Palaces for the People: the long-term effect of public libraries on child development in China

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Abstract

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Keywords*:* Public libraries; Human capital; Child development; Long-term effect; China
JEL Classification: I25; I28; J13; H43; O15

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"At the moment that we persuade a child, any child, to cross that threshold, that magic threshold into a library, we change their lives forever, for the better"

——Barack Obama (1961-)

1. Introduction

Public libraries are a nearly universal feature of modern societies, serving as a cornerstone of educational and cultural infrastructure. With over two million libraries globally, more than 400,000 are public libraries that cater to communities in both developed and developing regions.[1] Notably, children constitute one of the most active and consistent user groups of public libraries. In the United States, about 80% of children under twelve visit a library each year[2], and in England nearly two-thirds of school-age children do so.[3] Youth-oriented library services have also expanded in scope and sophistication: more than half of U.S. public libraries offer early literacy programs, 95% run summer reading activities, and many provide STEM initiatives that support science learning and creativity.[4] Given their indispensable role in promoting early reading and foundational informal learning, public libraries are increasingly recognized by policymakers as a key infrastructure for children's development and equitable educational opportunities.[5]

There is a growing body of empirical evidence that focuses almost exclusively on the short-term impacts of public libraries in developed countries. However, much less is known about the long-term impact of public libraries on human capital outcomes in developing countries. On the one hand, existing evidence has shown that public library usage or investment increases individual reading time, raises the time parents spend reading with young children, and improves children's reading achievement (Bhatt, 2010; Gilpin et al., 2024). Both studies are set in the U.S. context and concentrate on contemporaneous outcomes such as time allocation and test scores. It remains uncertain whether findings from the mature and well-resourced U.S. public library system generalize to developing-country settings where public library access is more limited and uneven. On the other hand, human-capital

[1] International Federation of Library Association and Institutions (IFLA). (2023). *Global Library Statistics 2023.* The Hague: IFLA. Retrieved from https://www.ifla.org/publications/global-library-statistics-ifla-journal/.
[2] WorldMetrics (2025). *Global Library Statistics and Trends.* Retrieved from https://worldmetrics.org/library-statistics/.
[3] Statista (2023). *Frequency of children visiting libraries in England (UK) from 2008–2020, by age group.* Retrieved from https://www.statista.com/statistics/421050/children-visiting-librarys-england-uk-by-age/.
[4] WorldMetrics (2025). *Global Library Statistics and Trends.* Retrieved from https://worldmetrics.org/library-statistics/.
[5] In the following contexts, "library" and "public library" can be used interchangeably.

accumulation is characterized by long lags and cumulative dynamics (Heckman and Mosso, 2014; Lavecchia, 2020), so short-term indicators may understate the enduring developmental contributions of public libraries. In addition, as a public investment, if public libraries crowd out private educational investment, then the beneficial effects of public libraries may not be sustainable over time, and may not necessarily lead to improvements in human capital.

This paper exploits the staggered rollout of public libraries across Chinese counties from 2002 to 2015 to examine the long-term impact of early-life public library exposure on child development. We use a unique episode, the National Cultural Information Resources Sharing Project (NCIRSP) launched by Chinese central government.[6] This large-scale expansion of public libraries provides an ideal empirical setting for at least three reasons. First, in China, modern public libraries expanded from a relatively low initial coverage, making this context representative of broader conditions in many developing countries (e.g., India and Indonesia). Second, the NCIRSP placed explicit policy emphasis on child development, making it particularly well-suited for examining the impact of public libraries on later childhood outcomes.[7] Finally, the NCIRSP adopted a phased "pilot-then-scale" implementation strategy that generated substantial variation in rollout timing across counties: some counties opened public libraries as early as 2002, others not until 2014–2015, and a subset did not open within our observation window. The plausibly exogenous timing of openings yields substantial variation across counties and birth cohorts in children's early-life exposure to public libraries.

This paper overcomes two major challenges related to the lack of literature on the long-term effects of public libraries. First, in low- and middle-income countries, detailed data on the expansion of public libraries is extremely scarce. We hand-collect county-level data on public library establishments from over 3,000 official yearbooks and merge these with

[6] In the early 2000s, China's county-level public libraries were scarce and unevenly distributed. Between 2002 and 2015, the NCIRS established nearly 2,800 standardized county libraries, making it one of the largest public cultural infrastructure programs in the world.

[7] The Chinese government required county libraries to provide child-focused spaces and services, including dedicated children's reading rooms, parent–child reading areas, and age-appropriate collections and activities, thereby creating a direct channel through which public libraries could affect young children.

individual records from the China Family Panel Studies (CFPS), a nationally representative longitudinal survey. Second, due to economic resource constraints, public library expansions rarely generate exogenous variation (Aabø and Audunson, 2012). In our setting, whether a region spontaneously builds a public library is guided by the NCIRSP launched by the central government. Our empirical strategy employs a difference-in-differences (DID) framework to compare children with and without public library exposure in early childhood by virtue of their county and date of birth. We include county fixed effects, cohort fixed effects, province-by-survey-year fixed effects, and a comprehensive set of child, household, and county-level controls. After including these controls, we provide direct evidence that treated and control counties have statistically similar public service capacity at baseline and the timing of public library openings are plausibly exogenous.

Our paper has three main results. First, we find that early-life public library exposure significantly improves cognitive ability, non-cognitive ability, and educational attainment. Each additional year of exposure from conception to age five raises cognitive ability by 0.094 standard deviations, non-cognitive ability by 0.098 standard deviations, and completed schooling by 0.068 years. Event-study estimates reveal no evidence of pre-trends and a gradual build-up of impacts. Second, the magnitude of the estimated effects is comparable to those of large-scale public infrastructure and early-life human capital programs in developing economies (Duflo, 2001; Adukia et al., 2020; Huang and Liu, 2023). Our estimates also substantially exceed the short-run returns documented for similar educational and cultural investments in developed countries (Bhatt, 2010; Gilpin et al., 2024). Third, heterogeneity analysis reveals larger effects for girls, children from lower-income households, and those born in counties with lower baseline educational attainment, suggesting that public libraries reduce developmental inequalities.

Our findings are robust across a comprehensive set of validity checks for identification. First, we address the potential endogeneity of library rollout timing. A primary concern is that counties establishing libraries earlier might have followed different developmental trajectories than later adopters. To mitigate this, we saturate our specification by interacting a

rich set of pre-determined county characteristics with cohort fixed effects, thereby absorbing differential trends correlated with baseline conditions. Conditional on these controls, balancing tests indicate that treated and control counties are comparable in pre-treatment public-service provision. We also provide evidence that these baseline characteristics have limited predictive power for the timing of library openings. Event-study estimates reveal no evidence of divergent pre-trends, indicating that the timing of library construction cannot be predicted by individuals. We further implement a design-based approach following Borusyak and Hull (2023), and confirm that our estimated effects are unlikely to be driven by non-random exposure to rollout shocks. Second, we demonstrate that our results remain robust to confounding factors, including concurrent investments in rural education, healthcare, and other cultural infrastructure. Third, a specification-curve analysis based on 288 model combinations indicates that our main conclusions are robust to alternative fixed-effects, cluster adjustments to standard errors, control sets, variable definitions, and sample restrictions. Fourth, we employ alternative empirical strategies, including propensity-score reweighting, stacked DID, and regression discontinuity designs, all of which obtain consistent estimates. Finally, several heterogeneity-robust estimators (Clarke and Tapia-Schythe, 2021; Sun and Abraham, 2021), permutation tests, and the analyses of potential spillover effects all yield consistent conclusions.

To shed light on the mechanisms behind these long-run effects, we examine three channels: parental investment, the early-childhood home environment, and adolescent learning behavior. First, household production models predict that lower access costs and higher expected returns increase parental investments in children's human capital (Cunha et al., 2010; Del Boca et al., 2014). Consistent with this prediction, early-life public library access increases educational spending, time spent with children, desired years of schooling, and the likelihood of adopting more supportive parenting styles. Second, theories of early human-capital formation emphasize the central role of the home environment for both cognitive and non-cognitive skills (Cunha and Heckman, 2007; Heckman and Mosso, 2014). In line with this, early-life public library exposure significantly raises the probability that

parents tell stories, engage in literacy activities, purchase books, and take children on outings during early childhood, indicating a more stimulating home environment. Third, models of dynamic complementarity and habit formation imply that early gains should translate into stronger learning effort later on (Becker and Murphy, 1988; Cunha and Heckman, 2007). We find that adolescents with greater early-life exposure are more likely to read outside class, read more books, study longer each week, and attain higher academic rank. Together, these results suggest that public libraries shape child development by shifting parental behavior, upgrading the early home environment, and fostering persistent study and reading habits.

We finally conduct a back-of-the-envelope calculation to quantify the economic efficiency implied by our estimates. We find that the total social benefits amount to RMB 74.84 billion, far exceeding the total financing cost of RMB 21.70 billion for county-level public library construction and operation. The implied benefit-cost ratio is about 3.45, with an internal rate of return of roughly 9.29 percent. This benefit-cost ratio can be comparable to high-return early-childhood and school-infrastructure programs documented in developing economies (Akresh et al., 2023), and exceeds most estimates for similar public investments in developed countries (Bhatt, 2010). Our results indicate that public libraries generate large economic and social returns.

This paper contributes to three strands of literature. **First,** it advances research on child development in economics by introducing public libraries as a novel form of early-life public investment. A large literature documents the lasting effects of interventions during critical developmental periods, including disease eradication (Bleakley, 2007; Hjort et al., 2017), health insurance expansions (Cohodes et al., 2016; Huang and Liu, 2023), transportation access (Adukia et al., 2020), nutrition programs (Maluccio et al., 2009; Hoynes et al., 2016), and preschool provision (Havnes and Mogstad, 2011; Cornelissen et al., 2018). However, the existing evidence ignores the potential role of public library.

To the best of our knowledge, the closest study to our paper is Brunello et al. (2017), who show that growing up with more books at home is associated with higher lifetime

earnings in European countries. Our paper differs them in two key respects. First, we study *public* rather than *private* cultural resources. Books at home are private cultural capital, closely tied to family socioeconomic status and highly endogenous. Public libraries instead provide books, spaces, and programs on a non-targeted basis and reach disadvantaged children, and they are therefore a more equitable and scalable cultural infrastructure tool (Aabø and Audunson, 2012; Jaeger et al., 2014). Second, we adopt a multidimensional view of child development. Rather than using adult income as a single proxy, we focus on cognitive skills, non-cognitive skills, and educational attainment by following Cunha and Heckman's (2007) framework. Our paper complements their findings by unlocking the human capital mechanism. Our results also indicate that public libraries complement schools and families in shaping human capital, rather than acting as a substitute for them.[8]

Second, this paper contributes to the growing literature on public library investment and its social returns by providing the first evidence on the long-run human capital effects of modern public libraries in the largest developing country setting. Previous research is exclusively focused on the short-term impacts in the United States. They exploit either historical expansions of Carnegie libraries (Berkes and Nencka, 2024) or contemporary expansions of public libraries (Bhatt, 2010; Gilpin et al., 2024). We differ from them along two main dimensions. On the one hand, we contribute to the Carnegie-library setting by studying the human capital outcomes in a large-scale experiment in different counties, rather than leveraging data from a single library. Carnegie libraries were built in the early twentieth century; their context is far from that of modern public libraries and has limited external validity.[9] Moreover, Berkes and Nencka (2024) study the impact of Carnegie libraries on regional innovation; our paper complements their findings by unlocking the underlying mechanism along the human capital. On the other hand, we go beyond short-run outcomes

[8] Starting from this point, this paper contributes to the broader literature on the relationship between public and private education investment, specifically whether they are complementary or substitutive (e.g., Yuan and Zhang, 2015). The empirical results in Section 6 of this study indicate that public cultural investment and private education investment are complementary.

[9] Carnegie libraries primarily served as book repositories and offered little in the way of child services or structured educational programs. This historical context is far from the role and operation of modern public libraries, and therefore has limited external validity for contemporary policy.

studied for modern U.S. public libraries.[10] Our results complement these prior works by providing credible causal effects and investigate the channels that determine the long-term impact of public libraries on child development.

Third, this paper contributes to the urban economics literature by providing new evidence on how public cultural goods shape human capital and spatial inequality. Prior work on local public goods has focused on tourist attractions, schools, and industrial facilities, showing that urban amenities influence children's long-run skills, earnings and health outcomes (Neilson and Zimmerman, 2014; Rizal et al., 2023; Chen, 2025; He et al., 2025). Yet the role of public libraries in shaping human capital remains understudied. Public libraries differ from traditional amenities in important ways. They provide non-excludable and non-rivalrous access to learning resources (Samuelson, 1954), reaching a broader population than location-specific facilities. For low-income families who often lack books, computers, and quiet study spaces at home, public libraries equalize access to essential learning inputs regardless of economic background. We show that public cultural investment in library generates sizable human capital gains for disadvantaged children, and highlight public libraries act as a cost-effective instrument for inclusive urban development.

The remainder of the paper is organized as follows. Section 2 describes the institutional background of the construction of public libraries. Section 3 introduces the data and variable definitions. Section 4 presents the empirical framework and identification strategy. Section 5 reports the main results, compares our estimates with related studies, and provides robustness and heterogeneity analyses. Section 6 investigates the underlying mechanisms. Section 7 discusses the costs and benefits of public library construction. Section 8 concludes.

2. Institutional Background

2.1 Public libraries in China

[10] Existing work on contemporary U.S. public libraries focuses on short-run behavioral responses, such as reading time, library use, and parental time with children (Bhatt, 2010; Gilpin et al., 2024).

Global Landscape of Modern Public Libraries.—Public libraries are a large-scale and widely adopted form of knowledge infrastructure, providing low-cost access to books and information in a locally accessible setting. According to a report by the International Federation of Library Associations and Institutions (IFLA), there are more than 400,000 public libraries worldwide, serving over 1.1 billion registered users and supported by around 1.6 million full-time-equivalent professional staff.[11] Despite this widespread presence, provision is highly uneven across space. More than 60 percent of the world's libraries are located in OECD member economies, while large parts of Sub-Saharan Africa and South Asia have fewer than one public library per 100,000 inhabitants.[12] In other words, public libraries are globally prevalent, but the intensity of access to library services differs sharply across regions.

The Expansion of Public Libraries in China, 2002-2015. — In early-2000s China, the provision of public libraries was similarly uneven across space, with modern services concentrated in large cities and limited access in many county, township, and rural areas. Large, historic libraries were concentrated in major metropolitan areas such as Beijing, Shanghai, and provincial capitals.[13] County-level facilities remained scarce and poorly equipped (Sturgeon, 2004). Reading rooms within cultural centers were small, outdated, and poorly maintained. In most townships and rural communities, public reading spaces were virtually absent, and textbooks constituted the main reading material for children.[14] The institutional landscape was characterized by a few urban flagship libraries and scattered rural

[11] See the *IFLA Library Map of the World* for cross-country library statistics, in https://librarymap.ifla.org/map/Metric/Number-of-libraries/LibraryType/National-Libraries,Academic-Libraries,Public-Libraries,Community-Libraries,School-Libraries,Other-Libraries/Weight/Totals-by-Country.

[12] UNESCO, World Culture Report 2023: Culture and Sustainable Development (2023).

[13] For instance, the National Library of China was approved for construction on 9 September 1909 and officially opened on 27 August 1912, making it one of the earliest and most prominent public libraries in the country. The Shanghai Library was established in 1952 and remains one of China's largest municipal libraries with over 58 million items in its collection. However, by contrast, in the early 2000s many inland provinces such as Ningxia and Qinghai had fewer than three county-level public libraries, reflecting a marked geographic imbalance. Source: The *China Library Yearbook (2002).*

[14] According to official statistics from the Ministry of Culture and Tourism of China, over 40 percent of county-level public libraries nationwide had been built before 1990, with facilities largely outdated and lacking modern functions such as reading areas, book stacks, and adequate preservation conditions. Source: Ministry of Culture and Tourism of the People's Republic of China, *Analysis of the Development of Public Libraries since the "Tenth Five-Year Plan"*, available at https://zwgk.mct.gov.cn/zfxxgkml/tjxx/202012/t20201204_906399.html.

culture stations, but no standardized county-level network. Recognizing this imbalance, the 2002 *Report on the Work of the Government* called for a transition toward stronger public service provision.[15]

To systematize the supply of public cultural resources, Chinese central government in 2002 launched the *National Cultural Information Resources Sharing Project* (NCIRSP). The program sought to integrate digital networks with library infrastructure to strengthen local cultural services and was backed by substantial fiscal support to accelerate standardization and expansion of grassroots facilities.[16] Implementation followed a phased "pilot-then-scale" approach: each province selected several counties for standardized library construction before nationwide expansion.[17] Figure 1 documents a substantial expansion of China's public-library system after 2002. The national number of public libraries increased from 2,697 in 2002 to 2,820 in 2008 and continued rising to 3,139 by 2015. This institutional growth coincided with broader access and greater collections, with county-level public-library coverage reaching 85.1 percent by 2009 and per-capita public-library holdings rising to 0.61 volumes by 2015, broadly in line with the benchmarks set under the *Twelfth Five-Year Plan*.[18]

[Insert Figure 1 here]

China's public-library push was later formalized in national legislation that set uniform national obligations while leaving core rollout decisions to counties. The *Public Library Law* consolidated long-standing implementation practices into binding national requirements, mandating that library services be incorporated into local development plans and that

[15] The report explicitly stated: "We must further emancipate our minds, completely shake off the fetters of the traditional planned economy, and genuinely shift the functions of government to macroeconomic regulation, market supervision, social management, and public service." (Report on the Work of the Government, delivered by Premier Zhu Rongji at the Fifth Session of the Ninth National People's Congress, March 2002; available at https://www.gov.cn/gongbao/content/2002/content_69957.htm (in Chinese).

[16] For example, the Guangzhou municipal government lists the Guangzhou New Library as a key construction project and reports a total estimated investment of RMB 8.4 billion. Source: https://www.gz.gov.cn/zwgk/fggw/sfbgtwj/content/post_2847486.html (in Chinese).

[17] In Shenzhen, for instance, early implementation concentrated on a limited set of districts: official records indicate that major library construction projects were already underway in Futian District by 2004. Comparable large-scale library facilities in other districts emerged only in later years, with new district libraries opening progressively across the city after the mid-2000s.

[18] See Report of the State Council on Building of Public Culture Service System, in https://www.lawinfochina.com/display.aspx?id=18880&lib=law (in Chinese).

urban–rural library networks be established. At the same time, the law preserved substantial local discretion, explicitly allowing counties to determine the scale, spatial layout, and sequencing of library construction in line with population size, geographic conditions, and fiscal capacity, with central fiscal transfers providing additional support to poorer regions.

In practice, the combination of national mandates and local discretion produced large, spatially patterned heterogeneity in the timing and intensity of library rollout. Figure 2 maps the geographic distribution of county-level library openings and illustrates a clear regional gradient: libraries are denser in eastern China, whereas they are more dispersed in the west, where openings also tend to occur later (darker shading). This pattern is consistent with differences in settlement density, fiscal capacity, and administrative jurisdiction. Counties with larger population centers and stronger administrative resources, often county seats, could complete facilities earlier because a single library served a larger resident base and could be embedded in existing cultural infrastructure; by contrast, more remote and sparsely populated counties faced higher per-capita construction costs and greater logistical constraints and therefore tended to construct later. Importantly, rollout timing also varies substantially within provinces, generating sharp differences in early-life exposure to public libraries across cohorts and counties and motivating the empirical analysis that follows.

[Insert Figure 2 here]

What Do China's Public Libraries Provide?—China's public libraries typically deliver a bundle of services that extends well beyond book lending. One component is digital access, as many libraries provide computer terminals, high-speed internet, online databases, and electronic journals (Shen, 2019). [19] A second component is dedicated learning space: quiet reading areas, individual workstations, and group rooms support both self-study and collaboration, and these facilities are often complemented by exhibition areas showcasing art, photography, and local history.[20] Third, libraries also invest in human-capital–oriented

[19] For example, the Shenzhen Library has developed one of the most advanced digital service systems in China, providing more than 100 public computer terminals, high-speed internet access, online databases, e-books, and electronic journals. Its 49,589 m² main building also includes several 24-hour self-service reading zones established under the "City of Libraries" initiative, reflecting Shenzhen's commitment to expanding digital access for its residents. Source: Shenzhen Library Wikipedia, available at https://en.wikipedia.org/wiki/Shenzhen_Library.

[20] For example, Nanjing Library has long been a center for art and culture display, hosting calligraphy and book exhibitions

programming, offering adult education in computer literacy and language learning, along with job-search assistance and entrepreneurship-related training (Xie and Jaeger, 2008). [21] In addition, they host cultural and community events such as author talks, film screenings, and workshops[22], which can make the library a focal meeting point for local residents (Audunson et al., 2020). Finally, reference and user-support services help visitors locate information and, in some settings, complete administrative tasks and navigate local regulations. Taken together, these features position the modern public library in China as a multi-service regional hub with the potential to influence cultural literacy and well-being.

In addition to these general services, China's public library system is also designed to support children and family reading. National guidelines and reading-promotion initiatives call for strengthening children's services[23] and, in practice, library service standards[24] and evaluation criteria[25] typically require (and score) the provision of children's collections, designated children's spaces, and regular child- and family-oriented activities. Consistent with this institutional design, local implementation commonly takes three forms. First, libraries offer resources targeted at caregivers, including parenting and family-education

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that attract thousands of visitors and integrate learning with aesthetic experience. Source: https://epaper.gmw.cn/gmrb/html/2014-10/28/nw.D110000gmrb_20141028_7-09.htm (in Chinese).

[21] For example, Haining Library, a county-level public library in Zhejiang Province, began offering adult digital literacy and job-skills programs in the mid-2000s. In 2006, the library opened a digital reading room with 31 public computers and also started providing free access to online databases such as CNKI from October 15, 2007. Between 2008 and 2015, it expanded those services to include Wi-Fi access, self-service borrowing, and mobile-library "farm reading rooms" across the county, significantly improving adults' ability to access digital information and training resources. (Source: Haining Library Wikipedia, available at https://en.wikipedia.org/wiki/Haining_Library.)

[22] For example, a nationwide reading boom has transformed Chinese libraries into dynamic urban "third spaces" that merge learning, leisure, and community culture. Source: Xinhua News Agency, *Reading Boom Transformed Library into Urban "Third Space"* (2025), available at https://english.news.cn/20250423/18b6b62ce20341d1832a7e735f9dd7de/c.html.

[23] The *National Reading Plan* explicitly calls for promoting family and parent-child reading to create favorable environments for children. The *National Reading Development Plan for the 13th Five-Year Period* states: "Vigorously advocate family reading and parent-child reading, leverage the important role of parental role modeling and guardianship, and promote the whole society to jointly create and maintain a favorable reading environment for children." See https://www.xinhuanet.com//politics/2016-12/27/c_129421928.htm (in Chinese).

[24] *Public Library Service Specifications* (GB/T 28220-2011) stipulates that public library services target all populations and should focus on cultivating children's reading habits (Article 4.3). Children's reading areas must be separated from adult areas with independent entrances where feasible, and outdoor activity spaces are recommended (Article 5.1.2). Independent children's libraries should open at least 40 hours per week (Article 6.1.1). See https://www.gdwc.gov.cn/uploadfiles/file/20170222/20170222105711_1416.pdf (in Chinese). *Public Library Service Specifications for Children* (GB/T 36720-2018) provides further detailed regulations on early childhood services. See https://std.samr.gov.cn/gb/search/gbDetailed?id=7643B2F25023267CE05397BE0A0AAF6A (in Chinese).

[25] The Ministry of Culture and Tourism's *Evaluation Standards for County-Level Public Libraries* (2013, 2017, 2023 editions) consistently include children's reading room facilities, picture book collections, and activity frequency as key assessment indicators. In Appendix Figure A.3, we present the spatial distribution of public library ratings by county.

materials and, in many locations, caregiver-facing lectures or workshops.[26] Second, they provide resources for children, most visibly through dedicated children's collections, especially picture books and other age-appropriate materials, often arranged in child-friendly sections. We present an illustrative example in Appendix Figure A.1, which shows a picture-book area designed for ages 0–2 with low, open shelving that facilitates easy browsing and early engagement. Third, libraries provide services for parent–child families by combining purpose-built shared-reading spaces with structured activities that encourage joint participation. One example is a parent–child reading area shown in Appendix Figure A.2, which features integrated adult–child seating designed to support shared reading and interaction.

