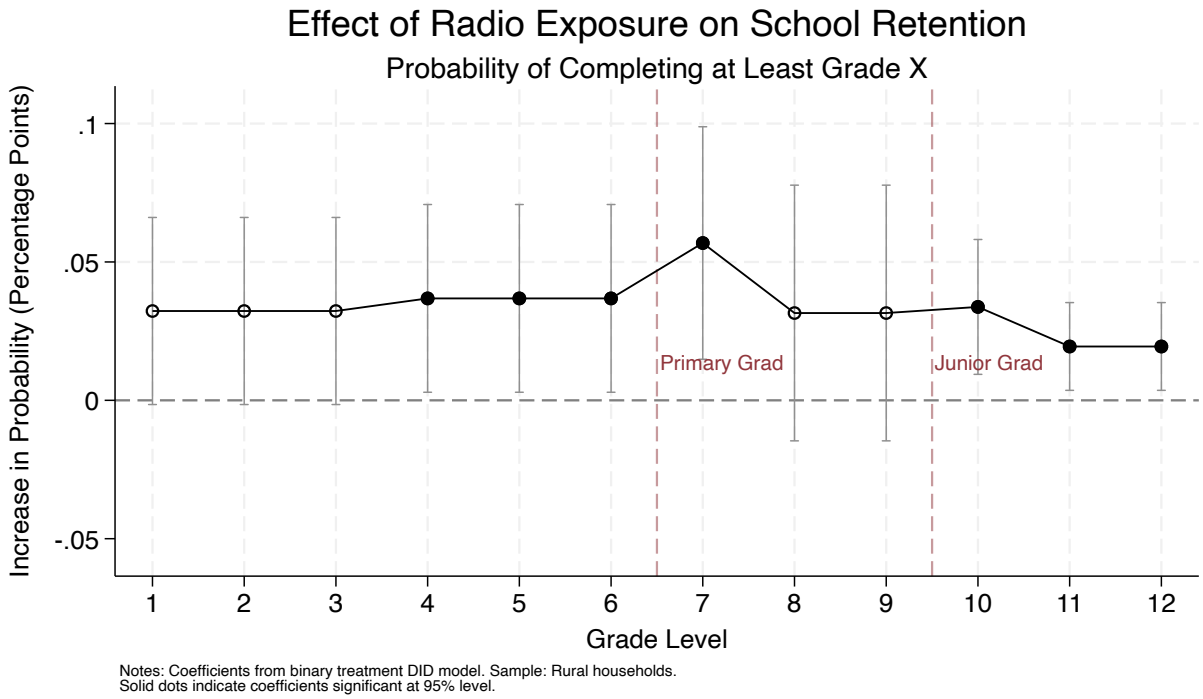


Figure C.7: Effect of Broadcasting on Grade-Level School Retention

Notes: Coefficient estimates and 95% confidence intervals from regressions of an indicator for completing at least grade g on the binary treatment indicator, for $g = 1, \ldots, 12$. Sample: rural households. Specifications include county fixed effects, province $\times$ cohort fixed effects, and baseline education $\times$ cohort trends. Vertical dashed lines mark primary school graduation (Grade 6) and junior high graduation (Grade 9). Standard errors clustered at the county level.

Table C.6 extends this analysis by re-estimating Equation (4.2) using alternative age cutoffs from 6 to 18. The estimated effect is small at age 6 (when no schooling decision has yet been made), rises monotonically through age 12, and declines thereafter. The pattern is consistent with broadcasting acting on the family's decision to continue past primary school, which occurs around age 12. Cutoffs much above or below this age fail to capture the relevant decision margin and yield smaller, less precise estimates.

Table C.6: Robustness: Alternative Age Cutoffs for Defining Treatment

	Age 6 (1)	Age 8 (2)	Age 10 (3)	Age 12 (4)	Age 14 (5)	Age 16 (6)	Age 18 (7)
Radio Before Age X	0.006	0.249**	0.240*	0.397***	0.052	0.183*	0.039
	(0.115)	(0.126)	(0.129)	(0.121)	(0.118)	(0.105)	(0.106)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Edu × Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column re-estimates Equation (4.2) with treatment defined as broadcasting arrival before the specified age. Outcome: years of schooling. Sample: rural households. Standard errors clustered at the county level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Heterogeneity: Han-Majority vs Minority-Majority Counties