2.2 Public libraries and long-term child development

How Do China's Public Libraries Potentially Influence Long-term Child Development? — In theory, early-life exposure to public libraries can have mixed effects on child development. On the one hand, improved access to libraries may promote both cognitive and socio-emotional skills. Children's developmental trajectories are shaped in part by intrinsic needs for exploration, knowledge acquisition, and social interaction. Ecological systems theory emphasizes that development emerges from continuous interactions between the child and the surrounding environment (Bronfenbrenner and Morris, 2006). As open-access and non-compulsory learning institutions, public libraries offer safe and inclusive settings that can accommodate these needs for cognitive exploration as well as social engagement (Oldenburg, 1999; Aabø and Audunson, 2012). Related work in educational psychology further suggests that learning contexts characterized by autonomy, accessibility, and low pressure are conducive to intrinsic motivation, which in turn supports sustained learning and skill accumulation (Deci and Ryan, 2000).

Several mechanisms may generate these positive effects. First, the presence of a nearby public library may reshape parental investments and parenting practices. In a household

[26] For example, Jiashan County Library, Zhejiang province, organized a family-oriented parenting lecture themed around positive emotion-focused parenting, with 40+ participants reported in the event summary. See https://ct.zj.gov.cn/art/2024/4/19/art_1652992_59020444.html (in Chinese).

production framework, child human capital is produced by combining parental time and goods inputs with external resources (Del Boca et al., 2014). By reducing the cost of educational materials and raising the perceived returns to learning-oriented time, libraries may strengthen parents' incentives to engage in shared reading, homework assistance, and guided study. More broadly, theories of the home learning environment highlight parent–child interaction quality as a central determinant of both cognitive and socio-emotional development (Bradley et al., 1979; Bornstein, 2005). Library services that provide parenting resources, caregiver lectures, and parent–child reading programs may therefore operate as a low-cost channel for transmitting effective parenting knowledge and updating beliefs about early investment.

Second, libraries may improve children's early learning environments by expanding access to language-rich and developmentally appropriate materials. Models of dynamic complementarity and sensitive periods predict that early inputs have persistent effects because they raise the productivity of later investments (Heckman, 2006; Cunha et al., 2010). To the extent that libraries increase exposure to books, structured learning routines, and cognitively stimulating activities, they can build early skills that support subsequent learning and academic achievement.

Third, early exposure to libraries may contribute to the formation of durable learning habits. Habit formation theories predict that repeated engagement reduces the future psychological cost of an activity and generates persistence over time (Becker and Murphy, 1988). Regular library visits and recurring programs may thus strengthen self-regulation and intrinsic motivation, transforming early interest in reading into sustained skill accumulation.

On the other hand, early-life exposure to public libraries may have limited effects, or even negative effects, when take-up is low or services are of insufficient quality. If collections and programming are poorly aligned with families' needs and cultural capital, usage and learning gains may remain modest (Bourdieu, 1986). Moreover, if households treat library services as substitutes rather than complements to private educational investments, parents may reduce their own time or monetary inputs. In such cases, public provision may

crowd out private investment, lowering total educational inputs and attenuating potential benefits (Yuan and Zhang, 2015). In summary, the magnitude and direction of the library effect depend not only on the effectiveness and quality of library services, but also on how families respond to public provision and integrate it into their broader investment strategies in children.

In our empirical setting, we hypothesize that early exposure to public libraries has a positive effect on children's long-term development for several reasons. First, both national policy and local implementation explicitly prioritize the role of public libraries in serving children (see Section 2.1). In practice, many libraries offer dedicated children's spaces, age-appropriate collections, and organized educational activities, increasing the likelihood that early exposure translates into substantive engagement rather than passive access. Second, high construction quality and strong operational efficiency in China's public library system reduce concerns about crowd-out or ineffective utilization. The national evaluation and rating system explicitly incorporates collection quality, service provision, and user satisfaction, including feedback from children and caregivers, creating institutional incentives for libraries to maintain well-curated, user-oriented services.[27] Consequently, public library provision is more likely to complement, rather than substitute for, household educational investments. Consistent with this interpretation, children account for a substantial share of library users[28], indicating that resources targeted at younger populations are actively used rather than underutilized. In the following sections, we empirically test this hypothesized positive relationship.

3. Data and variables

3.1 Data source

[27] China has established a national *Public Library Assessment Indicators* system under the cultural industry standards framework. The evaluation covers multiple dimensions, including collection quality, cataloging standards, service provision, and reader satisfaction, which together form the basis for library assessment and rating.

[28] For example, in Shanghai, public libraries reported approximately 304,000 active child readers in 2024. Children aged 5–14 accounted for about 90 percent of these users. Total circulation of children's books reached 15 million volumes, with an average of 8.73 books borrowed per child, representing a 25 percent increase relative to the previous year. Source: https://www.chinadaily.com.cn/a/202504/22/WS6807418ea3104d9fd3820d82.html.

This section describes the main data sources used in this study. We construct a unique county-level dataset on public library establishment by manually collecting information from prefecture-level and county-level statistical yearbooks. We then match this county-level information with individual-level survey data from the China Family Panel Studies (CFPS).

3.1.1 Macro-level data

County-level data on public library expansion are obtained from prefectural and county statistical yearbooks. These yearbooks are official statistical compilations that provide comprehensive and verified information on local economic, demographic, and infrastructure indicators. They are widely used in empirical research and are recognized as reliable sources in the literature (Almond et al., 2019; Li et al., 2025; Yu et al., 2025).

For each county, we identify the first year when a public library was established between 2002 and 2015, and the timing of library expansion varies considerably across counties. Figure 3 shows that expansion occurred in a staggered pattern rather than through a simultaneous nationwide rollout. The number of counties establishing libraries rose steadily during the mid-2000s and peaked around 2011–2012. Thereafter, the pace of new establishments declined as most regions completed their planned construction targets. This phased rollout generates meaningful temporal and geographic variation for the empirical analysis.

Additional county-level characteristics come from the *2000 Population Census*, including total population, age structure, sex ratio, average years of education, and share of non-agricultural population. The census timing aligns well with our identification strategy as it predates the policy rollout and captures pre-treatment county conditions. Number of hospital and clinical beds and terrain characteristics are drawn from the *2001 China County Statistical Yearbook*.

[Insert Figure 3 here]

3.1.2 Micro-level Data

The individual-level analysis relies on data from the *China Family Panel Studies* (CFPS). CFPS is a nationally representative longitudinal survey, often regarded as the Chinese counterpart of the U.S. *Panel Study of Income Dynamics* (PSID). The survey was launched in 2010 by the Institute of Social Science Survey at Peking University. It adopts a three-stage, probability-proportional-to-size (PPS) sampling design. The baseline survey covers 25 provincial administrative units across 31 provinces, representing about 95 percent of China's total population. In the first wave, CFPS completed interviews with 33,600 individuals from 14,798 households. Follow-up surveys have been conducted every two years. We use all six waves (2010, 2012, 2014, 2016, 2018, 2020) to capture the full range of outcome variables.

CFPS offers several advantages for this study. First, it reports each respondent's birth year and birth county, which allows precise matching with the timing of county-level library establishment. Second, it contains detailed demographic characteristics, such as gender, household registration, and family size. Third, the survey measures both educational attainment and multiple dimensions of child development[29], a combination rarely attainable in other Chinese datasets. It records completed years of schooling for all respondents and contains direct cognitive test scores, depression assessments, and a range of noncognitive scales for individuals required to complete the self-administered questionnaire, including children aged 10–15 and all adults.

3.2 Construction of main variables

3.2.1 Explanatory variable: library exposure

Our explanatory variable is the years of public library access during early childhood, defined as from conception through age five. This window is widely viewed as a critical period for human capital formation (Currie, 2011). We construct the measure by matching county library establishment years with individual birth date and inferred conception years. Because statistical yearbooks do not systematically report the month of library opening, we assume

[29] Variable availability differs across survey waves. We summarize the availability of all outcome variables in Appendix Table B.1.

each library opened in January of the recorded year.[30] For each child, exposure equals the number of years between conception and age 5 during which a public library is available in the child's birth county, capped between 0 and 6.

3.2.2 Child development outcomes

We focus on four outcome variables: cognitive ability, non-cognitive ability, depression, and educational attainment. [31] These dimensions correspond to the key domains emphasized in the skill-formation literature, which highlights the role of early inputs in shaping human capital accumulation across cognitive and soft skills (Cunha and Heckman, 2007; Cunha et al., 2010). Following established measurement approaches (Huang and Liu, 2023; Li et al., 2023), we construct four sets of outcome variables using available information in the CFPS.

Cognitive ability. CFPS includes two cognitive test batteries. The first set, administered in 2010, 2014, and 2018, consists of verbal and math tests designed to assess general language and quantitative reasoning skills. We standardize each score using the mean and standard deviation of the corresponding sample and denote them as the verbal and math test scores. The second set appears in 2012 and 2016 and includes word-recall and number-sequence tasks similar to those in the U.S. *Health and Retirement Study* (HRS). For the word-recall task, we average immediate and delayed recall scores, then standardize the mean to construct the word-recall score. The number-sequence test uses an adaptive two-stage design under an item-response-theory model; we standardize the resulting score within the sample. To capture overall cognitive performance, we aggregate standardized scores within each age group following the index approach of Huang and Liu (2023). A higher value represents stronger cognitive ability.

Non-cognitive ability. Following Li et al. (2023), we measure non-cognitive ability using the CFPS self-esteem scale, where questionnaire items capture personality traits

[30] We test the robustness of this timing assumption using alternative measures of library exposure, including IU-4, IU-5 (mid-year implementation), IU-5 (year-end implementation), and IU-5 (binary), as reported in Appendix Table C.9.
[31] We summarize the definitions and components of all outcome variables in Appendix Section B.

typically regarded as non-cognitive skills. Prior research demonstrates that non-cognitive ability constitutes a distinct and independently developed form of human capital (Cunha and Heckman, 2008), which in turn influences long-term economic and social outcomes (Heckman and Rubinstein, 2001). Consistent with Li et al. (2023), we construct a non-cognitive skills index from these items[32], recoding responses such that higher scores correspond to more favorable traits. The resulting index is standardized (z-scored) using the sample mean and standard deviation to ensure comparability across variables, with higher values reflecting greater self-efficacy.

Depression. We assess mental health using the 6-item Kessler Psychological Distress Scale (K6) and the Center for Epidemiologic Studies Depression Scale (CES-D) in 20-item and 8-item versions.[33] Both instruments screen for non-specific psychological distress in general populations and have been validated across multiple countries including China (Kessler et al., 2002). Following Li et al. (2023), we define depressive symptoms using standard cutoffs: for the K6, items are scored from 0 to 4 and respondents with a summed score $\geqslant$ 13 are classified as distressed; for the CES-D-20, items are scored from 0 to 3 (total range 0–60) and scores $\geqslant$ 16 indicate elevated risk of clinical depression; for the CES-D-8, we use a cutoff of $\geqslant$ 7.

Educational attainment. Educational achievement forms an integral part of child development and human capital accumulation (Card, 1999; Hanushek and Woessmann, 2012). We measure it by completed years of schooling, which capture both access to and persistence in formal education.

3.3 Sample and summary statistics

We restrict our analysis to individuals aged 10–19 in order to capture the critical stage of child development. This restriction is motivated by both data availability and theory: the CFPS reports key outcomes only for individuals aged ten and above, and the literature identifies ages 10–19 as a formative period for cognitive, social, and non-cognitive skill

[32] We show the full list of self-esteem items and their coding in Appendix Table B.2.
[33] We show the specific questionnaire items utilized in Appendix Table B.3.

formation (Jetha and Segalowitz, 2012; Bonnie et al., 2019). Accordingly, our sample consists of individuals born between 1991 and 2010 who are observed between ages 10 and 19. We show that the results are robust to alternative age cutoffs in Appendix Table C.8 (Panels A–D).

To address potential migration-related bias in the measurement of early-life library exposure, we retain only respondents who were (i) born in the same county where they currently reside, (ii) lived in that county at age three, and (iii) hold household registration (*hukou*) in that county at the time of the survey.[34] This restriction limits migration bias and ensures that subjects were most likely to have been exposed to newly established county libraries during early childhood. The CFPS data are merged with the county-level rollout dataset compiled from prefecture and county statistical yearbooks. The final sample consists of 19,162 adolescent observations representing 8,764 unique individuals aged 10–19.

Table 1 reports the summary statistics. The mean early-childhood library exposure (*Library share IU-5*) is 0.21 with a standard deviation of 0.94, showing wide variation across counties and birth cohorts. The average years of schooling equal 7.0. Standardized measures of cognitive and non-cognitive ability have means close to zero and unit variance by construction. The average depression incidence is 0.18. About 52 percent of the sample are male and 83 percent hold agricultural *hukou*.

[Insert Table 1 here]

4. Empirical strategy

4.1 Identification and estimation

We employ a difference-in-differences (DID) model to estimate the impact of early library exposure on child development. The strategy compares child development outcomes for those

[34] In Appendix Table C.8, Panel E, we relax the locality restriction and re-include children who migrated across counties between birth and survey years. The estimates remain stable, confirming that the main results are not driven by selective migration.

with early childhood exposure to public library in their county of birth to those born earlier (and therefore without childhood library exposure). Formally, we estimate the following model:

$$Y_{icbt} = \alpha + \beta \text{Exposure}_{cb} + \gamma X_{icbt} + \eta_b \text{CB}_c + \delta_c + \lambda_b + \tau_{pt} + \varepsilon_{icbt} \quad (1)$$

where Y_{icbt} denotes the outcome of individual *i* born in county *c*, cohort *b*, and observed in survey year *t*. It includes four measures of child development: cognitive ability, non-cognitive ability, depression, and educational attainment. Exposure_{cb} measures the total years of public library access from conception to age five in the child's birth county. X_{icbt} is a vector of individual and household characteristics, including gender, *hukou* status, parents' age, parental education, household size, and household income.

In our analysis sample, counties initiated public libraries at different times: some were early adopters, others adopted later, and a subset did not establish a public library during our study period. This staggered rollout allows us to compare children with or without library access in early-life by virtue of their county and date of birth. Therefore, we can allow for unrestricted county fixed effects δ_c, unrestricted birth-cohort fixed effects λ_b, and unrestricted province-by-survey-wave fixed effects τ_{pt}. We further include interactions between key determinants of treatment status (discussed in detail in Section 4.2) and cohort fixed effects $\eta_b \text{CB}_c$ to account for potential time-varying selection into library establishment at the county level. ε_{icbt} is the error term, and standard errors are clustered at the county level.

The parameter of interest is β, the effect of an additional year of early childhood library exposure, which is identified from variation within counties across birth cohorts. It estimates the average difference in outcomes between individuals with high and low exposure to county libraries from conception through age 5, based on the timing of public library rollout in their birth county. Since we do not directly observe individual use of public

libraries, we interpret β as an intent-to-treat (ITT) effect, which is likely smaller than a treatment effect on the treated.

4.2 Assessing the validity of the identification strategy

The validity of our empirical design depends on the exogeneity of public library rollout timing. As discussed in Section 2, the Chinese central government implemented the county-level public library program through a phased rollout, while granting local governments discretion to determine the exact timing of implementation based on local conditions. A potential concern is that library establishment may not be random: counties that adopted earlier may differ systematically from those that adopted later or not at all, and such pre-existing differences could account for the observed post-adoption outcomes. We address this in the following five ways.

First, we examine the determinants of the timing of public library introduction. *Public Library Law of the People's Republic of China* requires local governments to consider population, education, economic capacity, geography, and public services when planning county libraries.[35] Guided by these criteria, we compile a set of county characteristics measured in 2000, on the eve of the initial wave of county public library establishment, and test whether these pre-determined characteristics predict the timing of library adoption.

Specifically, we consider nine county-level characteristics: GDP[36], total population, sex ratio, the shares of population aged 0–14 and 65 and above, average years of education, urbanization rate (measured by the share of non-agricultural population in the county), an indicator for mountainous counties, and the number of hospital beds per 1,000 residents. Together, these variables capture pre-existing variation in economic development, demographic structure, geographic accessibility, and public service capacity. Following the approach of Hoynes and Schanzenbach (2009), we estimate regressions in which the

[35] As detailed in Section 2, Article 13 of the *Public Library Law of the People's Republic of China* explicitly stipulates these planning criteria.

[36] Due to limited availability of county-level GDP data, we use nighttime light intensity as a proxy for local economic activity. Nighttime lights provide broader spatial coverage and capture variation in economic development across counties more comprehensively than reported GDP figures (Henderson et al., 2012; Gibson et al., 2021).

dependent variable is the year of a county's public library establishment and the independent variables include this full set of baseline characteristics.

The results, reported in Appendix Table A.1, present specifications without (column (1)) and with province fixed effects (column (2)). In column (2), we find that these baseline characteristics can predict the timing of public library establishment. We then test the joint significance of these baseline characteristics. The P-value is 0.267 and therefore fails to reject the hypothesis of no correlation between the timing and baseline county characteristics. Overall, these results imply that the timing of public library construction is unlikely to have been driven by local conditions, supporting the interpretation that exposure to public libraries during early childhood is plausibly exogenous. Nonetheless, in order to control for possible differences in cohorts across counties that are spuriously correlated with the county treatment status, we include interactions of these 2000 pre-treatment county characteristics with cohort fixed effects $\eta_b \mathrm{CB}_c$ in the main specifications.

Second, we assess whether treatment and control counties are comparable after conditioning on the aforementioned baseline variables by conducting a balancing test. Following Li et al. (2025), we first compare treatment and control counties along the nine baseline characteristics and then examine differences in broader public service provision after controlling for these characteristics. Table 2 reports the results of these balancing checks. In Panel A, we present differences in the baseline variables and reveals sizable differences between treatment and control counties. For instance, counties that established public libraries tend to have higher levels of economic development, larger populations, higher average educational attainment, and are less likely to be located in mountainous regions.

In Panel B, we compare treatment and control counties across a range of public service provision measures in the initial year, including ten indicators, including education, healthcare, social welfare, and communication infrastructure. Column (3) shows that treatment counties exhibit significantly better public service provision along many dimensions. However, as shown in Column (4), once the nine baseline variables are controlled, none of these differences remain statistically or economically significant. Figure 4

visualizes the estimates from Column (4); all confidence intervals include zero, confirming that treatment and control counties are well balanced after conditioning on the baseline characteristics.

As an additional robustness check to alleviate remaining concerns about comparability, we re-estimate our baseline specifications using only counties that eventually established public libraries, thereby excluding never-treated counties from the sample. In this setting, we only use the difference in timing within the treatment counties for identification. The results, reported in Appendix Table C.5, are quantitatively similar to our main findings, further supporting the validity of the comparison group and our baseline identification strategy.

[Insert Table 2 and Figure 4 here]

Third, a remaining concern is that unobserved county characteristics may still be correlated with treatment timing, leading to omitted-variable bias. Following Borusyak and Hull (2023), we address this concern by exploiting the fact that such bias in staggered adoption designs can be mitigated by either (i) recentering treatment (differencing realized treatment from expected treatment) or (ii) controlling for the expectation of treatment. As discussed in Section 5, our estimates are stable across these alternative specifications, reinforcing the interpretation that the results are not driven by residual selection on unobservables.

Fourth, the key identifying assumption underlying our difference-in-differences framework is that, absent library construction, outcomes in treated and control counties would have followed parallel trends. To assess this assumption, we first estimate event-study specifications that trace outcome dynamics. The estimate post-policy coefficients rise steadily, consistent with gradual improvements in human capital. However, traditional event-study analysis may suffer from low power and biased estimates (Roth, 2022). Following Rambachan and Roth (2023), we construct robust confidence intervals under alternative restrictions on the parallel-trends assumption. As shown in Appendix Figure C.5, the estimates remain stable across specifications. We also conduct placebo and permutation tests

that reshuffle policy years. The results in Appendix Figures C.6 and C.7 confirm that the effects are not driven by spurious timing correlations.

Fifth, another potential concern is that the estimated effects may be confounded by other contemporaneous policies or public investments. If the rollout of county-level public libraries coincided with the expansion of other county- or community-level programs affecting child outcomes, it would be difficult to isolate the causal effect of library construction from these co-occurring changes. We therefore control for major county-level concurrent developments in education, health, cultural services, and basic infrastructure, and we also account for concurrent changes in community-level public services.[37] The estimated effects remain stable after including these county- and community-level controls.

5. Main results

5.1 Baseline results

Table 3 reports the baseline estimates obtained from Equation (1). All regressions include individual and household controls, county controls interacted with cohort fixed effects, as well as county, cohort, and province-by-survey fixed effects. We find that early-childhood exposure to public libraries has large, positive effects on several aspects of child development. Specifically, in Columns (1), (2), and (4), each additional year of public library exposure within the conception-to-age-five window raises cognitive ability by 0.094 standard deviations, non-cognitive ability by 0.098 standard deviations, and completed years of schooling by 0.068, all significant at the 1 percent level. Compared with the sample means reported in Table 1, these estimated coefficients imply improvements of approximately 9.5 percent, 10 percent, and 1 percent in the corresponding outcomes. In contrast, Column (3)

[37] At the county level, we consider major education sector reforms (such as the *Rural Education Informatization Reform*, the *Kindergarten Expansion Reform*, the *School Merging Policy*, and the *Two Exemptions and One Subsidy Program*), health and social insurance expansions (such as the *New Rural Cooperative Medical Scheme*), and cultural and sports initiatives (such as the *National Fitness Plan* and public cultural facility programs, including the rollout of *theaters and cinemas*). At the community level, we control for improvements in basic public services and infrastructure, including access to piped water, electricity, cable television, fixed telephones, mobile signal coverage, public roads, and piped gas.

shows a small and statistically insignificant coefficient (-0.002) for depression, suggesting that early library access primarily affects learning and socio-emotional development rather than mental-health outcomes.

[Insert Table 3 here]

To explore the dynamic effects of library exposure, we estimate an event study model following Hoynes et al. (2016) and Huang and Liu (2023). The specification is:

$$Y_{icbt} = \alpha + \sum_{k=-1}^{K} \beta_k D_{cb}^{k} + \gamma X_{icbt} + \eta_b CB_c + \delta_c + \lambda_b + \tau_{pt} + \varepsilon_{icbt} \quad (2)$$

where D_{cb}^{k} is a binary indicator equal to one if child *i*, born in cohort *b* and residing in county *c*, was first exposed to a public library at age *k*, and zero otherwise. It consists of a series of dummy variables indicating whether the child was first exposed to a public library within each two-year age interval, such as ages 0–1, 2–3, 4–5, and so on. We normalize the coefficients by omitting the indicator for first exposure at ages six to seven. Children in this group were already aged six to seven when a public library was established in their county and therefore experienced no exposure during early childhood; they serve as the reference group. Children first exposed at ages zero to five (i.e., the 0–1, 2–3, and 4–5 groups) are classified as partially exposed, while those born at least one year after library establishment (i.e., the age $\leqslant -1$ groups) are fully exposed from conception. In this specification, the coefficients β_k capture how the effect of public library exposure varies with the age at initial exposure.

Figure 5 reports event-study estimates by the child's age at first exposure to a public library. The horizontal axis is ordered from older to younger ages at first exposure. Children in the age ≤ -1 bin lived in counties where a public library was already available before conception and were therefore exposed throughout the entire conception-to-age-five window. Children first exposed at ages 0–1, 2–3, and 4–5 experienced progressively fewer years of exposure during early childhood. The omitted group consists of children first exposed at ages

6–7, who had no exposure within the conception-to-age-five window. The estimates exhibit a clear and systematic pattern across outcomes. In Panels A, B, and D, children who were first exposed to a public library between conception and age five experience sizable and statistically significant improvements in cognitive ability, non-cognitive ability, and educational attainment, relative to the reference group with no early-life exposure. Moreover, the magnitudes of the coefficients tend to be larger for earlier exposure ages, indicating that access to public libraries during early childhood is particularly productive, consistent with the presence of sensitive periods in human capital formation.

In contrast, the coefficients for children first exposed to a public library after the conception-to-age-five window are small in magnitude and statistically indistinguishable from zero. These post-window exposure bins serve as a placebo check, since children in these groups had no public library access during early childhood. The near-flat profile on the right-hand side of the figure indicates that counties receiving libraries at different times did not exhibit systematic differential trends in child outcomes among cohorts with no early-life exposure. This pattern supports the parallel-trends assumption that, absent early-childhood library exposure, outcomes would have evolved similarly across cohorts and counties. Finally, the estimates for depression (Panel C) display no systematic variation with age at first exposure and remain close to zero throughout the event window. This pattern mirrors the baseline regression results reported in Table 3 and indicates that early exposure to public libraries primarily affects cognitive, non-cognitive, and educational outcomes, with little detectable impact on mental health as measured by depression.

[Insert Figure 5 here]

5.2 Comparison with causal estimates in the literature

Early exposure to public libraries produces statistically significant improvements in children's cognitive, non-cognitive, and educational outcomes. To assess whether these effects are meaningful in magnitude, we compare our estimates with two strands of causal studies: infrastructure investments in developed countries and large-scale interventions in

developing countries. For comparability, we express the estimates as the effect of one additional year of exposure to the relevant infrastructure or intervention.

First, our estimated effects are larger than those documented for related investments in developed countries. Gilpin et al. (2024) find that U.S. public library capital investments increased students' reading achievement by about 0.01–0.04 standard deviations in the short run. By comparison, our estimate of 0.094 standard deviations for cognitive ability is roughly 2.4–9.4 times as large. This difference is consistent with the fact that we study early-life exposure in a developing-country setting where many counties were gaining standardized library access for the first time, whereas the U.S. estimates capture short-run effects in a mature and well-resourced public library system.

Second, our estimates are comparable to landmark interventions in developing countries. Duflo (2001) finds that Indonesia's large-scale school-building program increased schooling by 0.035 years, while our estimate implies 0.068 additional years of schooling per year of early-childhood library exposure. Adukia et al. (2020) show that rural road construction in India increased schooling by about 0.07 years, close to our estimate. These comparisons suggest that public libraries can generate educational gains comparable to those of major infrastructure programs in developing contexts.

Taken together, these comparisons show that the magnitude of our estimated effects is large relative to programs in developed countries and comparable to major infrastructure investments in developing contexts. Cultural infrastructure such as public libraries can be as productive as traditional investments in education when introduced in low-baseline environments. Policymakers should treat public libraries as essential infrastructure for human capital development and inclusive growth.

5.3 Heterogeneous analysis

We examine heterogeneity in early-life library exposure along three key dimensions: gender, household socioeconomic status (SES), and county educational development level. Household SES is proxied by parents' educational attainment. A family is classified as high

SES if at least one parent completed junior high school. County educational development is measured by total student enrollment in 2000, with counties above the median classified as high development. We estimate Equation (1) separately for each subgroup. Results are presented in Figure 6.

By gender. We find that public libraries generate larger and more significant benefits for girls than boys. Girls experience significant improvements in non-cognitive ability and educational attainment, though not in cognitive ability, while boys show smaller and statistically insignificant gains. One possible explanation is that patriarchal norms can generate gender differences in early-life investments (Deaton, 2008), so that early-life programs yield larger benefits for disadvantaged girls if parents respond by reallocating household resources in ways that partially compensate for girls' initial disadvantage (Zhang and Xu, 2016). This pattern is also consistent with prior evidence that early-life interventions often generate larger gains for girls in China and other developing-country settings (Field et al., 2009; Maluccio et al., 2009; Huang and Liu, 2023).

By household SES. In Panel B of Figure 6, we report estimates by socioeconomic status and find that the effects are stronger and broader among children from low-SES families. A natural interpretation is that libraries relax budget constraints faced by these households by providing free access to books, learning materials, and study space, so the marginal returns to an additional year of access are larger when private learning inputs at home are limited. These findings echo recent evidence that early-life exposure to infrastructure and healthcare reforms generates greater cognitive, non-cognitive, and educational returns for disadvantaged children (Huang and Liu, 2023; Zha and Zhou, 2025; Zhao et al., 2025). Our results suggest that public cultural infrastructure can reduce socioeconomic disparities in child development by disproportionately benefiting children from resource-poor households.

By county educational development. A growing literature shows that the marginal returns to public investments often depend on initial conditions and tend to be larger in places with lower baseline levels of education (Barro and Lee, 1996; Carneiro et al., 2011). In our setting, the marginal effect of public libraries may be larger in low-development counties,

where access to learning resources is initially more limited, than in high-development counties. Panel C of Figure 6 reports subgroup estimates by baseline county educational development and shows that library exposure has larger positive effects on cognitive ability, non-cognitive ability, and educational attainment in low-development counties than in high-development counties. This pattern suggests that public libraries disproportionately improve outcomes in initially resource-poor areas and thereby help narrow spatial disparities in child development.

[Insert Figure 6 here]

5.4 Robustness checks

After establishing the persistent effects of early public library exposure on child development outcomes, we conduct a comprehensive set of robustness checks to assess the validity and stability of our findings. In the main text, we address three potential concerns: (1) confounding from contemporaneous policies and investments; (2) sensitivity to model specification, alternative empirical strategies, and exposure definitions; and (3) potential cross-county spillover effects. We report additional robustness checks on heterogeneous treatment effects, violations of the parallel-trends assumption, placebo effects, and alternative sample definitions in Appendix Section C.

Confounding from contemporaneous policies and investments. A potential concern is that public library rollout may have coincided with other local policies or infrastructure improvements that also affected child development. To address this concern, we control for exposure to major education reforms, including the *Rural Education Informatization Reform*, the *School-Merging Policy*, the *Kindergarten Expansion Reform*, and the *Two Exemptions and One Subsidy program*; health and cultural policies, including the New Cooperative Medical Scheme, the National Fitness Plan, and the rollout of theaters and cinemas; and community-level infrastructure improvements, including access to tap water, electricity, television, landline telephones, mobile-signal coverage, public roads, and piped gas.[38] As

[38] For county-level reforms and cultural facilities, we define an exposure indicator equal to one if the policy or facility was

shown in Appendix Table C.1, the estimated effects of early-life library exposure remain similar in magnitude and statistically significant after adding these controls, suggesting that the baseline findings are unlikely to be driven by co-occurring policy changes or broader local development trends.

Model specification validity and alternative empirical strategies. We next examine whether the results are sensitive to alternative empirical strategies or modeling choices. First, following the logic of Borusyak and Hull (2023), we implement a design-based specification that recenters actual exposure by subtracting expected exposure under a counterfactual assignment process. Specifically, we randomly permute library opening years across counties, reconstruct individual-level exposure in each permutation, and use the average simulated exposure as expected exposure. Table 4 shows that the coefficients on recentered early-life library exposure remain positive and significant for cognitive ability, non-cognitive ability, and educational attainment, with magnitudes close to the baseline estimates. This indicates that the main findings are not driven by systematic timing-related differences between early- and late-adopting counties.

[Insert Table 4 here]

Second, we conduct a specification-curve analysis to assess robustness across alternative modeling choices. We vary five dimensions of the empirical specification: fixed effects, standard-error clustering, control variables, sample restrictions, and exposure definitions. Specifically, for fixed effects, we compare the baseline structure with alternatives that replace province-by-survey-year fixed effects with survey-year fixed effects or additionally include county-by-survey-year fixed effects. For standard errors, we cluster at the county, cohort, and province levels. For control variables, we compare the baseline controls with specifications that replace county-by-cohort controls with county-specific linear cohort trends. For sample restrictions, we include migrants, vary the age range from 10–19 to 10–18 or 10–20, and

in place in the individual's county at any point before age five, and zero otherwise. For community-level infrastructure, we use the CFPS community surveys to recover the first year in which each facility became available and construct cumulative years of exposure from conception through age five, following the construction of the baseline library-exposure measure.

examine whether library construction affects maternal fertility behavior. For exposure measurement, we replace the January-opening assumption with mid-year and year-end opening assumptions and use an alternative exposure window from conception to age four. Combining these choices yields 288 non-collinear specifications for each outcome. Figure 7 shows that the estimated effects on cognitive ability, non-cognitive ability, and educational attainment remain consistently positive across these specifications, while the estimates for depression remain small and centered around zero. These results suggest that the main conclusions are not driven by any particular choice of fixed effects, clustering level, control set, sample definition, or exposure measure.

[Insert Figure 7 here]

Spillover effects. Another concern is that children in untreated counties may benefit from libraries built in nearby treated counties, which would contaminate the comparison group. To test for cross-county spillovers, we use both ring-based and border-based designs following Zhao et al. (2025). In the ring-based design, we examine whether children in untreated counties located within 50, 75, or 100 kilometers of treated counties experience changes in developmental outcomes. In the border-based design, we construct a placebo exposure measure for counties adjacent to treated counties while controlling for actual treatment status. The results in Appendix Table C.13 show no meaningful spillover effects across distance bands or adjacent counties, indicating that the estimated effects primarily capture within-county improvements in library access.

Finally, we report a series of additional robustness checks in Appendix Section C. We first examine whether the results are sensitive to alternative identification strategies and control groups, including propensity-score reweighting, stacked difference-in-differences, regression discontinuity estimates around the exposure cutoff, and specifications that exclude never-treated counties from the control group. We then address concerns about treatment-effect heterogeneity, violations of parallel trends, and spurious timing correlations using heterogeneity-robust difference-in-differences estimators, sensitivity analyses for

parallel-trends violations, placebo tests among older cohorts with no early-childhood exposure, and permutation tests that randomly reassign library opening years across counties. Across these exercises, the estimated effects on cognitive ability, non-cognitive ability, and educational attainment remain positive and statistically significant, while the estimates for depression remain close to zero.

6. Mechanism analysis

Our results show that early exposure to public libraries significantly improves children's cognitive, non-cognitive, and educational development. To shed light on the underlying mechanisms, we examine three potential channels observable in the CFPS data: (i) parental investment and parenting styles, (ii) early-childhood family environment, and (iii) children's learning habits. We first construct a set of mechanism variables from relevant CFPS survey waves and age modules,[39] and then re-estimate Equation (1) by replacing the primary outcome variable with each mechanism measure in turn to explore the underlying channels.

6.1 Parental investment and parenting styles

We first examine whether early-life access to public libraries affects parental investment and parenting practices. Public libraries may lower the cost of educational inputs, provide parents with information and opportunities for parent-child learning, and increase the perceived returns to early investment (Cunha et al., 2010; Del Boca et al., 2014). To identify underlying mechanisms, we examine how early exposure to public libraries influences both parental investment and parenting styles.[40]

We first focus on three dimensions of parental investment: monetary investment in education, time devoted to childcare, and educational expectations for children. Monetary investment is quantified as annual educational expenditure (measured in hundreds of *yuan*),

[39] We provide the details regarding the specific survey waves and age modules used for each mechanism variable in Appendix Table B.4, and we report their summary statistics in Appendix Table B.7.
[40] We provide detailed variable definitions and construction procedures in Appendix Table B.5.

while time investment captures the daily hours parents devote to childcare. Educational expectations are proxied by the total years of schooling parents aspire for their children to complete. We then assess effects on parenting style using the Parental Bonding Instrument (PBI) developed by Parker et al. (1979), which captures two behavioral dimensions: emotional warmth and autonomy support. Following Li et al. (2023), we construct four parenting types from the CFPS data: optimal parenting (high warmth, high autonomy), affectionate constraint (high warmth, high control), affectionless control (low warmth, high control), and neglectful parenting (low warmth, low autonomy).

Table 5 reports the estimated effects of early-life library exposure on parental behaviors. The estimates in Panel A indicate that early exposure significantly bolsters parental investment, as evidenced by positive and statistically significant coefficients for educational spending, time spent with children, and educational expectations. Regarding parenting styles in Panel B, we find that library exposure significantly enhances the likelihood of optimal parenting by 0.4 percentage points ($p < 0.01$). In contrast, we find no evidence of significant shifts in the probabilities of affectionate constraint, affectionless control, or neglectful parenting.

These findings suggest that public libraries foster child development by transforming parental behavior and family dynamics. They stimulate greater educational investment, strengthen parent-child engagement, raise expectations, and promote supportive parenting. Unlike income-based transfers, which often improve material inputs without changing parenting quality (Lundberg et al., 2016), libraries combine resource access with social learning opportunities that reshape parental beliefs. This evidence underscores the unique role of cultural infrastructure in influencing human capital through behavioral as well as material channels.

[Insert Table 5 here]

6.2 Early-childhood family environment

Beyond direct parental investments and parenting style, we examine whether public libraries improve the early-childhood family environment. The home environment is a central input in early human-capital formation, shaping both cognitive and non-cognitive skills (Cunha and Heckman, 2007; Heckman and Mosso, 2014; Del Boca et al., 2014). Public libraries may enrich this environment by providing free access to books and educational materials and by offering accessible spaces for reading and learning, especially for families with limited learning resources at home. These resources may encourage parents to engage more frequently in storytelling, literacy activities, book purchasing, and recreational outings with children.

We construct five measures of early childhood family environment from the CFPS retrospective modules, guided by the Home Observation for Measurement of the Environment (HOME) framework (Bradley et al., 1979). The primary measure, the *Home Index*, aggregates standardized scores from four distinct components including parental storytelling, book purchasing, recreational outings, and literacy activities, all of which are also analyzed separately to capture specific environmental dimensions.[41] We reconstruct family environments for ages 3–5 from retrospective reports and link them to adolescent outcomes (ages 10–19) to ensure clear temporal ordering.

Table 6 presents the estimated effects of early-life library exposure on early-childhood family environment measures. Access to a library from the period of conception through age five significantly increases the composite Home index by 0.200 standard deviations, with the most pronounced impacts observed in parental storytelling and literacy activities, which increase by 0.25 and 0.26 standard deviations, respectively. We also find smaller but significant gains of 0.20 standard deviations in book purchasing and 0.14 standard deviations in recreational outings. These effects are economically large, because at the upper bound of the exposure measure (six years) the estimates imply a 1.2 standard deviation increase in the Home Index, which corresponds to an improvement of roughly 45 percentile points in the home-environment distribution (for example, moving from the 25th to about the 70th

[41] We provide detailed item construction in Appendix Table B.6.

percentile). Overall, these findings provide suggestive evidence of a mechanism through which early-life library exposure fosters child development: by expanding access to books and modeling educational engagement, libraries strengthen the family routines that constitute a higher-quality home learning environment.

[Insert Table 6 here]

6.3 Children's learning habits

In this subsection, we examine whether early-life library exposure generates persistent learning behaviors in adolescence. Because skills are self-productive and dynamically complementary, early gains may raise the returns to later investments and encourage sustained academic effort (Cunha and Heckman, 2007). Public libraries may contribute to this process by fostering early reading habits, lowering the cost of subsequent learning, and strengthening children's motivation to engage in academic activities.

To empirically test this pathway, we utilize CFPS data to examine four key dimensions of adolescent learning: extracurricular reading participation, the number of books read, weekly study hours, and academic ranking. As reported in Table 7, the estimates reveal that each additional year of library access before age five significantly enhances learning engagement: it raises reading participation by 6 percentage points, reading volume by 0.91 books, weekly study time by 0.23 hours, and academic ranking by 0.15 units. Collectively, these findings suggest that early-life library exposure fosters a sustained commitment to academic activities, ultimately driving long-term improvements in educational performance and human capital accumulation.

[Insert Table 7 here]

7. Discussion: A benefit-cost analysis

To evaluate the economic viability and policy significance of our findings, we provide a back-of-the-envelope calculation comparing the fiscal costs of library expansion against its

long-term social benefits. This analysis quantifies the aggregate returns for the cohort of children who gained access to public libraries between conception and age five during the expansion period. By translating our estimated developmental gains into monetary values, we can assess whether the large-scale public investment in cultural infrastructure is justified from a human capital perspective.

We first determine the cost of library construction. To capture the full fiscal burden of the expansion, we account for both upfront capital outlays and ongoing operating expenditures, since a functioning public library requires not only physical space but also recurrent inputs such as staff, acquisitions and collection maintenance, and day-to-day operations. We measure the scale of the program using the national increase in public-library floor space between 2002 and 2015, and we monetize this increase using two components priced in 2008[42]. Specifically, we estimate capital costs by multiplying the additional floor space by the average construction cost per square meter for cultural facilities in 2008[43], and we estimate operating costs by multiplying the average annual operating expenditure per square meter in 2008 by the same increase in floor space[44]. Summing these two components yields a total cost of RMB 21.7 billion in 2008 constant prices, which we interpret as the aggregate public spending required to expand and operate county-level libraries during the early-life exposure window from conception through age five.

Next, we calculate the social benefits. Benefits arise from improved cognitive ability and increased educational attainment, which raise lifetime earnings.[45] We begin by identifying the affected population: combining the total number of children born during 1997–2010 with

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[42] We adopt 2008 as the reference price year because it lies near the midpoint of both the NCIRSP library expansion period (2002–2015) and the early-childhood exposure window for the affected birth cohorts (1991–2010). It represents typical investment levels during large-scale library expansion and reflects the economic context in which sample children formed early skills.

[43] According to the *China Library Yearbook (2002–2015)*, newly built public libraries in China added 7,727,000 m² of total floor space. The *China Statistical Yearbook on Fixed Asset Investment (2009)* reports an average construction cost of RMB 2,142 per m² for cultural facilities in 2008. Multiplying these values gives RMB 16.55 billion in total construction expenditure at 2008 prices.

[44] According to the *China Library Yearbook (2009)*, the total operating expenditure of public libraries divided by total floor area implies an average annual operating cost of RMB 666.46 per m² in 2008. Applying this figure to the new library area (7,727,000 m²) gives RMB 5.15 billion in total operating costs (at 2008 prices).

[45] We focus on cognitive ability and education as the primary channels through which library access affects earnings. The literature finds no significant association between noncognitive ability and labor income (Glewwe et al., 2017; Glewwe et al., 2022).

the increase in county-level library coverage implies that about 40.54 million children gained access to a public library during the early-life exposure window from conception through age five.[46] Second, following Lindqvist and Vestman (2011), Chen et al. (2022), and Huang and Liu (2023), we estimate the wage returns to cognitive ability and education. OLS regressions yield returns of 13.1 percent for cognitive ability and 15.4 percent for education,[47] which we treat as upper-bound parameters. More conservative estimates in the literature are 10 percent for cognition (Glewwe et al., 2022) and 12 percent for education (Huang et al., 2022), and our baseline calibration uses the midpoint of these ranges. Applying the estimated treatment effects to these wage returns yields an annual earnings gain of 314.74 RMB per child.[48] Third, discounting future earnings at 5 percent yields a lifetime present value of 1,846.23 RMB per child.[49] Aggregating across the affected population produces baseline total social benefits of RMB 74.84 billion in 2008 prices.

Using these estimates, we compare the discounted costs and benefits over time. Table 8 summarizes the benefit-cost results. Under the baseline specification in column (1), total social benefits amount to RMB 74.84 billion and exceed total costs of RMB 21.70 billion (in 2008 prices), implying a net present value (NPV) of RMB 53.14 billion and a benefit–cost ratio (BCR) of 3.45. The program breaks even around the 27th year and yields an internal rate of return (IRR) of 9.29 percent. Column (2) applies a conservative scenario that reduces the wage returns to education and cognition, which lowers aggregate benefits to RMB 65.14 billion (NPV = RMB 43.44 billion; BCR = 3.00), delays the break-even point to the 28th year, and decreases the IRR to 8.80 percent. Column (3) assumes stronger productivity gains and higher exposure intensity, raising total benefits to RMB 84.54 billion (NPV = RMB 62.78

[46] Data source: China Statistical Yearbook, various years 1997-2010; China Library Yearbook, various years 2002-2015. The total population of children from conception through age five born between 1997 and 2010 equals 236.36 million. County-level public library coverage increased by 17.15 percentage points during the NCIRSP period (2002-2015). Exposed population = 236.36 × 0.1715 = 40.54 million.

[47] The sample includes workers aged 25-35 from CFPS 2010. The median annual income equals 15,645.93 RMB. The dependent variable is log annual income. Explanatory variables include years of schooling, standardized cognitive test scores, gender, *hukou* status, parental age, parental education, and family size. The specification includes birth year and county fixed effects with standard errors clustered at the birth year level.

[48] According to Table 3, library access increases education by 0.068 years and cognitive ability by 0.098 standard deviations. The CFPS 2010 average annual income of 15,645.93 RMB deflates to 15,252.88 RMB in 2008 prices. Annual benefit = 15,252.88 × (0.068 × 0.137 + 0.098 × 0.1155) = 314.74 RMB.

[49] The calculation assumes individuals work from age 22 to age 62, consistent with World Bank estimates of 40-year working lives in China. Future earnings are discounted at 5 percent annually.

billion; BCR = 3.90), advancing the break-even year to the 26th year, and increasing the IRR to 9.72 percent. Across all scenarios, benefits exceed costs within roughly three decades, and the IRR remains above the social discount rate, indicating that the county-level public library expansion generated substantial and durable economic gains.

To contextualize this finding, we compare our estimated BCR with those from evaluations of similar large-scale programs in both developed and developing countries, as summarized in Appendix Table D.2 Our estimated BCR of 3.45:1 substantially exceeds the BCR of 0.54:1 for China's contemporaneous expansion of the NCMS health insurance program (Huang and Liu, 2023). This contrast suggests that cultural infrastructure may offer superior social returns compared to certain health access programs in the same period. More strikingly, our estimate far exceeds the BCR of 1.23:1 reported for public library capital investments in the United States (Bhatt, 2010). A plausible interpretation is that the marginal value of library infrastructure depends on baseline access: in the United States, where public libraries were already widespread, additional investments likely generated relatively small incremental benefits, whereas in many rural and less developed Chinese counties in the 2000s, establishing a standardized county library introduced a previously scarce learning resource and therefore produced larger marginal impacts.

We also compare the magnitude of our BCR to those of the most successful education programs documented in the literature. Our BCR estimate is lower than the 6:1 BCR of the HighScope Perry Preschool Project, which is a targeted intervention for disadvantaged children (García et al., 2023). Additionally, our BCR is nearly one-third of the 10.89:1 BCR associated with Indonesia's INPRES school construction program (Akresh et al., 2023). Both China's county-level library construction and INPRES represent national, supply-side interventions that rapidly expanded access to foundational institutions in underserved areas—libraries and schools, respectively. Our results indicate that the universal provision of cultural infrastructure, such as libraries, may yield lower social returns compared to targeted interventions or those focused on the formal education stage.

The actual benefits of early exposure to public libraries may exceed our estimates. Two

factors support this view. First, the analysis does not account for intergenerational spillovers. By improving parental engagement and teaching practices, the library program may have enhanced the quality of future parenting and created persistent gains across generations (Baker et al., 2001; Zumbuehl et al., 2021). Second, our estimates omit welfare effects beyond income, including civic participation, criminal activities, and social cohesion (Laurence, 2011; Klinenberg, 2018). These omitted gains suggest that our BCR is a lower bound. Overall, even with conservative assumptions, the NCIRSP library expansion represents a highly worthwhile public investment. Governments can continue to strengthen public library systems, as the long-term social and economic benefits far outweigh the initial costs.

[Insert Table 8 here]

8. Conclusion

This paper provides the first large-scale causal evidence that early childhood access to public libraries significantly improves long-run human capital in China. Exploiting variation in the timing of the county-level rollout of new-built public libraries, we find that each additional year of public library exposure from conception to age five raises cognitive ability by 0.094 standard deviations, non-cognitive ability by 0.098 standard deviations, and completed schooling by 0.068 years. The effects are stronger for girls, children from disadvantaged households, and counties with lower baseline education level. Public libraries thus act as equalizers of early developmental opportunities. The gains arise through three channels: early-life library access increases parental investments and fosters more supportive parenting practices, improves the quality of the home learning environment, and helps children develop sustained study habits that persist into adolescence.

Based on our baseline estimates, we conduct a back-of-the-envelope calculation to assess the social benefits and costs of public library construction. Although we only include the income gains from improved cognitive ability and increased educational attainment, total

social benefits of RMB 74.84 billion far exceed total costs of RMB 21.70 billion (2008 prices). The benefit–cost ratio approximates 3.4 to 1, and the internal rate of return is 9.29 percent. These figures likely understate the true returns because of the potential intergenerational spillovers and non-pecuniary benefits. Future research could examine longer-term outcomes such as labor market performance and intergenerational mobility to guide more effective policy replication across developing contexts.