Table C.7: Heterogeneity: Han-Majority vs Minority-Majority Counties

	Years of Education		Primary Completion		Junior High Completion	
	(1) Han	(2) Minority	(3) Han	(4) Minority	(5) Han	(6) Minority
Broadcast Exposure before Age 12 (Treat)	0.345***	0.200*	0.048***	0.038**	0.022**	0.013
	(0.054)	(0.111)	(0.007)	(0.018)	(0.009)	(0.015)
Observations	2,225,009	186,619	2,225,009	186,619	2,225,009	186,619
Adj. R-squared	0.411	0.473	0.498	0.520	0.292	0.295
Control Mean	3.286	3.093	0.465	0.444	0.124	0.110
Number of Counties	682	118	682	118	682	118
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
School Baseline × Cohort	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable in columns (1)–(2) is years of education; in columns (3)–(4), an indicator for primary school completion (≥ 6 years); in columns (5)–(6), an indicator for junior high completion (≥ 9 years). The sample is restricted to rural individuals. "Han" denotes counties where the Han population share exceeds 50%; "Minority" denotes counties where the Han share is below 50%. "Radio Exposure" is a binary indicator equal to one if radio arrived in the county before the individual turned 12. All specifications include individual controls (gender, Han ethnicity). Standard errors clustered at the county level. The pattern parallels the linguistic-distance heterogeneity in Figure 5: junior-high effects are concentrated in Han-majority counties, where the Mandarin-medium school system is closer to the household's everyday language.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Distribution of Bootstrap Coefficients

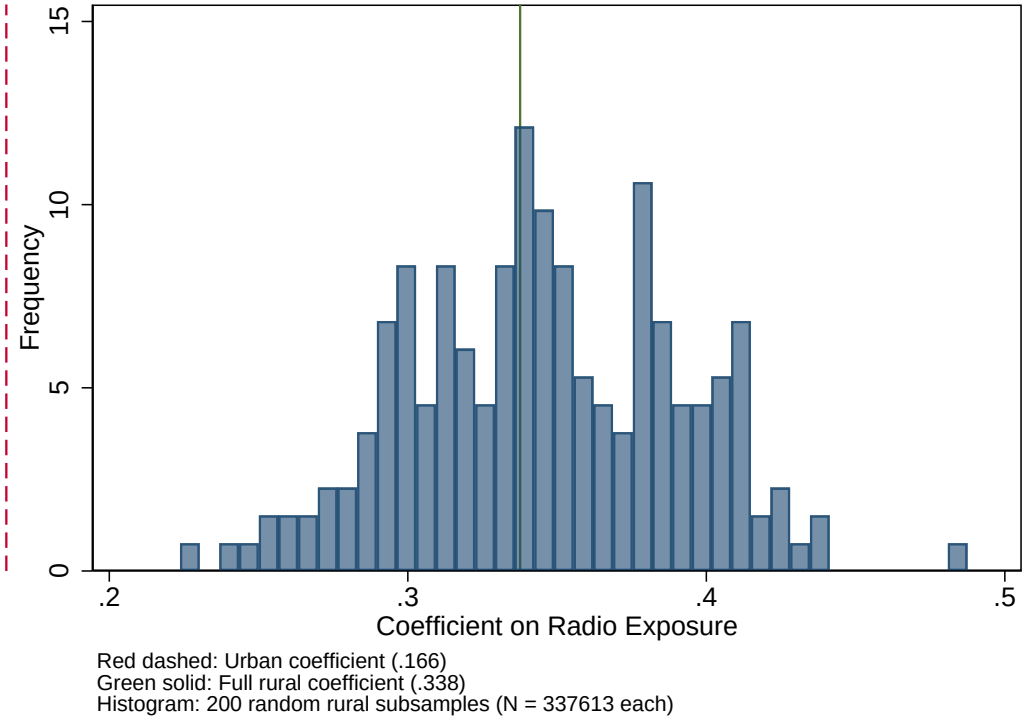


Figure C.8: Distribution of Bootstrap Coefficients on Radio Exposure

Notes: Distribution of estimated coefficients on Radio Exposure across 200 random rural subsamples, each of size $N = 337{,}613$ (matching the urban sample). Red dashed line: urban coefficient from the full 1990 individual sample (0.166); this sample differs from the linked household-head–child sample used in Table 4. Green solid line: the corresponding full-sample rural coefficient (0.338).

D Model Selection: Progressive Specifications

This appendix documents how the estimates change as fixed effects and controls are progressively added. The purpose is not to show that the baseline-education-by-cohort trends are necessary to obtain the result, which would be a poor argument for including them. It is to show what those trends are correcting. Early-adopting counties started from higher educational levels and were on flatter subsequent trajectories, as Figure C.6 illustrates; without the interactions this convergence enters the comparison as differential cohort drift, and the event-study coefficients slope accordingly. The relevant diagnostic is therefore the pre-treatment path, not the post-treatment point estimate. Figure D.9 and Figure D.10 plot event studies for each specification in Table D.8 and Table D.9: the pre-treatment coefficients are flat once the trends are included, and the treatment estimate is stable across the remaining specifications.