Our findings carry clear policy implications for developing and advanced economies alike. First, developing countries should not overlook the role of cultural infrastructure when allocating resources for human-capital formation. Public libraries represent a cost-effective instrument for human capital development and require no complex goal-oriented mechanism design. Second, libraries serve as essential social infrastructure for equality. Beyond redistributing resources, they ensure that every citizen can pursue knowledge with equal dignity by providing a shared intellectual common. Thus, they can reduce regional inequalities in the dimension of human capital accumulation. Third, the standardized and replicable nature of public library construction makes it well suited for rapid scaling in other developing countries. In resource constrained environments, policymakers can prioritize libraries because they offer a unique multiplier effect on public expenditure. These facilities are not discretionary luxuries but strategic foundations that provide the highest return on investment for long term human development. Overall, public libraries are a proven yet largely overlooked instrument for human capital formation and inclusive growth. Investing in cultural infrastructure can strengthen equality of opportunity and sustain long-term human capital development.

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Figures and Tables

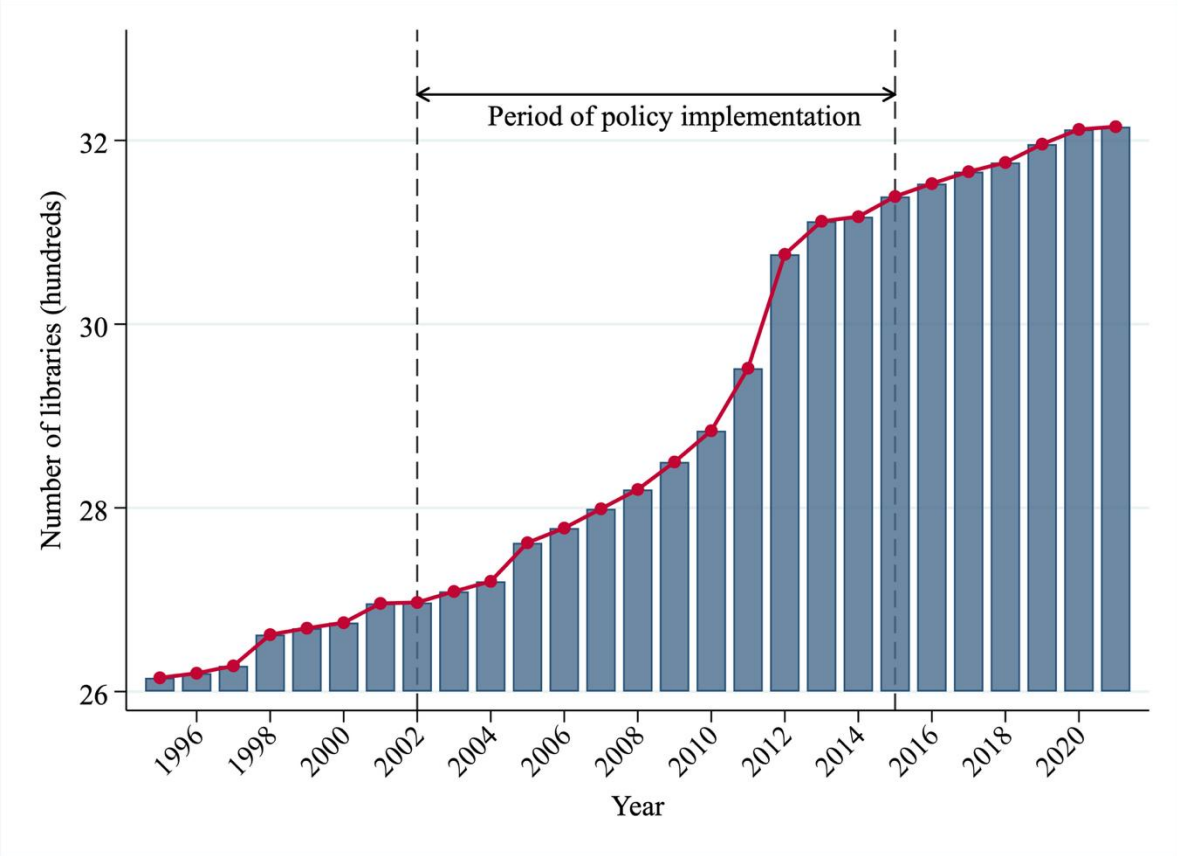


Figure 1 National trends in number of public libraries

Notes: The bars and red dots show the total number of public libraries nationwide in each year. The red line represents the growth trend. The dashed vertical lines indicate the period of NCIRSP policy implementation that promoted county-level library expansion. Data are from the *Statistical Yearbook of Chinese Libraries*.

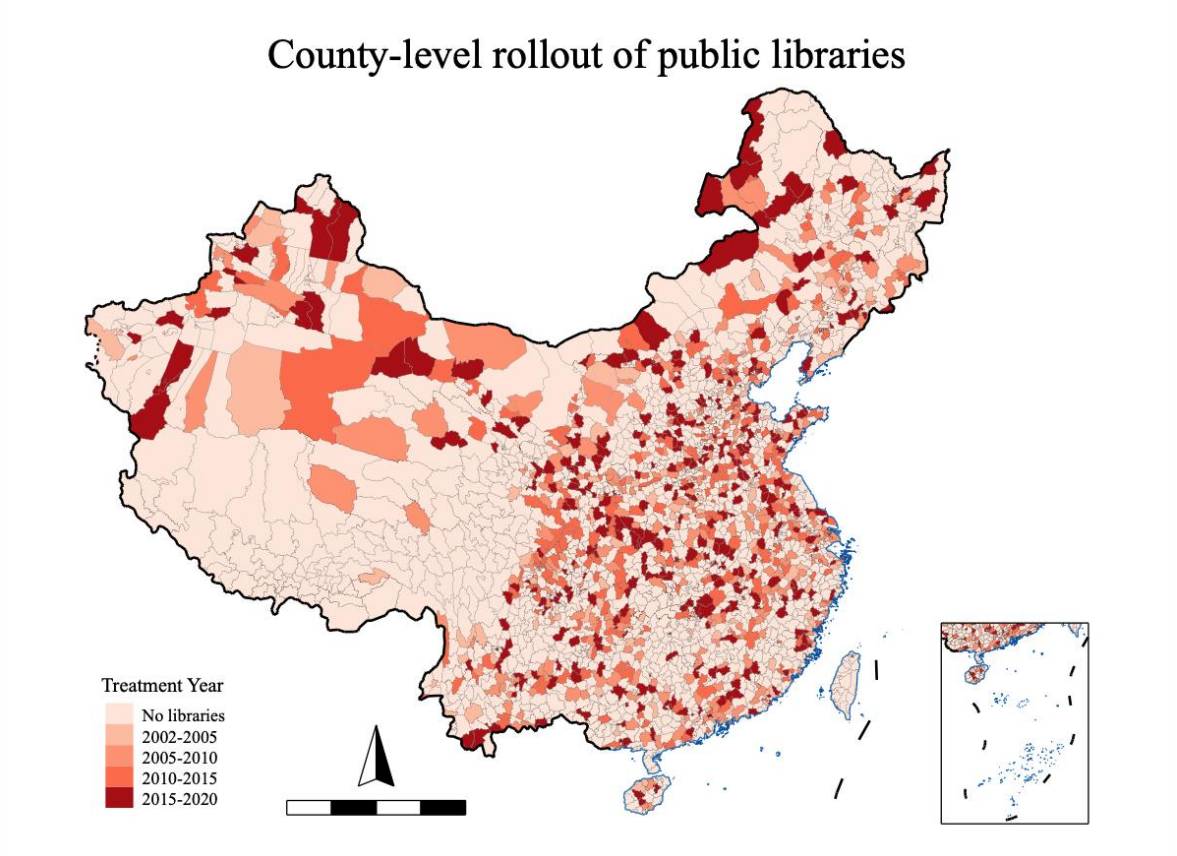


Figure 2 County-by-county roll-out of public libraries over the years

Notes: The map shows the county-level rollout of public libraries across China. Each color shade represents the period in which a county established its first public library. Darker colors indicate later implementation years. Counties without a public library are shown in light beige. Data are compiled from prefecture and county statistical yearbooks.

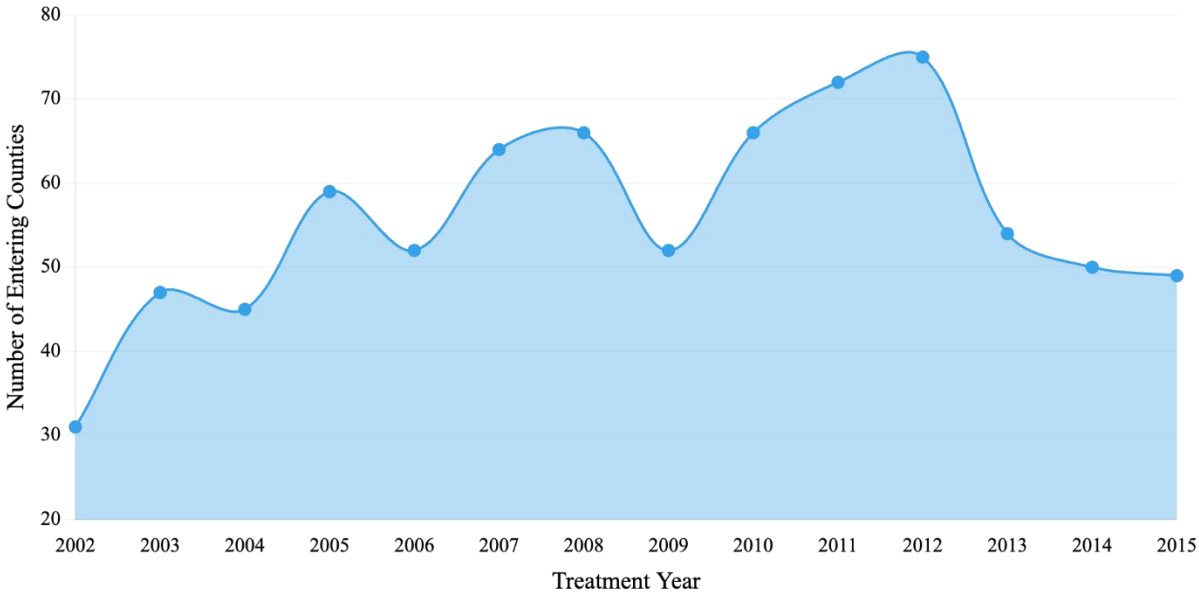


Figure 3 Number of counties by their year of entry into treatment

Notes: The figure shows the number of counties that established a public library in each year during the implementation period of the NCIRSP from 2002 to 2015. The height of each bar represents the number of counties entering treatment in that year. Data are compiled from prefecture and county statistical yearbooks.

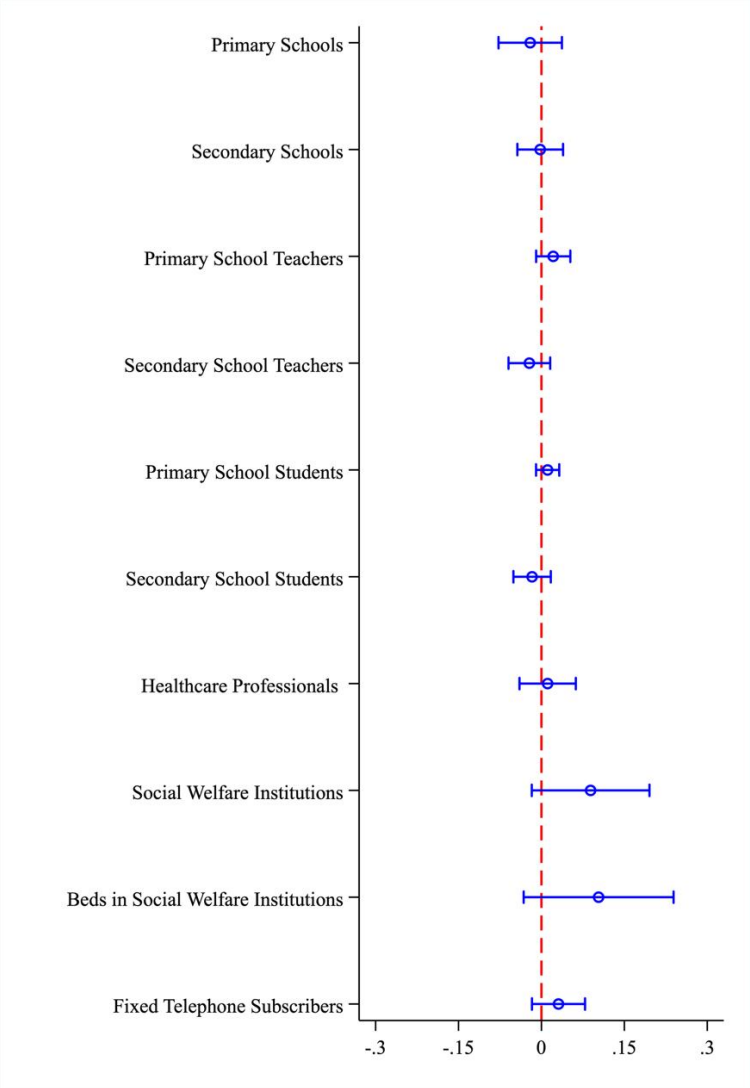


Figure 4 Balancing test

Notes: This figure visualizes the county-level balancing results for public service characteristics listed in Panel B of Table 2. Each point represents the estimated conditional difference between treatment and control counties after controlling for the baseline county-level characteristics $\eta_b CB_c$ in Equation (1). Horizontal bars show 95 percent confidence intervals. The red dashed line indicates zero difference. Robust standard errors are used to construct the confidence intervals.

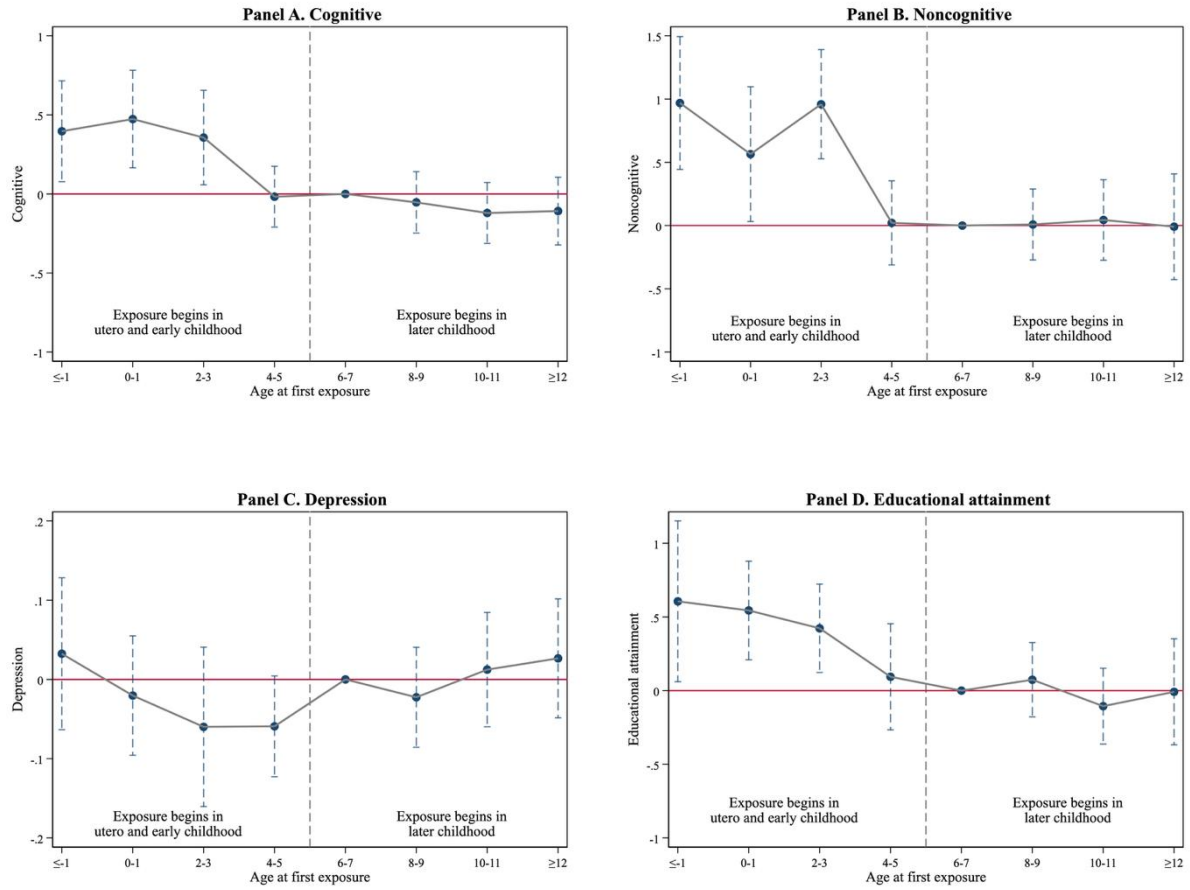


Figure 5 Event study – Effects of public libraries by age at first exposure

Notes: These figures illustrate the impacts of early-life public library exposure on child development outcomes. The outcome variables are indicated in the title of each panel. Each panel presents event-study coefficients estimated from Equation (2) using OLS. The sample includes individuals from birth cohorts 1991 to 2010 who were aged 10 to 19 in the CFPS waves from 2010 to 2020. The figure plots the estimated coefficients and the 90 percent confidence intervals from the main specification, where the key explanatory variables are categorical indicators for the age at which a county public library was first established. Age 6–7 serves as the omitted reference group. Negative event times indicate that exposure began in utero (i.e., full early-life exposure).

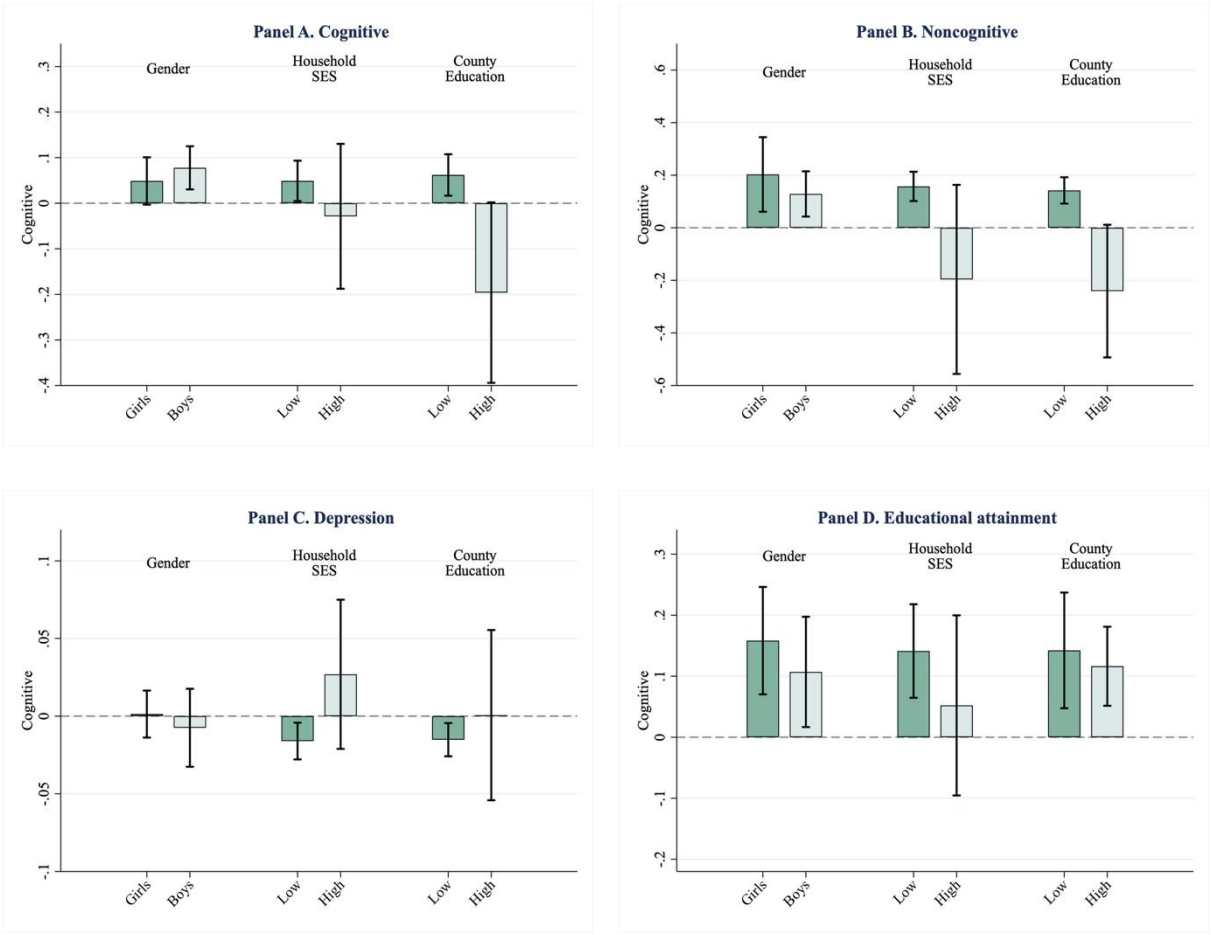


Figure 6 Heterogeneity analysis

Notes: These figures present subgroup estimates examining heterogeneity in the effects of public library exposure during early childhood (from conception through age 5) on adolescent outcomes. Each bar corresponds to a separate regression of Equation (1) estimated on the indicated subsample. In the first pair of bars, the dark green bars report estimates for girls, and the light green bars report estimates for boys. In the second pair of bars, the sample is divided by household socioeconomic status (SES), measured by parental education. Households with low SES are defined as those in which neither parent completed junior secondary school. Households with high SES are defined as those in which at least one parent completed junior secondary school or higher, and these estimates are shown in light green. In the third pair of bars, counties are grouped by baseline educational infrastructure, proxied by total student enrollment in 2000. Counties below the national mean are classified as low infrastructure and shown in dark green, while counties above the mean are classified as high infrastructure and shown in light green. Ninety percent confidence intervals are reported.

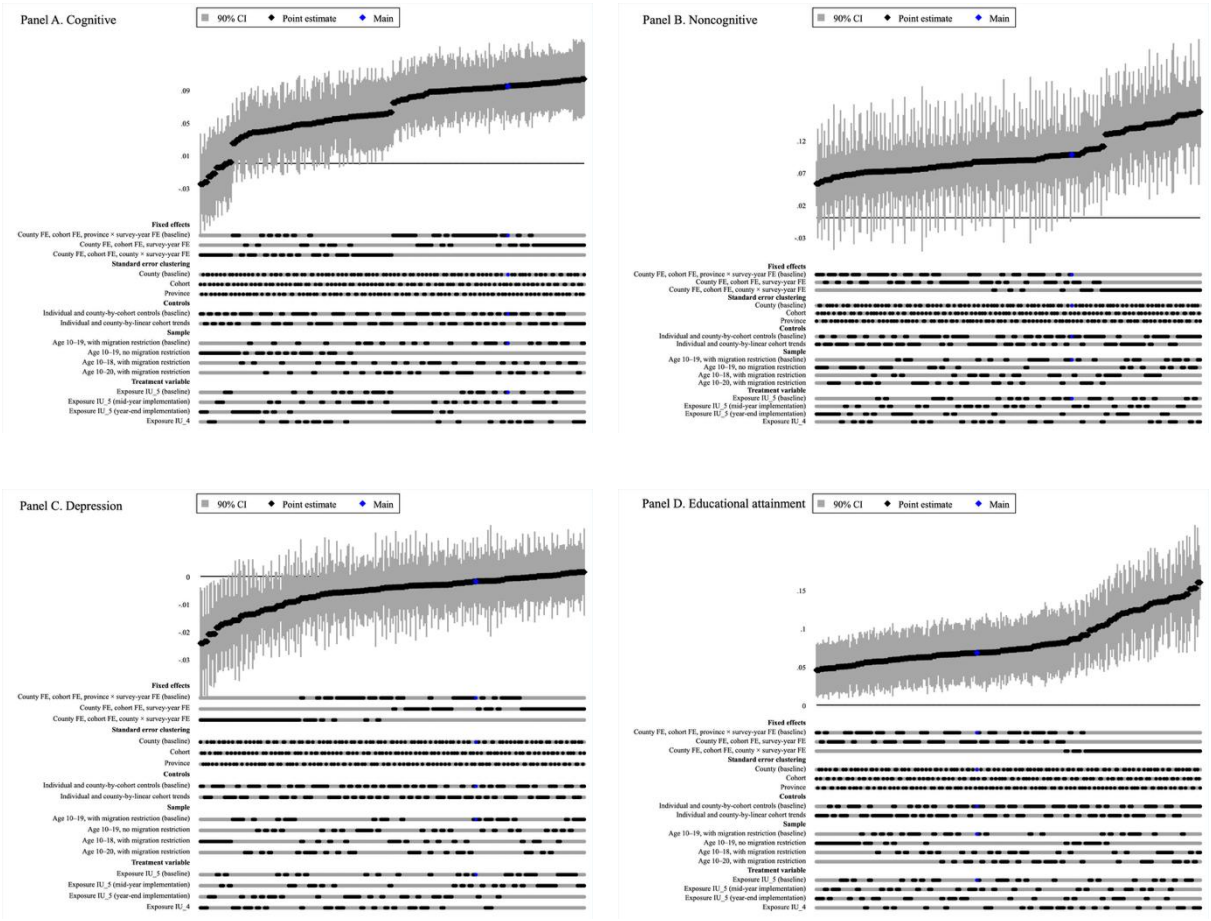


Figure 7 Specification curve

Notes: These figures display robustness tests across 288 alternative specifications. Each point represents an estimated coefficient and its 90% confidence interval, with blue markers indicating results from the main specification of Table 3. Specifications vary along four dimensions. First, fixed effects follow three alternative setups: (i) the baseline includes county and cohort fixed effects with province-by-survey-year interactions; (ii) an alternative uses survey-year fixed effects instead of province-by-survey-year interactions; (iii) another includes county-by-survey-year fixed effects (in addition to the county and cohort fixed effects). Second, standard errors are clustered at different levels: at the county level (baseline), or alternatively by cohort or province. Third, control variables differ across specifications: the baseline includes individual and county-by-cohort controls, while an alternative adds individual and county-by-linear cohort trends. Fourth, sample definitions vary from the baseline group of individuals aged 10–19 (excluding migrants) to alternative samples including (i) age 10–19 without migration restriction, (ii) age 10–18 excluding migrants, and (iii) age 10–20 excluding migrants. Fifth, the main treatment variable is *Library exposure IU 5*, defined using program implementation at the beginning of the year, with alternative exposure definitions based on mid-year, end-year, or a narrower exposure window (*IU 4*).

Table1 Summary statistics

Variables	Obs	Mean	SD
	(1)	(2)	(3)
Treatment variables			
Library exposure IU-5	19,162	0.210	0.943
Outcome variables			
Cognitive ability	12,739	-0.008	0.989
Non-cognitive ability	4,071	-0.011	0.992
Depression	14,617	0.178	0.383
Educational attainment	18,905	6.972	3.195
Individual and household characteristics			
Age	19,162	14.225	2.814
Male	19,162	0.519	0.500
Agricultural *hukou*	19,162	0.831	0.375
Parents' average age	19,162	41.419	5.275
Parents' educational attainment	19,162	6.118	3.851
Household size	19,162	4.903	1.717
Household total income	19,162	10.171	1.348
Selected county characteristics			
GDP	19,162	5.256	9.760
Total population	19,162	1.291	0.014
Sex ratio	19,162	107.656	10.643
Proportion of population aged 0–14	19,162	25.430	6.257
Proportion of population aged 65 and above	19,162	6.728	1.800
Average years of education	19,162	7.251	1.645
Urbanization rate	19,162	21.270	25.058
Mountainous county	19,162	0.376	0.484
Number of hospital and clinic beds	15,013	775.003	570.070

Notes: This table provides descriptive statistics for the main variables used in the baseline analysis. The sample consists of 8,764 children born between 1991 and 2010 from 160 sampled counties, who were interviewed in six CFPS waves from 2010 to 2020 at ages 10 to 19. "Library exposure IU-5" measures the number of years between conception and age five during which a public library was available in the county of birth. Cognitive and non-cognitive abilities are standardized. Educational attainment is measured in completed years of schooling. Hukou refers to the household registration system in China. County-level variables are measured in 2000 and drawn from the *2000 Population Census* and the *2001 China County*

Statistical Yearbook. GDP is proxied by nighttime light intensity. The urbanization rate is measured as the share of agricultural population.

Table 2 Balancing test

		(1) Treatment group	(2) Control group	(3) Unconditional difference	(4) Conditional difference	(5) Observation
Panel A: Selection criteria						
GDP		6.43	4.33	2.103***		2,652
		(11.46)	(8.80)	(0.588)		
Total population		12.96	12.70	0.253***		2,312
		(0.72)	(0.85)	(0.041)		
Sex ratio		106.11	106.85	-0.744***		2,312
		(5.09)	(4.85)	(0.280)		
Proportion of population aged 0–14		22.09	23.56	-1.471***		2,312
		(5.00)	(5.42)	(0.282)		
Proportion of population aged 65 and above		7.27	6.81	0.457***		2,312
		(1.88)	(1.79)	(0.104)		
Average years of education		7.69	7.43	0.259***		2,312
		(0.92)	(1.39)	(0.057)		
Urbanization rate		24.86	26.07	-1.201		2,312
		(21.20)	(23.38)	(1.199)		
Mountainous county		0.20	0.32	-0.120***		2,677
		(0.40)	(0.47)	(0.022)		
Number of hospital and clinic beds		9.11	7.48	1.637***		2,115
		(6.42)	(5.90)	(0.372)		
Panel B: Characteristics of regional public services in 2000						
Education Services	Primary schools	5.10	5.13	-0.032	-0.020	1,266
		(0.85)	(0.90)	(0.062)	(0.029)	
	Secondary schools	3.35	3.17	0.176***	-0.002	1,262
		(0.66)	(0.78)	(0.049)	(0.021)	
	Primary school teachers	7.62	7.48	0.139***	0.021	1,331
		(0.66)	(0.77)	(0.047)	(0.016)	
	Secondary school teachers	7.23	7.01	0.221***	-0.022	1,193
		(0.75)	(1.02)	(0.058)	(0.019)	
	Primary school students	10.64	10.40	0.240***	0.011	2,168
		(0.79)	(1.00)	(0.048)	(0.011)	
	Secondary school students	10.09	9.66	0.432***	-0.017	2,154
		(0.79)	(1.32)	(0.052)	(0.017)	
Healthcare and Welfare Services	Healthcare professionals (hospitals & clinics)	6.94	6.59	0.353***	0.011	814
		(0.86)	(0.87)	(0.074)	(0.026)	
	Social welfare institutions	2.53	2.31	0.228***	0.089	1,774
		(0.95)	(1.01)	(0.060)	(0.054)	
	Beds in social welfare institutions	5.42	5.05	0.371***	0.103	1,771
		(1.34)	(1.38)	(0.084)	(0.069)	
Communication Services	Fixed telephone subscribers	10.41	9.85	0.558***	0.031	2,067
		(0.93)	(1.28)	(0.059)	(0.024)	

Notes: This table reports the balancing test comparing pre-treatment characteristics between counties with and without public library establishment. Panel A presents county-level baseline characteristics corresponding to $\eta_b CB_c$ in Equation (1), including economic, demographic, and social development indicators. Panel B reports county-level public service characteristics in 2000. Columns (1) and (2) show mean values for the treatment and control groups. Column (3) reports unconditional mean differences. Column (4) reports conditional differences after controlling for the county-level characteristics listed in Panel A. Robust standard errors are reported in parentheses. *, ** and *** represent statistical significance at the 10%, 5% and 1% levels, respectively.

Table 3 Baseline estimation

Dep. var.:	Cognitive ability	Non-cognitive ability	Depression	Educational attainment
	(1)	(2)	(3)	(4)
Library exposure IU-5	0.094***	0.098***	-0.002	0.068***
	(0.021)	(0.029)	(0.006)	(0.026)
Individual and household controls	Yes	Yes	Yes	Yes
County base controls × cohort FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Province × survey FE	Yes	Yes	Yes	Yes
Observations	9,792	3,136	11,321	14,800

Notes: Each column reports a separate regression based on Equation (1). The sample includes individuals from birth cohorts 1991 to 2010 who were aged 10 to 19 in the CFPS waves from 2010 to 2020. Individual and household controls include the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status. The county-level controls used to construct interactions with cohort fixed effects include GDP, population size, sex ratio, age structure, average years of education, urbanization rate, and the number of hospital and clinic beds. All regressions include county fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects. Standard errors are clustered at the county level. *, ** and *** represent statistical significance at the 10%, 5% and 1% levels, respectively.

Table 4 Robustness checks: accounting for non-random exposure

	(1)	(2)	(3)	(4)
	Cognitive ability	Non-cognitive ability	Depression	Educational attainment
Library exposure IU-5 (recentered)	0.093***	0.107***	-0.001	0.079***
	(0.022)	(0.028)	(0.006)	(0.027)
RI 90% CI	[0.022, 0.164]	[0.017, 0.204]	[-0.017, 0.014]	[0.008, 0.137]
Observations	9,792	3,136	11,321	14,800

Notes: The local and individual-level estimations control for potential counterfactual shocks by averaging over 999 random permutations of library establishment years across counties. Following Borusyak and Hull (2023), the table reports regressions with a recentered exposure measure and controlling for mean placebo exposure generated by the same counterfactuals. Randomized-inference-based 90% confidence intervals are reported. Robust standard errors are in parentheses. *, ** and *** represent statistical significance at the 10%, 5% and 1% levels, respectively.

Table 5 The effect of library exposure on parental investment and parenting styles

	(1)	(2)	(3)	(4)
Panel A. Educational investments				
	Educational expenditure	Time spent with child	Educational expectation	
Library exposure IU-5	0.474**	0.144**	0.081*	
	(0.224)	(0.058)	(0.042)	
Observations	12,949	3,883	7,620	
Panel B. Parenting skills				
	Optimal parenting	Affectionate constraint	Affectionless control	Neglectful parenting
Library exposure IU-5	0.004***	-0.003	-0.001	-0.001
	(0.001)	(0.002)	(0.001)	(0.001)
Observations	15,012	15,012	15,012	15,012
Individual and household controls	Yes	Yes	Yes	Yes
County base controls × cohort FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Province × survey FE	Yes	Yes	Yes	Yes

Notes: Each parameter is estimated from a separate regression using Equation (1). The dependent variables are presented in the first row of each panel. The sample includes individuals from birth cohorts 1991 to 2010 who were aged 10 to 19 in the CFPS waves from 2010 to 2020. Individual and household controls include the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status. The county-level controls used to construct interactions with cohort fixed effects include GDP, population size, sex ratio, age structure, average years of education, urbanization rate, and the number of hospital and clinic beds. All regressions include county fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects. Standard errors are clustered at the county level. *, ** and *** represent statistical significance at the 10%, 5% and 1% levels, respectively.

Table 6 The effect of library exposure on childhood family environment

	(1)	(2)	(3)	(4)	(5)
	Home index	Story	Book	Tour	Literacy
Library exposure IU-5	0.200***	0.247***	0.204***	0.135*	0.257**
	(0.056)	(0.071)	(0.037)	(0.081)	(0.110)
Individual and household controls	Yes	Yes	Yes	Yes	Yes
County base controls × cohort FE	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
Province × survey FE	Yes	Yes	Yes	Yes	Yes
Observations	2,747	2,747	2,747	2,747	2,747

Notes: Each parameter is estimated from a separate regression using Equation (1). The dependent variables are presented in the first row of each panel. The sample includes individuals from birth cohorts 1991 to 2010 who were aged 10 to 19 in the CFPS waves from 2010 to 2020. Individual and household controls include the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status. The county-level controls used to construct interactions with cohort fixed effects include GDP, population size, sex ratio, age structure, average years of education, urbanization rate, and the number of hospital and clinic beds. All regressions include county fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects. Standard errors are clustered at the county level. *, ** and *** represent statistical significance at the 10%, 5% and 1% levels, respectively.

Table 7 The effect of library exposure on children's learning outcomes

	(1)	(2)	(3)	(4)
	Reading participation	Reading volume	Study time	Academic ranking
Library exposure IU-5	0.060*	0.907*	0.228**	0.153***
	(0.034)	(0.532)	(0.115)	(0.051)
Individual and household controls	Yes	Yes	Yes	Yes
County base controls × cohort FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Province × survey FE	Yes	Yes	Yes	Yes
Observations	4,762	3,527	2,860	4,443

Notes: Each parameter is estimated from a separate regression using Equation (1). The dependent variables are presented in the first row of each panel. The sample includes school-enrolled individuals from birth cohorts 1991 to 2010 who were aged 10 to 19 in the CFPS waves from 2010 to 2020. Individual and household controls include the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status. The county-level controls used to construct interactions with cohort fixed effects include GDP, population size, sex ratio, age structure, average years of education, urbanization rate, and the number of hospital and clinic beds. All regressions include county fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects. Standard errors are clustered at the county level. *, ** and *** represent statistical significance at the 10%, 5% and 1% levels, respectively.

Table 8 Benefit-cost analysis of public library construction

	(1)	(2)	(3)
Panel A. Parameters			
Discount rate (%)	5	5	5
Benefit estimation	Baseline	Lower bound	Upper bound
Working life start age	22	22	22
Working life end age	62	62	62
Total 0-5 yr population (1997-2010, millions)	236.36	236.36	236.36
Library coverage rate (%)	17.15	17.15	17.15
Exposed population (millions)	40.54	40.54	40.54
Returns to education (%)	13.70	12.00	15.40
Returns to cognitive skills (%)	11.55	10.00	13.10
Avg. annual income (RMB, 2008)	15,252.88	15,252.88	15,252.88
Annual benefit per child (RMB, 2008)	314.74	273.94	355.54
PV of benefit per child (RMB, 2008)	1846.23	1606.90	2085.56
Panel B. Costs and Benefits (Billions of RMB, 2008)			
Costs			
Construction expenditure	16.55	16.55	16.55
Operating expenditure	5.15	5.15	5.15
Total costs	21.70	21.70	21.70
Benefits			
Total social benefits	74.84	65.14	84.54
Net Present Value (NPV)	53.14	43.44	62.78
Benefit-Cost Ratio (BCR)	3.45	3.00	3.90
Break-even year	27th	28th	26th
Internal Rate of Return (IRR, %)	9.29	8.80	9.72

Notes: All values are in RMB (2008 prices). The total population of children aged 0–5 (1997–2010) comes from the *China Statistical Yearbook* (1997–2010), and the county-level library coverage rate from the *China Library Yearbook* (2002–2015); their product gives the exposed population. The assumed working-life span of 22–62 years follows the *Chinese Human Capital Report 2024*[50]. Construction costs are computed by multiplying the national increase in library floor space (*China Library Yearbook*, 2002–2015) by the average construction cost for cultural facilities (*China Statistical Yearbook on Fixed Asset Investment*, 2009). Operating costs are derived from annual expenditure per m² in the

[50] See https://humancapital.cufe.edu.cn/en/hcip/hcp/a2024_Human_Capital_Report_Appendix_Chinese_.htm (in Chinese).

China Library Yearbook (2009). Total costs equal the sum of these two components. The internal rate of return (IRR) is the discount rate that equates the present values of benefits and costs.

Online Appendix

Palaces for the People: the long-term effect of public libraries on child development in China

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Appendix A. Auxiliary Materials



Figure A.1 Picture book area for children aged 0–2 years

Notes: Photographed at Nanshan District Library, Shenzhen, China. The area shown provides age-appropriate picture books and a low-shelf design to encourage visual access, tactile interaction, and early reading engagement among children aged 0–2. This facility exemplifies how public libraries under the NCIRSP have incorporated specialized spaces for infants and toddlers.



Figure A.2 Parent-child reading area

Notes: Photographed at Qingpu District Children's Library, Shanghai, China. The area integrates seating for both adults and children, creating a comfortable environment for shared reading activities and family participation in early literacy. This design illustrates public libraries' broader role in supporting intergenerational learning through dedicated parent–child reading zones.

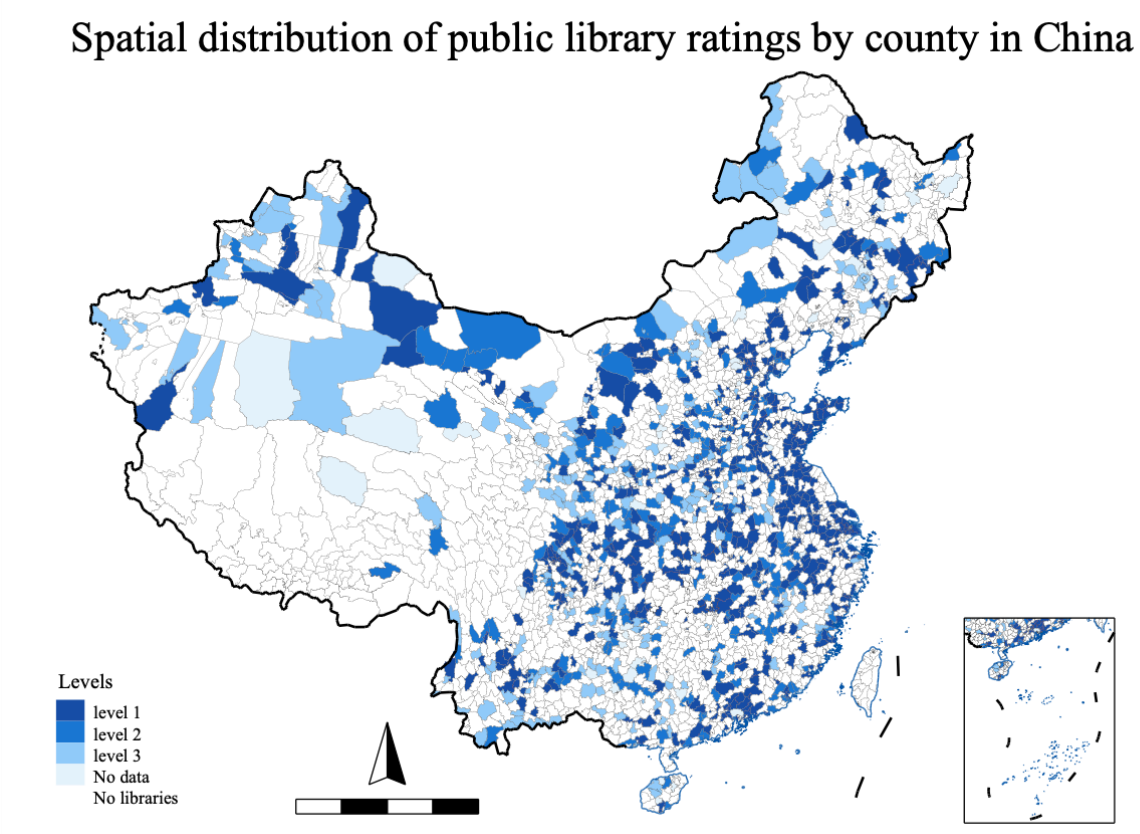


Figure A.3 Spatial distribution of public library ratings by county in China

Notes: The map shows the spatial variation in county-level public library ratings across China. Each color shade represents the official rating level assigned to county public libraries. Darker colors indicate higher-rated libraries. Counties without a public library are shown in light beige. Data are compiled from prefecture and county statistical yearbooks.

Table A.1 Determinants of public library rollout timing

	（1）	（2）
Dependent var.	Public Library Rollout Timing	
GDP	-0.087*	-0.075
	(0.050)	(0.074)
Log population	-0.348	-0.249
	(0.674)	(0.843)
Sex ratio	0.015	0.045
	(0.069)	(0.083)
Proportion of population aged 0–14	0.022	0.131
	(0.077)	(0.110)
Proportion of population aged 65 and above	-0.078	0.068
	(0.212)	(0.302)
Average years of education	-0.037	0.580
	(0.665)	(0.944)
Urbanization rate	-0.035	-0.064
	(0.032)	(0.043)
Mountainous county	-0.908	-0.393
	(0.668)	(0.725)
Number of hospital and clinic beds	0.001	0.001
	(0.001)	(0.001)
P-value of joint significance	0.482	0.267
Province fixed effects	No	Yes
Observations	205	205
R^2	0.042	0.151

Notes: The unit of observation is the county. The dependent variable is the year in which the county established a public library. Control variables are drawn from the 2000 Population Census and the 2001 China County Statistical Yearbook. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix B. Data Appendix

Table B.1 Overview of outcome variables in CFPS (Wave 2010–2020)

Outcome variable		Respondents					
		Wave 2010	Wave 2012	Wave 2014	Wave 2016	Wave 2018	Wave 2020
Cognitive test	Word test A score	⩾10	X	⩾10	X	⩾10	X
	Math test A score	⩾10	X	⩾10	X	⩾10	X
	Word test B score	X	⩾10	X	⩾10	X	⩾10
	Math test B score	X	⩾10	X	⩾10	X	⩾10
Noncognitive test		10	10;12;14	10- to 21-year-old individuals without historical records	10- to 23-year-old individuals without historical records	10- to 23-year-old individuals without historical records	X
Depression	6-item Kessler Psychological Distress Scale (K6)	⩾10	X	⩾10	X	X	X
	Center for Epidemiologic Studies Depression (CES-D)	X	⩾10	X	⩾10	⩾10	⩾10
Educational attainment		All	All	All	All	All	All

Notes: This table summarizes the temporal and age-specific availability of each outcome variable across CFPS waves 2010–2020. The numbers "10," "12," and "14" refer to data collected only for respondents aged exactly 10, 12, and 14 years, respectively, unless otherwise specified. "⩾10" indicates variables available for all respondents aged 10 or older. Entries marked "X" denote that the corresponding variable was not recorded or unavailable in that survey wave.

Table B.2 Noncognitive ability and corresponding details (Rosenberg Self-Esteem Scale)

How much do you agree with these statements from Rosenberg Self-Esteem Scale? 1. Totally disagree; 2. Disagree; 3. Neither agree nor disagree; 4. Agree; 5. Totally agree. Item scale=1 if "Agree" or "Totally agree"; 0 *otherwise. Scale for each item is averaged and then standardized to create the self-esteem index: the higher the score, the higher the individual's degree of approval toward himself.*

Item	Survey Question (Rosenberg Self-Esteem Scale)	Reverse Coded
1	I feel that I'm a person of worth, at least on an equal basis with others.	No
2	I feel that I have a number of good qualities.	No
3	All in all, I am inclined to feel that I am a failure.	Yes
4	I am able to do things as well as most other people.	No
5	I feel I do not have much to be proud of.	Yes
6	I take a positive attitude toward myself.	No
7	On the whole, I am satisfied with myself.	No
8	I wish I could have more respect for myself.	Yes
9	I certainly feel useless at times.	Yes
10	At times I think I am no good at all.	Yes

Table B.3 Depression and corresponding details (K6 & CES-D scales)

Variable	Survey question	Reverse Coded
The following are some descriptions of people's mental statuses. Please select according to your statuses in the past month. (0 = Never; 1 = Once a month; 2 = 2–3 times a month; 3 = 2–3 times a week; 4 = Almost every day). Depression = 1 if K6 Score ⩾ 13.		
6-item Kessler Psychological Distress Scale (K6) (Wave 2010 and 2014)	(1) Feel depressed and cannot cheer up no matter what you are doing.	No
	(2) Feel nervous.	No
	(3) Feel upset and cannot remain calm.	No
	(4) Feel hopeless about the future.	No
	(5) Feel that everything is difficult.	No
	(6) Think life is meaningless.	No
Here are some descriptions of people's mental statuses. Please select according to your statuses in the past week. 0 = Never; 1 = Sometimes (1–2 days); 2 = Often (3–4 days); 3 = Most of the time (5–7 days). Depression = 1 if 20-item CES-D Score ⩾ 16 or 8-item CES-D Score ⩾ 7.		
Center for Epidemiologic Studies Depression (CES-D) (Wave 2012, 2016, 2018, and 2020)	(1) I am worried about some trivial things.	No
	(2) I have a poor appetite and do not want to eat.	No
	(3) I feel depressed despite the help from relatives and friends.	No
	(4) I find myself not worse than others.	Yes
	(5) I cannot concentrate on things.	No
	(6) I am in a low spirit.	No
	(7) I find it difficult to do anything.	No
	(8) I find the future promising.	Yes
	(9) I feel that I have been a loser for a long time.	No
	(10) I feel scared.	No
	(11) I cannot sleep well.	No
	(12) I feel happy.	No
	(13) I talk less than usual.	No
	(14) I feel lonely.	No
	(15) I find that people are not friendly to me.	No
	(16) I have a happy life.	Yes
	(17) I cried or I want to cry.	No
	(18) I feel sad.	No
	(19) I find that others do not like me.	No
	(20) I feel that I cannot continue with my life.	No

Table B.4 Overview of mechanism variables in CFPS (Wave 2010–2020)

Mechanism variable	Respondents					
	Wave 2010	Wave 2012	Wave 2014	Wave 2016	Wave 2018	Wave 2020
Panel A. Educational investments						
Educational expenditure	All; parent-reported					
Time spent with child	All adults; matched to child information using household identifiers	X	X	X	X	All adults; matched to child information using household identifiers
Educational expectation	10-15 years; parent-reported			X		
Panel B. Parenting skills						
Parenting skills (including optimal parenting; affectionate constraint; affectionless control; neglectful parenting)	11	11; 13; 15	10-16 years (imputed)	X	X	Individuals enrolled in school prior to university
Panel C. Childhood family environment						
HOME	1-5 years; parent-reported; starting point determined by child's age				0-5 years; parent-reported	
Panel D. Children's learning outcomes						
Reading participation	X	X	⩾10	⩾10	⩾10	⩾10
Reading volume	X	X	⩾10	⩾10	⩾10	⩾10
Study time	⩾10	X	X	X	X	X
Academic ranking	X	⩾10	⩾10	⩾10	⩾10	⩾10

Notes: This table summarizes the temporal and age-specific availability of each mechanism variable across CFPS waves 2010–2020. The numbers "10–15," "11," and "11; 13; 15" refer to variables collected only for respondents aged exactly within those age ranges (for example, "11; 13; 15" indicates data collected from children aged 11, 13, and 15 years). "⩾10" means data are available for individuals aged 10 and above, while "0–5" or "1–5" denote variables reported by parents for children aged 0 to 5 or 1 to 5 years, respectively. "All" indicates that the question was asked to all respondents in the designated group, and "X" represents that the variable was not recorded or unavailable in that wave.