Table D.8: Model Selection: Baseline Regressions with County Fixed Effects

	Years of Education				
	(1)	(2)	(3)	(4)	(5)
Radio Exposure (Treat)	−0.062	−0.066	0.115	0.401***	0.408***
	(0.180)	(0.180)	(0.098)	(0.101)	(0.100)
Male		0.982***	0.982***	0.982***	0.983***
		(0.041)	(0.041)	(0.041)	(0.041)
Rural		−2.097***	−2.101***	−2.098***	−2.115***
		(0.070)	(0.070)	(0.070)	(0.070)
Han Ethnicity		0.240**	0.242**	0.242**	0.239**
		(0.098)	(0.097)	(0.097)	(0.095)
Observations	540,560	540,560	540,488	540,488	540,400
Adj. R-squared	0.206	0.264	0.269	0.269	0.274
County FE	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes			
Province × Cohort FE			Yes	Yes	Yes
School Baseline × Cohort				Yes	Yes
Province × Head Cohort FE					Yes

Notes: The dependent variable is years of education. "Radio Exposure" is a binary indicator equal to one if radio arrived in the county before the individual turned 12. Standard errors clustered at the county level are reported in parentheses. Province × Cohort FE denotes province-by-birth-year fixed effects. School Baseline × Cohort controls interact baseline primary and junior secondary school enrolment rates with birth year. The treatment effect is small and insignificant without controlling for differential educational trajectories (columns 1–3), emerges once baseline education × cohort trends are included (column 4), and is stable under the full specification (column 5).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