Table B.5 Parental bonding instrument items used to construct the measures of parenting styles (PBI scale)

Dimension	Survey question
Here are some descriptions of how parents/guardians treat their children. According to the actual situation last year, please select how your parents/guardians treated you. (1. Never; 2. Rarely; 3. Sometimes; 4. Often; 5. Always). Exploratory factor analysis (EFA) is used to identify items under each dimension.	
Care	(1) When you did something wrong, the parents/guardians would ask about reasons and talk with you about what you should do.
	(2) The parents/guardians encouraged you to do things with great effort.
	(3) The parents/guardians were gentle when talking to you.
	(4) The parents/guardians encouraged you to think independently.
	(5) The parents/guardians would tell you reasons when they asked you to do something.
	(6) The parents/guardians liked to talk with you.
	(7) The parents/guardians praised you.
Overprotection	(1) The parents/guardians asked about what happened to you at school.
	(2) The parents/guardians checked your homework.
	(3) The parents/guardians helped you with your schoolwork.
	(4) The parents/guardians told stories to you.
	(5) The parents/guardians played with you [for example, playing chess or playing outside].
	(6) The father/mother attended parent–teacher meetings at school.

Table B.6 Home observation for measurement of the environment (HOME) items used to construct the measures of childhood family environment (story, book, tour, literacy)

Dimension	Survey question
Here are some descriptions of the interaction between parents/guardians or family and their children. According to the actual situation last year, please select the frequency that best describes how often each activity occurred. (1. Several times a year or less than that; 2. Once a month; 3. Two or three times a month; 4. Several times a week; 5. Everyday). We apply a backfilling procedure: for each respondent, the reported frequencies at ages 3–5 are imputed to all subsequent observations in adolescence (ages 10–19). Scale for each item is averaged and then standardized to create home index: the higher the score, a more enriched childhood family environment.	
Story	(1) How often do you read for example, stories, to your child?
Book	(2) How often do you buy books for your child, such as picture books?
Tour	(3) How often do you or your family members take the child out to play, such as strolling in the park, going to the playground, shopping, or having a picnic?
Literacy	(4) How often do you or your family members use toys, games or other things to help the child learns characters?

Table B.7 Summary statistics of mechanism variables

Variables	Obs	Mean	SD
	(1)	(2)	(3)
Panel A. Educational investments			
Educational expenditure	16,532	3.417	14.393
Time spent with child	4,962	1.162	1.717
Educational expectation	9,638	16.657	2.916
Panel B. Parenting skills			
Optimal parenting	19,162	0.031	0.174
Affectionate constraint	19,162	0.905	0.293
Affectionless control	19,162	0.032	0.175
Neglectful parenting	19,162	0.032	0.175
Panel C. Childhood family environment			
Home index	3,450	-0.386	1.080
Story	3,450	2.288	1.690
Book	3,450	1.487	1.159
Tour	3,450	1.992	1.570
Literacy	3,450	2.289	1.693
Panel D. Children's learning outcomes			
Reading participation	6,163	1.094	1.121
Reading volume	4,605	8.179	17.207
Study time	3,746	1.033	2.510
Academic ranking	5,718	3.382	1.158

Notes: This table presents descriptive statistics for the mechanism variables used in the mediation analysis. The sample includes 8,764 children born between 1991 and 2010 from 160 counties, observed in six CFPS waves (2010–2020) when respondents were aged 10 to 19. Panels A to C use the full sample, and Panel D is restricted to children attending school at the time of the survey. Educational expenditure denotes annual household spending on children's education (hundred RMB), time spent with child measures daily hours parents devote to childcare, and educational expectation captures parents' desired years of schooling for their children. Parenting styles are derived from the Parental Bonding Instrument (PBI; Parker et al., 1979) following Li et al. (2023) and are defined in Table B.5. Early-childhood family environment measures follow the HOME inventory framework (Caldwell and Bradley, 1984) and are defined in Table B.6.

Appendix C. Other Robustness Checks

Although the preceding analysis demonstrates that access to public libraries significantly improves children's cognitive ability, noncognitive ability, and educational attainment, further corroboration is required to verify the robustness of these findings. In this section, we perform a series of additional robustness checks. Specifically, we (i) control for confounding effects of contemporaneous policies and investments; (ii) consider alternative empirical approaches, including propensity-score reweighting, stacked difference-in-differences (DID), and regression discontinuity designs; (iii) test the sensitivity of results to alternative fixed effects, clustering choices, and sample definitions; (iv) examine treatment-effect heterogeneity; (v) conduct sensitivity analyses of the parallel-trends assumption; (vi) perform placebo and permutation tests; and (vii) assess potential spatial spillover effects. The consistency of results across these tests reinforces the credibility of our baseline estimates.

C.1 Confounding from contemporaneous policies and investments

Children's developmental outcomes may also be affected by other policy initiatives implemented around the time public libraries expanded. To reduce concerns that our estimates capture these concurrent changes rather than library exposure, we explicitly account for a broad set of overlapping policy interventions. Our baseline specification in Equation (1) includes province-by-survey-year fixed effects τ_{pt}, which absorb time-varying province-specific shocks such as province-wide education or social policy changes. We therefore focus on more granular developments at the county and community levels, including (i) county-level education reforms, (ii) county-level expansions in health and cultural services, and (iii) community infrastructure improvements.

Education reform policies. We account for four major county-level education reforms implemented during the public library expansion period: the *Rural Education Informatization Reform*, the *School Merging Policy*, the *Kindergarten Expansion Reform*, and the *Two*

Exemptions and One Subsidy Program. For each reform, we construct cohort-specific exposure measures and add them as controls in our baseline regression. Panel A of Table C.1 reports the results after controlling for each education reform separately and jointly. The estimated effects of public library exposure remain robust across all specifications.

The *Rural Education Informatization Reform* introduced computer-assisted learning in rural primary schools between 2004 and 2007. By providing digital learning tools and internet access, this program may have improved cognitive development independently of public library access (Bianchi et al., 2022). If informatization coincided with public library construction in the same counties, our estimates could capture the combined effect of both interventions. To address this concern, we construct a binary indicator equal to 1 if an individual was born after 1999 and resided in a treated county at age five.

The *School Merging Policy* ("*Che dian bing xiao*"), launched in 2001, consolidated rural primary schools and teaching points to improve resource allocation and teaching quality. While this policy may have enhanced educational efficiency in some areas, it also increased travel distances for many rural students, potentially raising dropout rates and reducing educational attainment. If school mergers disproportionately affected counties that later received public libraries, our estimates could be biased downward. Following Song and Luo (2024), we construct policy intensity as the percentage decline in the number of primary schools in each county between 2000 and 2006, and interact this measure with an indicator for birth after 1995.

The *Kindergarten Expansion Reform*, implemented between 2003 and 2007, encouraged private sector participation in preschool provision. This reform substantially increased kindergarten enrollment in rural areas, which may have enhanced early childhood development through improved access to pre-primary education. Failing to control for this expansion could bias our estimates upward if public library construction and kindergarten growth occurred simultaneously in the same counties. Following the approach used for the *School Merging Policy*, we measure reform intensity as the percentage change in the number of kindergartens

in each county during 2003–2007 and interact this continuous measure with an indicator for birth after 1998.

The *Two Exemptions and One Subsidy* program was launched in 2001 to provide free textbooks, waive miscellaneous school fees, and subsidize boarding expenses for students in compulsory education. The program primarily targeted nationally designated poverty-stricken counties, many of which also received early library[1] construction. By alleviating the financial burden of schooling, this subsidy may have independently increased children's educational attainment (Shi, 2012; Wang, 2018), potentially leading our estimates of the effect of public libraries to be upward biased if not properly accounted for. To address this concern, we collect data on the list of national poverty-stricken counties from the *Poverty Monitoring Report of Rural China* published by the National Bureau of Statistics and construct a binary treatment indicator equal to 1 if an individual was born after 1996 and lived in a nationally designated poverty-stricken county at age five.

Health and cultural service expansions. We further account for concurrent policy initiatives that expanded health and cultural services at the county level. Specifically, we consider three major programs: the *New Cooperative Medical Scheme* (NCMS), the *National Fitness Plan*, and the construction of cultural venues such as theaters and cinemas. For each program, we construct measures of early-childhood exposure and include them as additional controls in the baseline specification. As reported in Panel B of Table C.1, the estimated effects of public library access remain positive and statistically significant across all specifications, indicating that the main findings are robust to the inclusion of these concurrent health and cultural service expansions.

The *NCMS*, China's rural health insurance program, was launched in 2003 and expanded rapidly across counties during the following decade. By reducing out-of-pocket medical expenses and improving access to healthcare, the NCMS may have enhanced child health and indirectly supported cognitive development and schooling (Huang and Liu, 2023). If the rollout

[1] Throughout this paper, the term "library" refers to public libraries; "library" and "public library" are used interchangeably.

of the NCMS overlapped with public library construction, our estimates could conflate the effects of the two initiatives. To address this concern, we construct a binary indicator equal to 1 if an individual was born after 1998 and resided in an NCMS-covered county at age five.

The second phase of the *National Fitness Plan* (2001–2010) fostered a substantial expansion of public sports infrastructure, including stadiums, gymnasiums, and community fitness centers. These facilities may have improved children's physical health and noncognitive skills by encouraging greater participation in sports and recreational activities (Barron, 2000). We construct a binary indicator equal to 1 if an individual was born after 1996 and lived in a county with newly developed sports facilities at age five.

Additionally, we account for exposure to other *cultural venues*, specifically theaters and cinemas, which were also expanding during this period. Such facilities provide access to films, performances, and cultural events that may complement public libraries in enriching children's cultural and cognitive environments. We construct a binary indicator equal to 1 if theaters or cinemas existed in the individual's county at age five.

Community infrastructure development. During the NCIRSP period, many rural communities experienced rapid improvements in basic infrastructure, including access to tap water, electricity, television, telephones, mobile signals, paved roads, and piped gas. These improvements may have enhanced child development through multiple channels: better sanitation and water quality could improve health (Chen et al., 2022; Ai and Tan, 2025), expanded access to television and telecommunications could provide educational content and information (Zha and Zhou, 2025; Zhang and Xu, 2025), and improved transportation could reduce school commuting time (Adukia et al., 2020). Failing to account for these changes could bias our estimates if infrastructure rollout coincided with public library construction.

Following Huang and Liu (2023), we use CFPS community survey data from 2010 and 2014 to construct early childhood exposure measures for seven types of community infrastructure: tap water, electricity, cable or satellite television, landline telephones, mobile signal coverage, public roads, and piped gas. For each infrastructure type, we calculate the

number of years between the individual's conception and age five during which the facility was available in their community, yielding exposure measures analogous to our public library exposure variable.

Panel C of Table C.1 shows that our baseline results remain unchanged after controlling early exposure for community infrastructure. The estimated effects of library exposure on cognitive ability, non-cognitive ability, and educational attainment are nearly identical to those in Table 3, both when infrastructure types are controlled separately and when all seven are included jointly. This robustness confirms that the observed effects reflect the impact of public libraries rather than concurrent improvements in basic community infrastructure.

Table C.1 Controlling for contemporaneous policies and public investments

	（1）	（2）	（3）	（4）
	Cognitive ability	Non-cognitive ability	Depression	Educational attainment
Panel A. Controlling for contemporaneous education reforms				
Rural education informatization reform	0.092***	0.091***	-0.003	0.073***
	(0.021)	(0.029)	(0.005)	(0.027)
School merging policy	0.095***	0.097***	-0.002	0.073***
	(0.021)	(0.029)	(0.006)	(0.024)
Kindergarten expansion reform	0.095***	0.093***	-0.002	0.066**
	(0.022)	(0.029)	(0.005)	(0.027)
Two exemptions and one subsidy	0.096***	0.098***	-0.003	0.067***
	(0.022)	(0.028)	(0.006)	(0.026)
Jointly controlling for all education reforms above	0.094***	0.081***	-0.005	0.074***
	(0.021)	(0.029)	(0.005)	(0.025)
Panel B. Controlling for health and cultural service expansions				
New cooperative medical scheme	0.098***	0.093***	-0.003	0.069***
	(0.021)	(0.029)	(0.005)	(0.026)
National fitness plan	0.100***	0.105***	-0.001	0.068**
	(0.020)	(0.030)	(0.006)	(0.026)
Cultural venues (theater/cinema)	0.100***	0.090***	-0.002	0.070***
	(0.019)	(0.029)	(0.006)	(0.026)
Jointly controlling for all health and cultural services above	0.104***	0.102***	-0.003	0.070***
	(0.019)	(0.030)	(0.006)	(0.025)
Panel C. Controlling for community infrastructure development				
Tap water	0.094***	0.097***	-0.002	0.068**

	(0.022)	(0.029)	(0.006)	(0.026)
Electricity	0.094***	0.098***	-0.002	0.068**
	(0.022)	(0.029)	(0.006)	(0.026)
Cable/satellite TV	0.094***	0.097***	-0.002	0.070***
	(0.021)	(0.029)	(0.006)	(0.026)
Landline phone	0.094***	0.096***	-0.002	0.068**
	(0.021)	(0.029)	(0.006)	(0.026)
Mobile signal	0.094***	0.097***	-0.002	0.073***
	(0.021)	(0.029)	(0.006)	(0.026)
Public road	0.094***	0.098***	-0.002	0.068***
	(0.021)	(0.029)	(0.006)	(0.026)
Piped gas	0.094***	0.098***	-0.002	0.068***
	(0.021)	(0.029)	(0.006)	(0.026)
Jointly controlling for all infrastructure measures above	0.094***	0.097***	-0.001	0.072***
	(0.022)	(0.028)	(0.006)	(0.026)

Notes: The sample includes individuals from birth cohorts 1991 to 2010 who were aged 10 to 19 in the CFPS waves from 2010 to 2020. Each estimate is from a separate regression of the outcome on the number of years between conception and age five during which a public library had been established in the county. Each row corresponds to a specification that controls for the contemporaneous policy listed in the first column. Individual and household controls include the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status. The county-level controls used to construct interactions with cohort fixed effects include GDP, population size, sex ratio, age structure, average years of education, urbanization rate, and the number of hospital and clinic beds. All regressions include county fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects. Standard errors are clustered at the county level. *, ** and *** represent statistical significance at the 10%, 5% and 1% levels, respectively.

C.2 Alternative empirical strategies

In the main analysis, we estimate the long-term impacts of public library expansion on child development outcomes using a DID framework. We address endogeneity concerns through baseline balancing tests and recentering procedures (Borusyak and Hull, 2023). To further test the robustness of our findings and mitigate potential identification issues, we complement the baseline DID results with a series of alternative empirical strategies. Specifically, we apply propensity score reweighting to adjust for differences in pre-treatment characteristics, implement a stacked DID approach to account for staggered treatment timing, exploit a regression discontinuity (RD) design to capture near-threshold comparisons in public

library construction, and re-estimate the models excluding counties that never established public libraries.

1. Propensity score reweighting

An alternative nonparametric method of ensuring balance between treated and control units is propensity score reweighting. Overlap weights have been shown to achieve exact balance on covariates using logistic propensity scores (Li et al., 2019). We calculate the propensity score *p* as the predicted probability that an individual is assigned to the treated group (i.e., exposed to the intervention before age 5), conditional on a rich set of baseline covariates measured prior to treatment exposure. Specifically, *p* is estimated via a logistic regression of the binary treatment indicator on a comprehensive set of baseline county-level characteristics measured in the year 2000: (1) GDP, (2) total population, (3) sex ratio, (4) proportion of population aged 0–14, (5) proportion of population aged 65 and above, (6) average years of education, (7) urbanization rate, (8) number of hospital and clinic beds, and (9) land area. Standard errors are clustered at the county level.

Following Rexer (2025), we construct overlap weights as $w_c = T_c(1-p)+(1-T_c)p$. where T_c denotes treatment assignment and *p* is the estimated propensity score. This weighting scheme places greater emphasis on observations with propensity scores near 0.5, thereby focusing on counties where treatment and control groups are most comparable, while downweighting units at the extremes of the propensity score distribution. Figure C.1 estimates the average difference between treatment and control groups across fixed covariates and initial conditions in the unweighted sample and with reweighting. The figure demonstrates that this approach substantially improves covariate balance across all nine county characteristics.

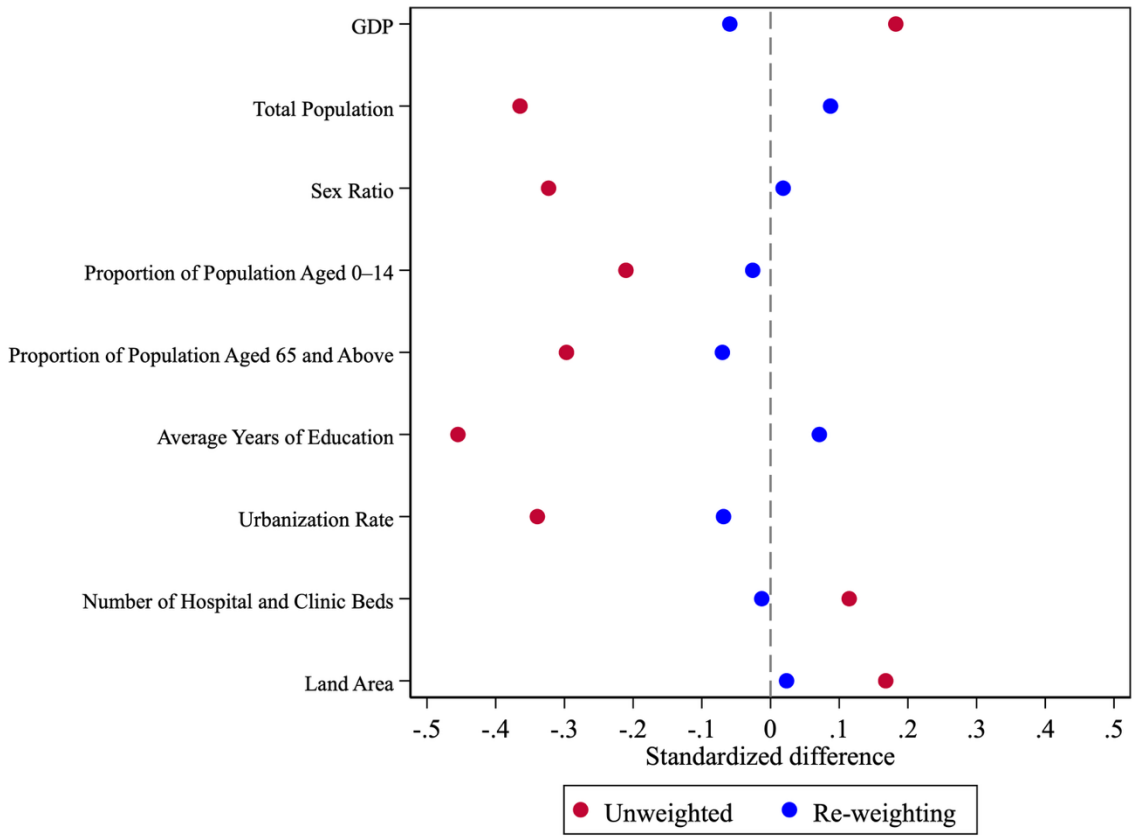


Figure C.1 Reweighting distribution

Notes: This figure plots standardized differences in baseline county characteristics between treated and control groups before and after applying reweighting. Red dots denote unweighted differences, and blue dots denote differences after reweighting. The reweighting procedure assigns each treated county a weight of $(1 - \hat{p})$ and each control county a weight of $\hat{p}$ to balance the distribution of covariates across groups.

Using the derived propensity scores, we re-estimate Equation (1) applying these overlap weights. Table C.2 presents the reweighted estimation results. Panel A reproduces the unweighted baseline estimates for comparison, while Panel B presents the overlap-weighted estimates. The coefficients retain their original signs, and most remain statistically significant, with point estimates broadly similar in magnitude to the baseline. Notably, the effect on depression becomes marginally significant under overlap weighting (−0.020, significant at the 10% level), suggesting enhanced precision when focusing on more comparable counties. These findings confirm that our main results are robust to propensity-score reweighting.

Table C.2 Propensity score re-weighting results

	（1）	（2）	（3）	（4）
	Cognitive ability	Non-cognitive ability	Depression	Educational attainment
Panel A. No weights				
Library exposure IU-5	0.094***	0.098***	-0.002	0.068***
	(0.021)	(0.029)	(0.006)	(0.026)
Observations	9,792	3,136	11,321	14,800
Panel B. Overlap weights				
Library exposure IU-5	0.063**	0.114**	-0.020*	0.063**
	(0.029)	(0.046)	(0.011)	(0.027)
Observations	4,675	1,432	5,363	7,082
Individual and household controls	Yes	Yes	Yes	Yes
County base controls × cohort FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Province × survey FE	Yes	Yes	Yes	Yes

Notes: Each column reports results from a separate regression based on Equation (1), estimated using propensity-score re-weighting. The sample includes individuals from birth cohorts 1991 to 2010 who were aged 10 to 19 in the CFPS waves from 2010 to 2020. Individual and household controls include the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status. The county-level controls used to construct interactions with cohort fixed effects include GDP, population size, sex ratio, age structure, average years of education, urbanization rate, and the number of hospital and clinic beds. All regressions include county fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects. Standard errors are clustered at the county level. *, ** and *** represent statistical significance at the 10%, 5% and 1% levels, respectively.

2. Stacked DID

A potential concern with our baseline specification is treatment-timing heterogeneity: because counties established public libraries in different years, children begin to be exposed at different ages. In settings with staggered adoption, conventional two-way fixed effects estimators can be biased due to "forbidden comparisons" that use already-treated units as controls (Goodman-Bacon, 2021). The stacked event-study approach addresses this concern by estimating cohort-specific effects and then aggregating them to obtain an overall treatment effect.

Following Campbell (2024), we implement the stacked DID estimator as follows. First, we identify treatment cohorts based on the year each county first established a public library. All counties that began treatment in the same year constitute a single cohort. Second, for each treatment cohort, we construct an independent subsample containing: (i) treated counties in that cohort, and (ii) control counties that either established public libraries more than 17 years later or never established public libraries during our sample period. Third, within each subsample, we define relative time e, where $e = 0$ denotes the year of adoption. Finally, we vertically stack all subsamples to form the analysis dataset.