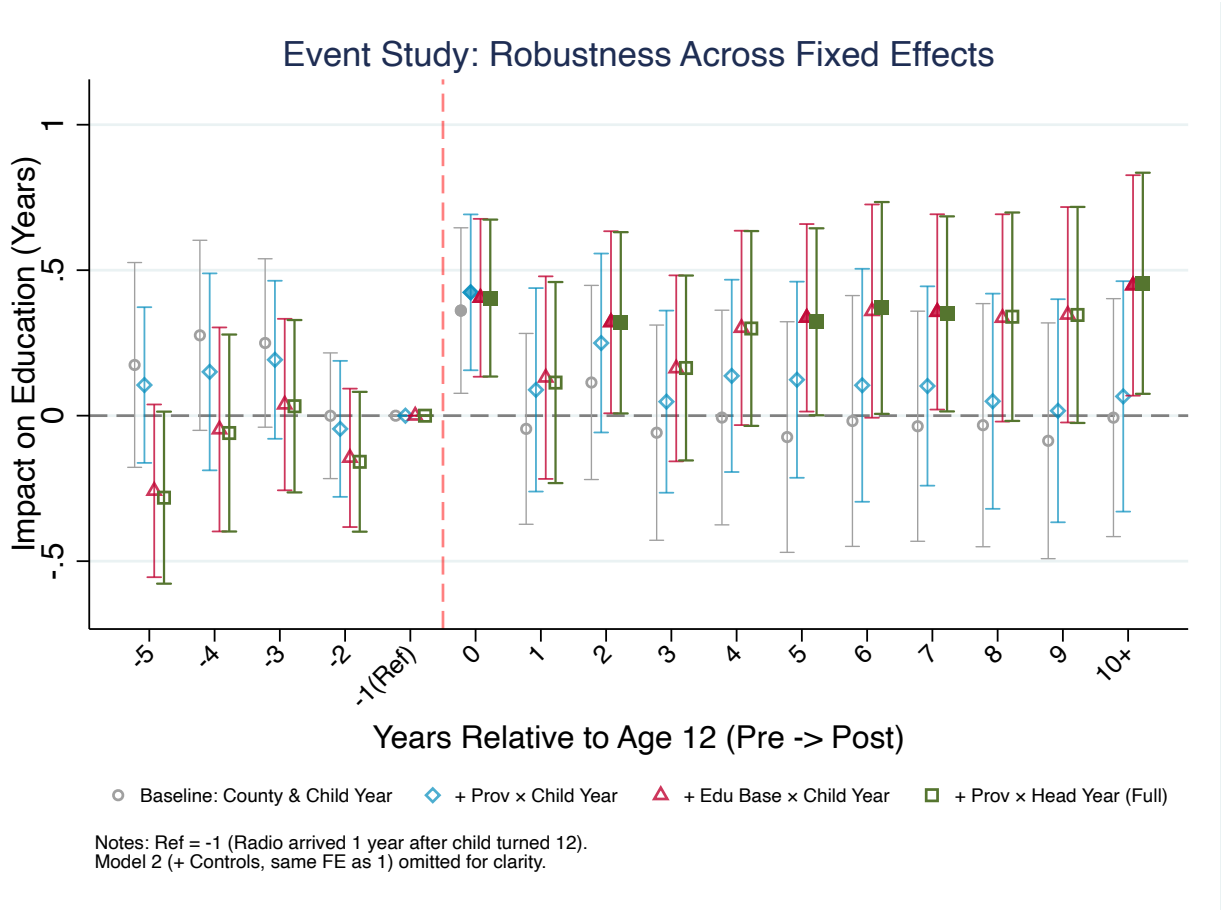


Figure D.9: Event Study: Robustness Across County FE Specifications

Notes: Event-study coefficients from specifications corresponding to Table D.8. Each series adds one layer of fixed effects. Reference cohort ($k = -1$): children who turned 13 one year after radio arrived. Pre-treatment coefficients are small and statistically indistinguishable from zero across all specifications; post-treatment coefficients jump discretely upward once baseline education × cohort trends are included. Model 2 (+ Controls, same FE as 1) omitted for clarity.

Table D.9: Model Selection: Household Fixed Effects with Progressive Specifications

	Years of Education			
	(1)	(2)	(3)	(4)
Radio Exposure	0.301	0.172	0.279*	0.415**
	(0.203)	(0.240)	(0.157)	(0.158)
Male		1.500***	1.497***	1.496***
		(0.047)	(0.048)	(0.048)
Observations	377,248	377,248	377,064	377,064
Adj. R-squared	0.339	0.420	0.423	0.423
Household FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes		
Province × Cohort FE			Yes	Yes
Edu Base × Cohort				Yes

Notes: The sample includes rural households with two or more children. Household fixed effects absorb all household-level controls (rural status, ethnicity, parental education, province × head cohort). Edu Base × Cohort controls for differential educational convergence across counties by interacting county baseline education with child birth year. The treatment effect strengthens from 0.301 (insignificant) to 0.415 ($p < 0.05$) as the specification progressively accounts for differential educational trajectories. Standard errors clustered at the county level are reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