In the stacked dataset, we estimate the following model:

$$Y_{icbet} = \alpha + \beta Exposure_{cb} + \gamma X_{icbet} + \eta_b CB_c + \delta_{ec} + \lambda_{eb} + \tau_{ept} + \varepsilon_{icbet} \quad \text{(C1)}$$

where Y_{icbet} denotes the outcome of individual i born in county c, cohort b, event time e, and observed in survey year t. It includes four measures of child development: cognitive ability, noncognitive ability, depression, and educational attainment. $Exposure_{cb}$ measures the total years of public library access from conception to age five in the child's birth county. X_{icbet} is a vector of individual and household characteristics, including gender, *hukou* status, parents' age, parental education, household size, and household income. The model includes event-by-county fixed effects (δ_{ec}), event-by-cohort fixed effects (λ_{eb}), and event-by-province-by-survey-year fixed effects (τ_{ept}). These saturated fixed effects account for all time-invariant differences across treatment cohorts, absorb cohort-specific trends, and control for province-level shocks that vary by event time and survey wave.

Table C.3 reports the stacked DID estimates, with standard errors clustered at the county level. The results closely mirror our baseline findings. The magnitudes are comparable to or slightly larger than the baseline estimates in Table 3, confirming that our main results are not driven by biases arising from staggered treatment adoption.

Table C.3 Stacked DID estimates

	（1）	（2）	（3）	（4）
	Cognitive ability	Non-cognitive ability	Depression	Educational attainment
Library exposure IU-5	0.099***	0.082**	0.001	0.084**
	(0.026)	(0.034)	(0.009)	(0.032)
Individual and household controls	Yes	Yes	Yes	Yes
County base controls × cohort FE	Yes	Yes	Yes	Yes
Event × county FE	Yes	Yes	Yes	Yes
Event × cohort FE	Yes	Yes	Yes	Yes
Event × province × survey FE	Yes	Yes	Yes	Yes
Observations	140,722	46,298	163,629	215,084

Notes: Each column reports results from a separate regression estimated using the stacked event-wise dataset. Individual and household controls include the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status. The county-level controls used to construct interactions with cohort fixed effects include GDP, population size, sex ratio, age structure, average years of education, urbanization rate, and the number of hospital and clinic beds. All regressions include event-by-county, event-by-cohort, and event-by-province-by-survey-wave fixed effects. Standard errors are clustered at the county level. *, ** and *** represent statistical significance at the 10%, 5% and 1% levels, respectively.

To further examine the heterogeneous effects by age at first exposure, we estimate an event study specification using the stacked dataset:

$$Y_{icbet} = \alpha + \sum_{k=-1}^{K} \beta_k D_{cb}^{k} + \gamma X_{icbet} + \eta_b CB_c + \delta_{ec} + \lambda_{eb} + \tau_{ept} + \varepsilon_{icbet} \quad \text{(C2)}$$

where D_{cb}^{k} .indicates that the child was first exposed to a public library at age *k*. Similar to the event-study specification in the main text (Equation 2), we group cohorts into two-year bins based on age at first exposure. The omitted category is children first exposed at ages six to seven, which serves as the reference group. We also include the same set of fixed effects and control variables as in Equation (C1).

Figure C.2 plots the event study coefficients from Equation (C2), revealing a clear increasing gradient from right to left across cognitive, noncognitive and educational outcomes. For depression (Panel C), the pattern shows small and statistically insignificant effects across

all age groups, consistent with our main findings. The stacked DID approach thus replicates our baseline results under an identification strategy robust to treatment timing heterogeneity.

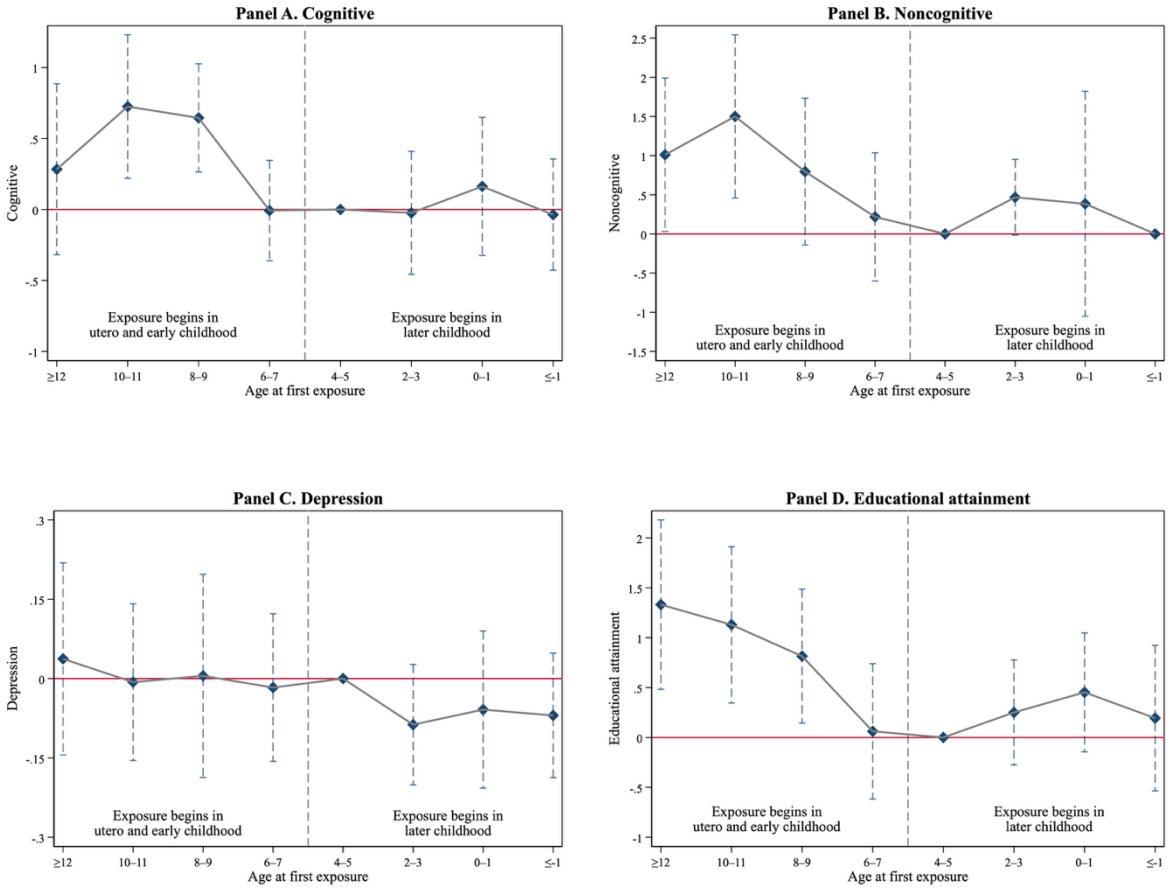


Figure C.2 Event study: effects of library exposure on child development using stacked DID

Notes: These figures illustrate the impacts of early-life public library exposure on child development outcomes. The outcome variables are indicated in the title of each panel. Each panel presents event-study coefficients estimated from Equation (2) using the stacked event wise dataset. The figure plots the estimated coefficients and the 90 percent confidence intervals from the main specification, where the key explanatory variables are categorical indicators for the age at which a county public library was first established. Age 6–7 serves as the omitted reference group. Negative event times indicate that exposure began in utero (i.e., full early-life exposure).

3. Regression discontinuity (RD)

While our stacked DID approach addresses treatment timing heterogeneity, two additional

concerns warrant further attention. First, the primary data on library establishment are recorded at the annual level because monthly information is unavailable for some counties, raising the possibility of measurement error in exposure timing. Second, to further refine causal identification, we complement the DID analysis with a regression-discontinuity (RD) design that exploits a sharp cutoff in early-childhood library exposure. To mitigate potential mismeasurement in the RD setting, we augment the annual dataset with available monthly establishment dates and impute missing months as June of the reported establishment year. The RD estimates therefore compare cohorts of children born immediately before and after a public library opened in their county, effectively contrasting otherwise similar individuals who differ only in their cumulative exposure during the critical early-childhood period.

Following Gray et al. (2023), we match each individual's birth month to the opening date of the nearest public library in their birth county and construct a running variable measuring the time distance between birth and public library establishment. We define the cutoff at 60 months (being exactly five years old at the time of library opening), comparing children born just before versus just after this threshold. Children born 60 or more months before public library opening received full exposure from birth to age five, while those born just after this cutoff received progressively less early childhood exposure. We employ a triangular kernel with a bandwidth of ±84 months around the cutoff. The RD model is specified as:

$$Y_{icmt} = \alpha + \beta D_{ic} + \delta_1 R_{ic} + \delta_2 (D_{ic} \times R_{ic}) + \gamma X_{icmt} + \theta_m + \mu_{pt} + \varepsilon_{icmt} \quad \text{(C3)}$$

where Y_{icmt} denotes the outcome for individual *i* born in county *c*, month *m*, and observed in survey year *t*. D_{ic} is the running variable measuring the number of months between the individual's birth and the library opening date. D_{ic} is an indicator equal to one if $R_{ic} \le 60$ (i.e., the individual was born no more than 60 months before the library opened, implying exposure to an operating public library during early childhood), and zero otherwise. The interaction term $D_{ic} \times R_{ic}$ allows the slope of the running variable to differ on either side of

the cutoff. X_{icmt} includes individual and household controls, θ_m denotes month-of-birth fixed effects, and μ_{pt}. represents province-by-survey-year fixed effects. Standard errors are clustered at the month-of-birth level.

Table C.4 reports the RD estimates. Public library exposure generates positive and statistically significant effects on cognitive ability and noncognitive ability. For educational attainment, the point estimate remains positive but loses statistical significance, suggesting some sensitivity. Consistent with our baseline results, the effect on depression remains statistically insignificant. The RD estimates for cognitive and noncognitive ability are somewhat larger than the baseline DID estimates, which is consistent with RD identifying a local average treatment effect for cohorts near the cutoff that may be more responsive to early-childhood inputs. Figure C.3 provides visual evidence of the discontinuity. Panels A and B show clear jumps in cognitive and noncognitive abilities at the threshold, while Panels C and D exhibit no discernible discontinuities. The graphical evidence confirms that children born just before the public library opened experience substantially better developmental outcomes than those born just after. These results demonstrate that our main findings are robust to an alternative identification strategy that relies solely on quasi-random variation in exposure timing near the discontinuity threshold.

Table C.4 Regression discontinuity (RD) estimates

	（1）	（2）	（3）	（4）
	Cognitive ability	Non-cognitive ability	Depression	Educational attainment
Library exposure IU-5	0.161**	0.257*	0.011	0.004
	(0.079)	(0.149)	(0.030)	(0.113)
Individual and household controls	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Province × survey FE	Yes	Yes	Yes	Yes
Observations	2,034	784	2,367	3,428

Notes: Each column reports results from a regression discontinuity (RD) design that compares individuals born just before and after the time when a public library had been established in the county. The running variable is the number of months between an individual's birth and the library's opening date, with a cutoff

at 60 months (five years of exposure). The estimation uses a triangular kernel and a bandwidth of ± 84 months around the cutoff. The model controls for month-of-birth fixed effects and province-by-survey-year fixed effects. Individual and household controls include the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status. Standard errors are clustered by month of birth. *, ** and *** represent statistical significance at the 10%, 5% and 1% levels, respectively.

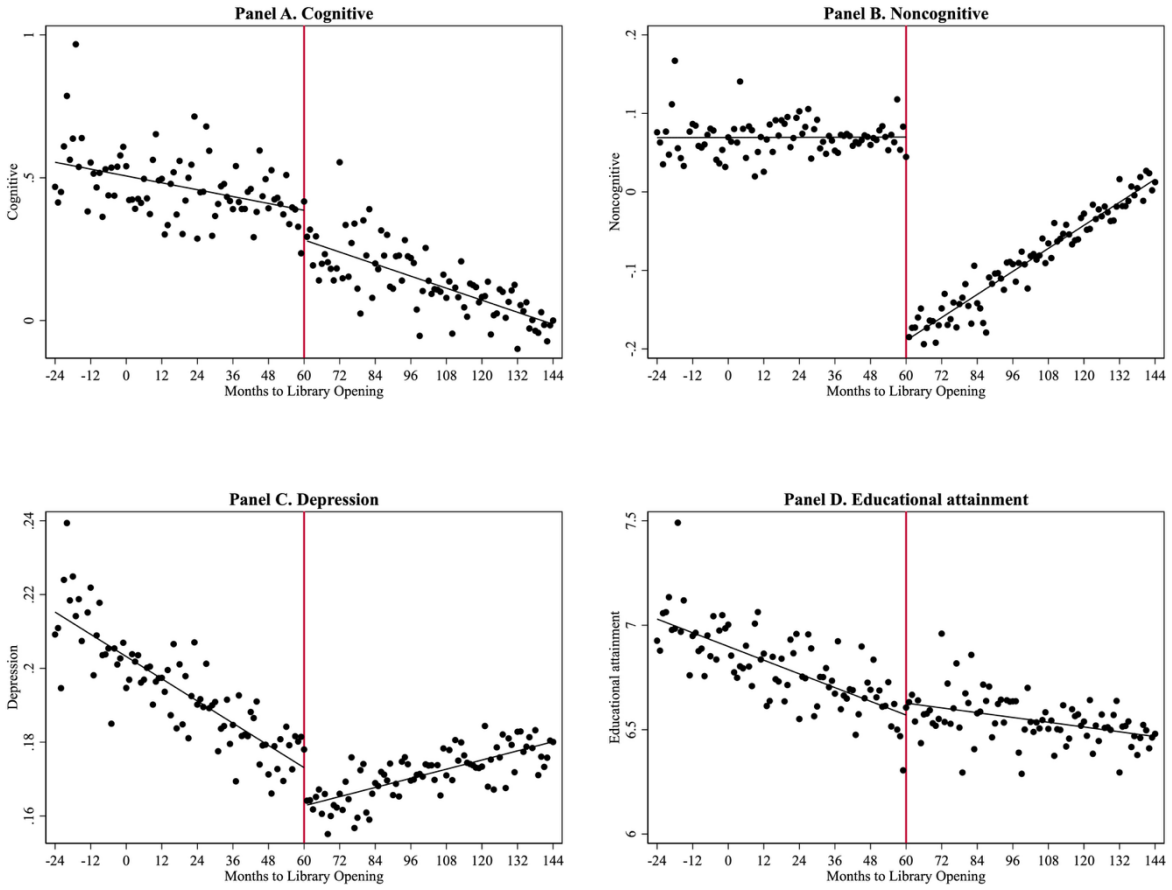


Figure C.3 Regression discontinuity (RD) estimates

Notes: Each panel plots local average relationships from a regression discontinuity (RD) design comparing individuals born just before and after the establishment of a public library in the county. The horizontal axis shows the number of months between an individual's birth and the library's opening date, with the vertical line indicating the cutoff at 60 months (five years after birth). Dots represent binned mean outcomes, and the fitted lines are local linear regressions estimated on either side of the cutoff.

4. Excluding counties that never built libraries

One may argue that identification can also be achieved by focusing solely on counties that established libraries before 2015, as this allows for contrasting individuals with varying years of exposure to public library access. However, excluding counties that never established libraries would result in a sample composed entirely of treated counties, which would reduce the variation in the treatment variable (early-life public library exposure). This reduction in variation could lead to lower estimation precision and may yield less robust results. Moreover, this approach would eliminate the control group of never-treated counties, potentially compromising the construction of credible counterfactuals and reducing the accuracy of our estimates.

In contrast, by including untreated counties in our analysis, both the sample size and the variation in the treatment variable are increased, thereby improving the precision of our estimates. This identification strategy is widely adopted in the existing literature, such as Chen et al. (2022) and Li and Xiao (2023). For example, Li and Xiao (2023) analyzed the effects of tap water access on child health in rural China by including both treated villages (those with access to tap water) and untreated villages (those without access) during the sample period.

Nevertheless, we conduct a robustness check by excluding counties that never established libraries. In this specification, identification is derived from the variation in the timing of public library establishment across counties, allowing us to exploit differences in timing within the treatment counties. The results in Table C.5 show that public library exposure continues to generate positive effects on cognitive ability (0.085, significant at the 1% level), noncognitive ability (0.215, significant at the 1% level) and educational attainment (0.127, significant at the 5% level), while the effect on depression remains close to zero and insignificant. These findings are consistent with our baseline results that include untreated counties, indicating that our main conclusions are robust to this alternative empirical strategy.

Table C.5 Excluding counties that never built libraries

	(1)	(2)	(3)	(4)

	Cognitive ability	Non-cognitive ability	Depression	Educational attainment
Library exposure IU-5	0.085***	0.215***	-0.006	0.127**
	(0.024)	(0.040)	(0.012)	(0.053)
Individual and household controls	Yes	Yes	Yes	Yes
County base controls × cohort FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Province × survey FE	Yes	Yes	Yes	Yes
Observations	3,334	979	3,817	4,944

Notes: Each column reports a separate regression based on Equation (1), excluding counties that never had public libraries. The sample includes individuals from birth cohorts 1991 to 2010 who were aged 10 to 19 in the CFPS waves from 2010 to 2020. Individual and household controls include the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status. The county-level controls used to construct interactions with cohort fixed effects include GDP, population size, sex ratio, age structure, average years of education, urbanization rate, and the number of hospital and clinic beds. All regressions include county fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects. Standard errors are clustered at the county level. *, ** and *** represent statistical significance at the 10%, 5% and 1% levels, respectively.

C.3 Additional specification checks

To further assess the robustness of our baseline findings, we conduct a series of additional specification checks examining alternative model specifications, sample restrictions, potential selection issues, and variable measurement choices. In the main text, we provided a specification curve analysis that systematically tested the robustness of our results across 288 different combinations of modeling choices, addressing concerns about specification sensitivity. Here, we present detailed results for each alternative specification and additionally examine the potential impact of sample selection and attrition. These robustness checks help establish that our findings reflect genuine causal effects rather than artifacts of specific empirical choices.

1. Alternative fixed effects, clustering and control variable sets

Our baseline specification in Equation (1) includes county fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects, with standard errors clustered at the county

level. We examine whether our results are robust to alternative choices along these dimensions. Table C.6 presents results from three sets of robustness checks.

In Panel A, we explore alternative fixed effects specifications. We first replace the baseline province-by-survey-wave fixed effects with survey-year fixed effects, which represent a less granular control for temporal variation across regions. The results remain consistent with our baseline findings. We then introduce county-by-survey-year interactions (in addition to the county and cohort fixed effects), which flexibly capture local, time-varying county-specific shocks. Under this richer specification, the estimates remain positive and significant for cognitive ability, noncognitive ability, and educational attainment.

Next, we examine the sensitivity of our inference to alternative clustered levels of standard errors in Panel B. We first cluster at the cohort level to account for potential serial correlation within county-cohort cells. Under this specification, the estimated effects on cognitive skills and educational attainment remain statistically significant, whereas the effect on noncognitive skills becomes somewhat sensitive to the clustering choice. We then cluster at the province level to allow for arbitrary correlation within provinces over time and across cohorts. The results continue to show positive and significant effects for cognitive ability, noncognitive ability, and educational attainment, confirming that our inference is not driven by the choice of clustering level.

Finally, we assess the robustness of our results to alternative control variable sets in Panel C. In the baseline specification, we include a full set of county-by-cohort fixed effects, which absorb any discrete cohort-level variation within counties. To test whether our results depend on this relatively strong form of control, we replace these fixed effects with county-by-linear cohort trends (as in Hoynes et al., 2016), allowing for smoother, gradual cohort-specific changes across counties. When including individual-level controls together with these county-by-linear cohort trends, the estimated effects remain positive and statistically significant for most outcomes: cognitive ability (0.085, $p < 0.01$), non-cognitive ability (0.072, $p < 0.05$), and educational attainment (0.063, $p < 0.01$), while the coefficient on depression remains small and

insignificant. These results suggest that our main findings are not driven by the functional form of county-level cohort controls or by observable individual characteristics. Together, these specification checks demonstrate that our main findings are not sensitive to alternative choices of fixed effects structures, clustered levels of standard errors, or control variable sets.

Table C.6 Alternative fixed effects, clustering and control variable sets

	（1）	（2）	（3）	（4）
	Cognitive ability	Non-cognitive ability	Depression	Educational attainment
Panel A. Alternative fixed effects specifications				
County FE, cohort FE, survey-year FE	0.101***	0.106***	0.001	0.064**
	(0.021)	(0.027)	(0.006)	(0.025)
Observations	9,797	3,140	11,323	14,801
County FE, cohort FE, county × survey-year FE	0.058**	0.159***	-0.011	0.133***
	(0.027)	(0.027)	(0.008)	(0.043)
Observations	9,771	3,082	11,307	14,796
Panel B. Alternative clustering levels				
Cohort standard error	0.094***	0.098	-0.002	0.068**
	(0.026)	(0.063)	(0.010)	(0.025)
Observations	9,792	3,136	11,321	14,800
Province standard error	0.094***	0.098***	-0.002	0.068**
	(0.018)	(0.022)	(0.005)	(0.027)
Observations	9,792	3,136	11,321	14,800
Panel C. Alternative control sets				
Individual and county-by-linear cohort trends	0.085***	0.072**	0.003	0.063***
	(0.024)	(0.027)	(0.007)	(0.024)
Observations	10,556	3,438	12,318	16,030

Notes: Each column reports results from a separate regression based on Equation (1). The sample includes individuals from birth cohorts 1991 to 2010 who were aged 10 to 19 in the CFPS waves from 2010 to 2020. Panel A applies alternative fixed effects specifications by replacing the baseline province-by-survey-wave fixed effects with survey-year fixed effects and further with county-by-survey-year fixed effects. All regressions in this panel include county fixed effects and cohort fixed effects, and standard errors are clustered at the county level. Panel B examines alternative levels of standard error clustering, including clustering at the cohort level and at the province level. All regressions include county fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects. Panel C evaluates robustness to alternative control variable specifications. The baseline includes individual controls and county-by-cohort fixed effects, while

the alternative replaces the latter with county-by-linear cohort trends. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

2. Sample selection

Our estimated effect would be biased if the sample of individuals used for analysis was not randomly selected. Thus, in this subsection, we consider four types of sample selection, which are potentially the most problematic: mortality selection, fertility selection, age restriction and migration behavior.

Mortality selection. In principle, the establishment of a county public library is unlikely to directly affect infant or child mortality. Unlike interventions such as vaccination campaigns, improvements in water and sanitation, or expansions in maternal and neonatal health care, libraries do not change the immediate disease environment or access to life-saving medical services, which are central determinants of early-life mortality (Mosley and Chen, 1984; Cutler and Miller, 2005). The main channels through which libraries plausibly operate, such as access to books and learning materials, information, and cognitively enriching environments, are more likely to influence human capital and socio-emotional development than short-run survival outcomes.

Because county-level infant and child mortality data are not consistently available in China for our period, we cannot directly test for mortality selection. Nonetheless, even if early-life library access reduced mortality through indirect mechanisms such as improved parental knowledge and information acquisition, the resulting selection would not overturn our main conclusions. If library access increased survival among more vulnerable children, the treated cohorts would be more negatively selected in terms of underlying health endowments. This would tend to attenuate estimated impacts on later outcomes, implying that our baseline estimates would, if anything, represent a lower bound on the true long-run effects of early-life library exposure.

To test for survival bias, we examine whether cohort sizes change with early-life public library exposure. If library access improved the survival of more vulnerable children through indirect channels, such as increasing parents' knowledge of hygiene and disease prevention, we would expect larger cohort sizes among cohorts with greater exposure. Collapsing the data to the county by birth year by survey year level, we regress the logarithm of cohort size on library exposure. Table C.7, Panel A reports no statistically significant effect of library exposure on cohort size. We also assess potential gender-biased survival by constructing the male-to-female cohort ratio at the same level of aggregation. Column (2) indicates that library exposure has negligible and statistically insignificant effects on sex ratios.