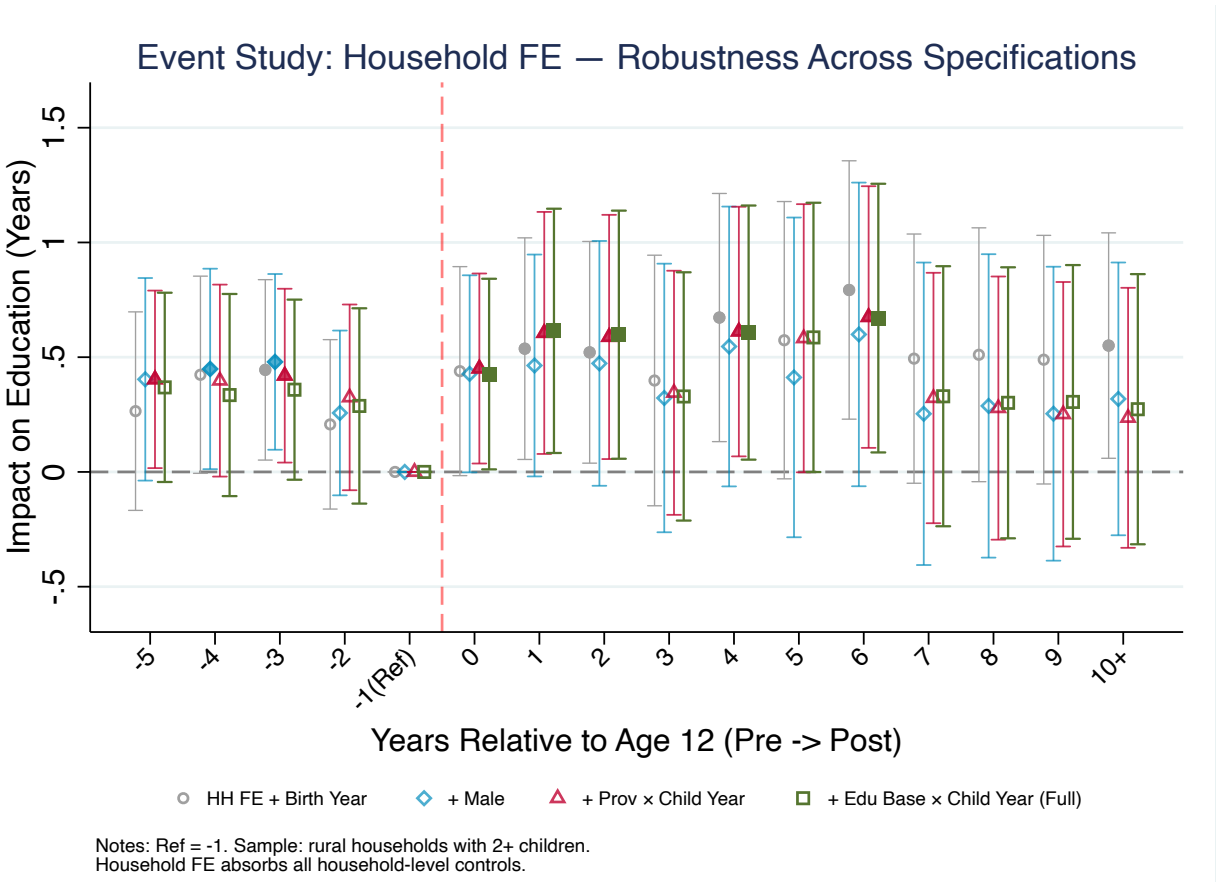


Figure D.10: Event Study: Robustness Across Household FE Specifications

Notes: Event-study coefficients from household-fixed-effects specifications corresponding to Table D.9. Each series adds one layer of fixed effects. Reference cohort ($k = -1$): children who turned 13 one year after radio arrived. Sample: rural households with 2+ children. Household FE absorbs all household-level controls. Pre-treatment coefficients are broadly flat; post-treatment coefficients are stable across specifications.

E Data Structure

This section illustrates the structure of the primary data sources used in the analysis.

Table E.10: Linked Analysis Dataset: Sample Observations

Child					*Household Head*				*County*	
ID	Birth Year	Gender	Yrs Edu	Jr High	Head Birth Yr	Literacy	Edu Yrs	Ethnicity	Radio Year	Province
1	1960	M	9	1	1932	Illiterate	0	Han	1956	Shandong
2	1963	F	6	0	1932	Illiterate	0	Han	1956	Shandong
3	1943	M	6	0	1928	Literate	6	Han	1958	Henan
4	1962	M	9	1	1928	Literate	6	Han	1958	Henan
5	1958	F	3	0	1935	Illiterate	0	Han	1957	Sichuan
6	1965	M	9	1	1935	Illiterate	0	Han	1957	Sichuan

Notes: Illustrative observations from the linked analysis dataset. Each row is a child linked to their household head. Children 3 and 4 illustrate a within-household sibling pair with different treatment statuses: the county station opened in 1958, after child 3 (born 1943) reached age twelve but before child 4 (born 1962) did. The treatment indicator is $\mathbf{1}[\text{Radio Year} \leq \text{Child Birth Year} + 12]$.

Table E.11: Broadcasting Station Records: Sample Entries from the *Compendium of County Conditions*

County	Province	Est. Year	Line Length (km)	Loudspeakers
Jiutai	Jilin	1952	87	156
Xindeng	Zhejiang	1955	142	310
Zouping	Shandong	1956	215	480
Fengyang	Anhui	1957	168	352
Wushan	Sichuan	1958	96	210
Dali	Yunnan	1960	73	145

Notes: Hypothetical illustrative entries from the *Compendium of County Conditions in China* (*Zhongguo Xianqing Daquan*). Each county entry reports the year of first wired broadcasting station establishment, total broadcast line length (pole-kilometres), and number of installed loudspeakers. Data are digitised from the infrastructure history section of approximately 2,000 county entries. Valid first-establishment years are obtained for 1,175 counties.

Table E.12: Surname HHI Construction: Sample Counties

County	Province	Top Surname	Top Share	HHI	Interpretation
County A	Fujian	Chen	0.32	0.142	High concentration (strong clans)
County B	Guangdong	Huang	0.28	0.118	High concentration
County C	Shandong	Wang	0.08	0.021	Low concentration (weak clans)
County D	Henan	Li	0.07	0.018	Low concentration

Notes: Illustrative examples of county-level surname HHI construction from the 2005 Mini-Census. HHI $= \sum_s (n_s/N)^2$, where n_s is the count of surname s in the county and N is total population. Higher HHI indicates greater surname concentration, proxying for stronger lineage/clan networks. Counties in southeastern China (Fujian, Guangdong) tend to have higher HHI due to historically cohesive lineage organisations, while northern plains counties tend to have lower HHI.