Fertility selection. A related concern is that library establishment could affect women's fertility decisions. For example, parents who value education and child development may delay childbearing until after a local public library opens, expecting improved learning resources and a more supportive early-childhood environment. In that case, the decision to have a child would be correlated with both treatment intensity and children's later outcomes, potentially biasing estimates based only on observed births. To address fertility selection, we follow Ferrara et al. (2012) and estimate whether the timing of births responds to county library openings. Using women's retrospective fertility histories, we construct an annual panel for each woman from 1991 to 2016 and estimate the following linear probability model:

$$F_{icpt} = \alpha + \sigma Library_{c,t-1} + \beta X_{icpt} + \delta_c + \lambda_t + \tau_{pt} + \eta_t CB_c + \varepsilon_{icpt} \quad \text{(C4)}$$

where i represents the woman, c represents the county, p represents the province, and t represents the year. F_{icpt} is defined as one if the woman gives birth to a living child in year t and zero otherwise. $Library_{c,t-1}$ is an indicator equal to one if county c established a public library in year t-1. The parameter of interest σ captures the effect of establishing a library at the county level on the probability of giving birth in the following year. X_{icpt} is a set of individual controls, including age in year t, years of education, and the quadratic terms of the

two variables. We also include county fixed effects δ_c, year fixed effects λ_t, and province-by-year fixed effects τ_{pt}, as well as a set of county characteristics that interacted with year fixed effects $\eta_t CB_c$.

In Panel B of Table C.7, we present the results separately for women whose fertility ages ranged from 15 to 29 and from 30 to 49. The estimates are close to zero and statistically insignificant in both subsamples, indicating that fertility timing does not respond to county library establishment. Overall, we find no evidence that fertility selection biases our estimates.

Table C.7 Survival bias and fertility selection

	(1)	(2)
Panel A: Survival bias		
Dependent variable:	Log (Cohort size)	Male to female cohort ratio
Library exposure IU-5	-0.022	0.006
	(0.017)	(0.040)
Observation	4,950	3,907
R^2	0.418	0.226
Panel B: Fertility selection		
Dependent variable:	Whether giving birth in year *t*	
Sample:	Fertility age 15-29	Fertility age 30-49
$Library_{t-1}$	-0.001	-0.002
	(0.000)	(0.001)
Observation	41,782	7,306
R^2	0.099	0.176

Notes: In Panel A, each column represents a separate regression of cohort size or ratio on exposure to library using Equation (1). We include all individual and county-level controls and fixed effects. In Panel B, each column represents a separate regression of whether a woman giving birth in year *t* on $Library_{t-1}$ (an indicator for the establishment of a library in county *c* in year *t*-1) with the sample restriction (shown as the column name) imposed. We present the results from the sample of women whose fertility ages range from 15 to 29 and 30 to 49. All regressions include county, year, and province-by-year fixed effects, individual characteristics in year *t*, and county controls interacted with year fixed effects. Standard errors in parentheses are clustered at the county level.

Age restrictions. Our baseline specification focuses on individuals aged 10 to 19 for several reasons. First, the nationwide public library construction program was launched in 2002 and concluded in 2015, and we utilize six waves of CFPS data from 2010 to 2020. Given this timing, the oldest individuals in our sample who were exposed to public libraries during early childhood are 19 years old. Second, the CFPS provides consistent measures of cognitive ability, noncognitive ability, and depression only for respondents aged 10 and above (see Table B.1), which naturally defines the lower bound of our analytic sample. To comprehensively examine child development outcomes while maximizing sample coverage, we restrict our baseline sample to ages 10-19.

However, one might be concerned that this age restriction could introduce bias if the effects of public library exposure vary systematically across different age groups or developmental stages. Younger children may still be in the formative period where public library exposure continues to shape their cognitive development, while older adolescents may have already experienced most of the cumulative effects. Alternatively, certain outcomes such as educational attainment may only become fully observable for older cohorts who have had more time to complete their schooling. To address these concerns, we examine the robustness of our findings to alternative age ranges.

In Panels A through D of Table C.8, we present results using progressively broader age ranges, restricting the sample to ages 10-17, 10-18, 10-20, and 10-21, respectively. Across all four samples, the estimated effects remain positive and statistically significant for cognitive ability, noncognitive ability, and educational attainment. This consistency demonstrates that our findings are not artifacts of the particular age cutoff employed in the baseline specification, and that the beneficial effects of early-life public library exposure remain detectable across alternative sample compositions.

Migration restrictions. While our baseline sample restrictions effectively address migration during childhood by leveraging the exogeneity of birth timing and the rigidity of China's *hukou* system during compulsory education, a potential concern remains regarding

post-childhood migration patterns. Individuals who experienced greater benefits from early public library exposure may possess enhanced human capital that facilitates geographic mobility toward urban centers or economically prosperous regions. If such selective out-migration systematically occurs among those most positively affected by public library access, and these migrants are excluded from our analytical sample, our estimates would suffer from sample selection bias. The direction of this bias is theoretically ambiguous: exclusion of high-ability migrants who benefited most from libraries would lead us to underestimate true treatment effects, whereas exclusion of lower-performing individuals seeking employment opportunities elsewhere could produce upward bias. We provide three complementary pieces of evidence demonstrating that migration-induced selection poses minimal threats to our inference.

First, the CFPS survey design incorporates methodological features specifically tailored to minimize migrant underrepresentation. The survey implements probability sampling through a residence-based mapping framework that substantially mitigates the challenge of separating individuals from their household units, thereby ensuring rigorous and representative household enumeration. Critically, household members who are temporarily absent or decline personal interviews remain part of the sampling frame provided they maintain economic linkages with their origin households. Core sociodemographic characteristics of these individuals are systematically recorded through proxy respondent interviews, substantially reducing information loss attributable to physical absence during survey administration.

Second, the CFPS implements a multidimensional approach to migrant identification that substantiates the validity of our sample construction.[2] Migration status is ascertained through three complementary instruments. (1) The individual-level questionnaire captures destination-based migration by comparing current residence against *hukou* registration location, which our baseline sample restriction addresses by requiring geographic correspondence between birthplace and current residence. (2) The household roster questionnaire adopts an origin-based

[2] Detailed documentation is available at: http://www.isss.pku.edu.cn/cfps/cjwt/cfpsxkt/1295323.htm (in Chinese).

perspective to identify out-migrants, capturing individuals who departed from their origin households—our requirement that respondents maintain *hukou* registration in their current county effectively addresses this dimension. (3) The age distribution in our analytical sample (10-19 years) stands in stark contrast to the mean age of migrants in the CFPS 2010 baseline (40.8 years), suggesting that post-treatment migration probability remains negligible within our study cohorts.

Third, we directly assess the empirical magnitude of potential migration-induced bias by re-estimating Equation (1) on the unrestricted sample that includes all migrants. Panel E of Table C.8 reports these results. The effects remain statistically significant and quantitatively comparable across cognitive, noncognitive and educational dimensions. To further ensure robustness, we construct a binary indicator for sample exclusion, which equals one for individuals excluded from the baseline sample due to migration restrictions and zero otherwise. We then regress this indicator on our treatment variable using the full sample. Panel B, Column (3) of Table C.9 shows that the coefficient on public library exposure is statistically insignificant, providing no evidence that early-life public library exposure systematically influences the probability of being excluded from our analytical sample due to migration.

Table C.8 Age and migration restrictions

	（1）	（2）	（3）	（4）
	Cognitive ability	Non-cognitive ability	Depression	Educational attainment
Panel A. Restricting to ages 10-17				
Library exposure IU-5	0.101***	0.072**	-0.004	0.069***
	(0.023)	(0.034)	(0.007)	(0.024)
Observations	7,965	2,779	9,019	12,437
Panel B. Restricting to ages 10-18				
Library exposure IU-5	0.092***	0.096***	-0.004	0.073***
	(0.023)	(0.031)	(0.006)	(0.026)
Observations	8,928	2,998	10,212	13,658
Panel C. Restricting to ages 10-20				
Library exposure IU-5	0.090***	0.079***	-0.003	0.080***

	(0.021)	(0.030)	(0.005)	(0.026)
Observations	10,683	3,387	12,493	15,997
Panel D. Restricting to ages 10-21				
Library exposure IU-5	0.095***	0.080***	-0.006	0.084***
	(0.021)	(0.028)	(0.005)	(0.026)
Observations	11,537	3,605	13,632	17,160
Panel E. Including migrant sample				
Library exposure IU-5	0.046*	0.078**	-0.001	0.056**
	(0.025)	(0.032)	(0.006)	(0.022)
Observations	11,001	3,632	12,738	16,852
Individual and household controls	Yes	Yes	Yes	Yes
County base controls × cohort FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Province × survey FE	Yes	Yes	Yes	Yes

Notes: Each column reports results from a separate regression based on Equation (1). Panel A to Panel D restrict the sample to individuals aged 10–17, 10–18, 10–20, and 10–21 in the CFPS waves, respectively. Panel E extends the sample to include migrant individuals who were excluded from the baseline estimation. Individual- and household-level controls include the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status. County-level controls used to construct interactions with cohort fixed effects include GDP, population size, sex ratio, age structure, average years of education, urbanization rate, and the number of hospital and clinic beds. All regressions include county fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

3. Sample attrition

Another potential threat to our main findings concerns bias arising from sample attrition. While the CFPS provides a nationally representative sample and employs telephone and online interviews to minimize attrition and nonresponse among mobile populations, some survey dropout inevitably occurs. If attrition correlates systematically with early-life public library exposure, our estimates could suffer from selection bias. The direction of such bias depends on whether individuals who benefited more (or less) from public library access are differentially likely to exit the sample. We conduct three complementary tests to assess whether nonrandom attrition poses a substantive concern.

First, we re-weight observations to account for differential retention rates across individuals. Following Zhao et al. (2025), we weight each observation by the inverse of the number of times an individual appears in the pooled sample, ensuring that the combined data accurately reflect the overall sample distribution over the entire pooled period. This weighting scheme down-weights repeatedly observed respondents while up-weighting those who appear less frequently, thereby correcting for potential overrepresentation of consistently retained individuals. Panel A of Table C.9 presents results using this inverse-frequency weighting approach, suggesting that differential retention does not meaningfully bias our main results.

Next, we directly examine whether early-life public library exposure predicts sample attrition at critical developmental junctures. Following Huang and Liu (2023), we focus on two key nodes: age 10, when cognitive and noncognitive measures first become available in CFPS, and age 16, when adolescents transition to either employment or senior secondary education. Using the subsample of children aged 4-9 in CFPS 2010, we regress an indicator for leaving the sample before age 10 on our public library exposure measure, controlling for the same individual, household, and county-level covariates as well as the full set of fixed effects employed in Equation (1). In Panel B of Table C.9, Column (1) shows that there is no evidence that early library access predicts attrition by age 10. Similarly, using children aged 10-15 in CFPS 2010, we examine whether library exposure predicts dropout before age 16. Column (2) reveals an insignificant coefficient, again providing no support for differential attrition at this critical transition point.

Table C.9 Attrition bias tests

	(1)	(2)	(3)	(4)
Panel A. Re-estimation of main outcomes with retention weights				
Dep. var.	Cognitive ability	Non-cognitive ability	Depression	Educational attainment
Library exposure IU-5	0.110***	0.098***	-0.009*	0.060**
	(0.020)	(0.029)	(0.005)	(0.025)
Observations	9,792	3,136	11,321	14,800

Panel B. Attrition regressions based on constructed dropout indicators

Sample wave	CFPS 2010		CFPS 2010–2020	
Dep. var.	Leaving the sample before age 10	Leaving the sample before age 16	Not living in county of birth	
Library exposure IU-5	0.004	-0.034	-0.002	
	(0.008)	(0.050)	(0.008)	
Observations	2,943	6,553	17,090	
Individual and household controls	Yes	Yes	Yes	Yes
County base controls × cohort FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Province × survey FE	Yes	Yes	Yes	Yes

Notes: Each column reports results from a separate regression testing for potential attrition bias. In Panel A, the sample are weighted by the inverse of an individual's number of observations. In Panel B, the dependent variable of in each specification is an indicator for whether the individual dropped out of the CFPS sample under different definitions of attrition. Column (1) uses children aged 0–5 in CFPS 2010 and defines attrition as leaving the sample before age 10. Column (2) uses children aged 10–15 in CFPS 2010 and defines attrition as leaving the sample before age 16. Column (3) uses an indicator for whether the individual no longer lived in their county of birth by 2020, capturing out-migration. All regressions include individual- and household-level controls (the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status). County-level controls used to construct interactions with cohort fixed effects include GDP, population size, sex ratio, age structure, average years of education, urbanization rate, and the number of hospital and clinic beds. All regressions include county fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

4. Measurement of variables

To ensure that our findings do not depend on specific measurement choices, we examine robustness to alternative specifications of both the treatment variable and outcome variables.

Alternative measures of public library exposure. Our baseline specification employs a continuous measure of library exposure (Library exposure IU-5) that counts the number of years between conception and age 5 during which a child had access to a public library in their county. Due to data limitation, we assume that public libraries opened at the beginning of the

year in which construction was completed. We assess sensitivity to this measurement approach along several dimensions.

First, we narrow the exposure window by one year, using Library exposure IU-4 to capture access from conception through age 4. Second, we vary the assumed timing of library openings within the reported establishment year by constructing exposure measures that assume implementation at mid-year or at year-end, rather than at the beginning of the year. Third, we replace the continuous exposure measure with a binary indicator equal to one if a child experienced any library exposure between conception and age 5. Table C.9 reports the results, and we find the estimated effects remain positive, and the results for cognitive ability, noncognitive ability, and educational attainment are highly consistent in magnitude and statistically significant across all specifications. The stability of the estimates across these alternative exposure measures indicates that our findings are not sensitive to how treatment intensity is defined or to assumptions about the timing of public library availability.

Table C.10 Alternative measures of library exposure

	（1）	（2）	（3）	（4）
	Cognitive ability	Non-cognitive ability	Depression	Educational attainment
Library exposure IU-4	0.092***	0.089**	-0.005	0.078***
	(0.025)	(0.039)	(0.007)	(0.028)
Library exposure IU-5	0.091***	0.091***	-0.001	0.066***
(Mid-year implementation)	(0.021)	(0.032)	(0.006)	(0.024)
Library exposure IU-5	0.078***	0.074**	-0.004	0.066***
(Year-end implementation)	(0.022)	(0.034)	(0.007)	(0.024)
Library exposure IU-5	0.348***	0.395***	0.012	0.224*
(Binary)	(0.067)	(0.098)	(0.020)	(0.116)
Individual and household controls	Yes	Yes	Yes	Yes
County base controls × cohort FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Province × survey FE	Yes	Yes	Yes	Yes

Observations	9,792	3,136	11,321	14,796

Notes: Each column reports results from a separate regression based on Equation (1). Four alternative measures of library exposure are used, as indicated by the row labels: *IU-4*, *IU-5 (mid-year implementation)*, *IU-5 (year-end implementation)*, and *IU-5 (binary)*. All regressions include individual- and household-level controls (the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status), as well as county baseline controls interacted with cohort fixed effects. County fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects are also included. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Alternative measures of outcome variables. We further assess whether our results are sensitive to how child development outcomes are measured. For cognitive ability, the baseline specification uses standardized test scores, and we alternatively use the raw (unstandardized) score. For noncognitive ability, we replace the Rosenberg Self-Esteem Scale (RSES) with the Locus of Control (LOC) scale, which captures beliefs about the extent to which individuals can control events that affect them (Rotter, 1966). Following Li et al. (2023), we construct a standardized LOC index from the relevant questionnaire items. The LOC items are listed in Table C.12. For depression, we move from a binary indicator based on clinical cutoffs to the standardized continuous CES-D score. Educational attainment is recoded from years of schooling into indicators for completing primary school, middle school, and high school.

Table C.11 presents the estimates using these alternative outcome measures. The coefficients retain their expected signs and remain statistically significant in nearly all specifications. The results indicate that our conclusions are not driven by particular measurement choices. The beneficial effects of early-life public library access appear consistently across multiple operationalizations of child development outcomes.

Table C.11 Alternative measures of outcome variables

(1)	(2)	(3)	(4)	(5)	(6)
Cognitive ability	Non-cognitive ability	Depression	Educational attainment		

	(Original score)	(locus of control)	(continuous)	(Primary school)	(Middle school)	(High school)
Library exposure IU-5	0.067***	0.309***	0.015	0.012***	0.004	0.000
	(0.017)	(0.116)	(0.027)	(0.004)	(0.003)	(0.001)
Individual and household controls	Yes	Yes	Yes	Yes	Yes	Yes
County base controls × cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × survey FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,792	2,904	11,368	14,800	14,800	14,800

Notes: Each column reports results from a separate regression based on Equation (1). Alternative measures of outcome variables are used, as indicated by the column labels. All regressions include individual- and household-level controls (the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status), as well as county baseline controls interacted with cohort fixed effects. County fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects are also included. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table C.12 Alternative noncognitive ability measurement (Locus of Control Scale)

How much do you agree with these statements from Nowicki–Strickland Locus of Control Scale for Children? 1. Totally disagree; 2. Disagree; 3. Neither agree nor disagree; 4. Agree; 5. Totally agree. Item scale=1 if "Agree" or "Totally agree"; 0 otherwise. Scale for each item is re-scored in the internal direction and then averaged and standardized to create the internal–external locus of control index: the higher the score, the more internal the individual.

Item	Survey Question (Locus of Control Scale)	Reverse Coded
1	One of my main goals in life is to make my parents be proud of me.	No
2	I pursue my own goals rather than following others.	No
3	I will try my best to make friends like me.	No
4	I decide my own life goals.	No
5	If I decide to do something, I will complete it no matter what.	No
6	Some children are born to be lucky.	Yes
7	Don't spend too much time striving since it will never prove effective.	Yes
8	It is nearly impossible to correct a mistake once you make one.	Yes
9	The best way to deal with a problem is not to think about it.	Yes
10	If something bad is about to happen, then it will happen no matter what you do.	Yes
11	I believe that planning in advance helps me to do things better.	No

12	Sometimes I think I am forced to do things due to my hard life.	Yes
13	I don't think I can solve the difficulties I am now facing by myself.	Yes
14	I can control things that happen to me.	No

C.4 Heterogeneous treatment effects

Recent research demonstrates that two-way fixed effects (TWFE) regressions can produce misleading estimates in settings with staggered treatment adoption and heterogeneous treatment effects (De Chaisemartin and D'Haultfoeuille, 2020; Sun and Abraham, 2021). The core issue arises because TWFE estimators use already-treated units as controls for newly treated units, which can generate negative weights on some treatment effect estimates. When treatment effects vary across cohorts or over time, these negative weights can distort the average treatment effect and even produce sign reversals.

To address this concern, we re-estimate our event study specifications using the heterogeneity-robust estimators proposed by Clarke and Tapia-Schythe (2021) and Sun and Abraham (2021). The Clarke and Tapia-Schythe's estimator provides an alternative approach that accounts for treatment effect heterogeneity in staggered adoption designs. The Sun and Abraham's estimator computes cohort-specific average treatment effects and then aggregates them using weights that ensure all components receive positive weight. We compare these estimators with our baseline OLS specification to assess whether our event study results are sensitive to the presence of heterogeneous treatment effects. Figure C.4 presents the results, demonstrating that our event study findings are robust to concerns about heterogeneous treatment effects in staggered adoption designs.

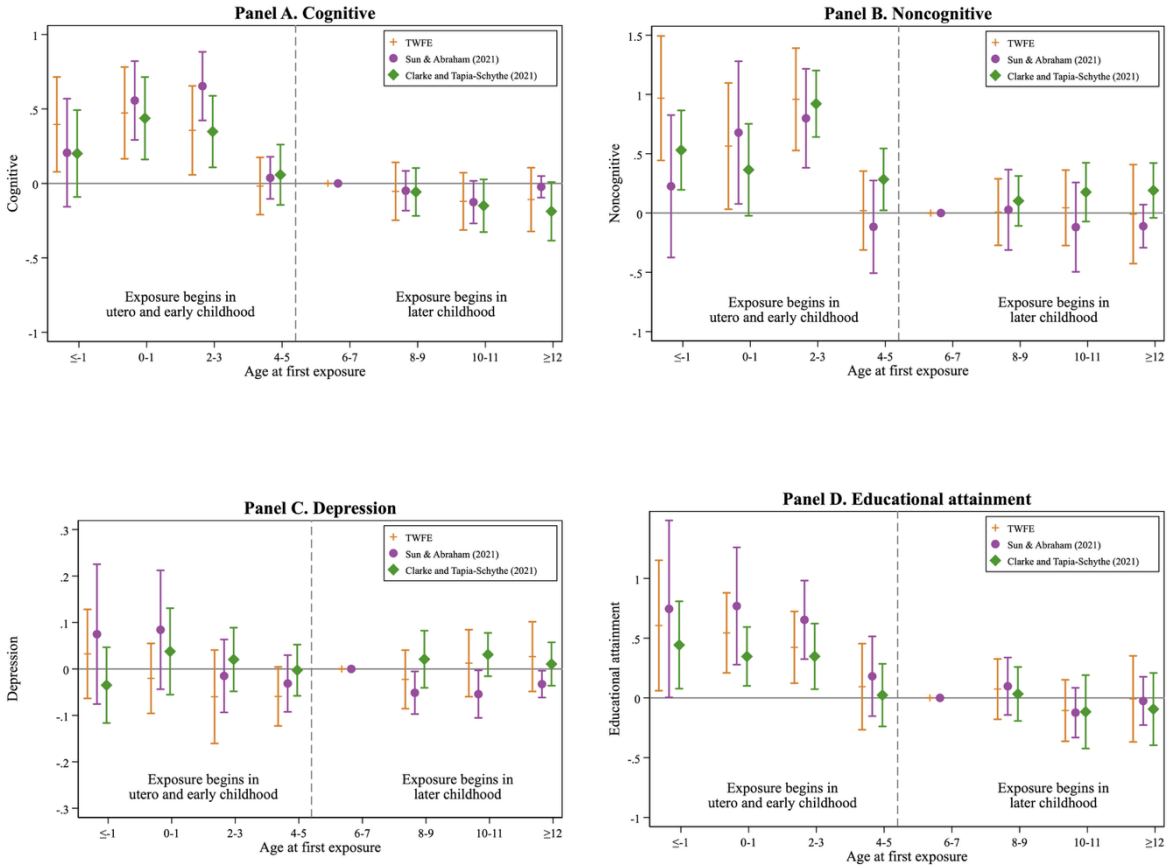


Figure C.4 The heterogeneity robust DID estimates

Notes: These figures overlay the event-study plots constructed using the estimator proposed by Sun and Abraham (2021) and Clarke and Tapia-Schythe (2021). The outcome variables are shown in the title of each subgraph. Each panel presents event-study coefficients estimated from Equation (2) using OLS. The sample includes individuals from birth cohorts 1991 to 2010 who were aged 10 to 19 in the CFPS waves from 2010 to 2020. The figure plots the estimated coefficients and the 90 percent confidence intervals from the main specification, where the key explanatory variables are categorical indicators for the age at which a county public library was first established. Age 6–7 serves as the omitted reference group. Negative event times indicate that exposure began in utero (i.e., full early-life exposure).

C.5 Sensitivity analysis of parallel trends assumption

While our event study results show no evidence of differential pre-trends (Figure 5), we formally assess the robustness of our estimates to potential violations of the parallel trends assumption. We apply the Honest Difference-in-Differences framework developed by

Rambachan and Roth (2023), which allows researchers to construct robust confidence sets under varying assumptions about the magnitude of possible deviations from parallel trends.

We implement the relative magnitudes approach, which bounds post-treatment violations of parallel trends relative to the maximum pre-treatment violation. Specifically, $\bar{M}$ represents the maximum allowable magnitude of trend violations in the post-treatment period as a multiple of the largest violation observed in the pre-treatment period. When $\bar{M} = 0$, post-treatment trends are restricted to follow the same pattern as pre-treatment trends. As $\bar{M}$ increases, we permit larger deviations from parallel trends in the post-treatment window. For each value of $\bar{M}$, we construct robust confidence intervals for the average treatment effect across the first four post-treatment periods.

Figure C.5 presents the sensitivity analysis results. For cognitive ability (Panel A), the confidence intervals exclude zero as $\bar{M}$ ranges from 0 to 0.3. For noncognitive ability (Panel B), statistical significance persists as $\bar{M}$ extends to 0.7. For educational attainment (Panel D), the estimates remain significant for $\bar{M}$ values up to 0.16. These results demonstrate that our findings are robust to plausible violations of parallel trends, with treatment effects remaining statistically significant even when allowing post-treatment deviations up to 30%, 70%, and 16% of the maximum pre-treatment violation, respectively. In contrast, for depression (Panel C), the sensitivity curve intersects the x-axis, indicating that the estimated effect becomes indistinguishable from zero even under minimal deviations from parallel trends. This pattern mirrors our baseline findings, suggesting no statistically significant treatment effect on depressive outcomes.

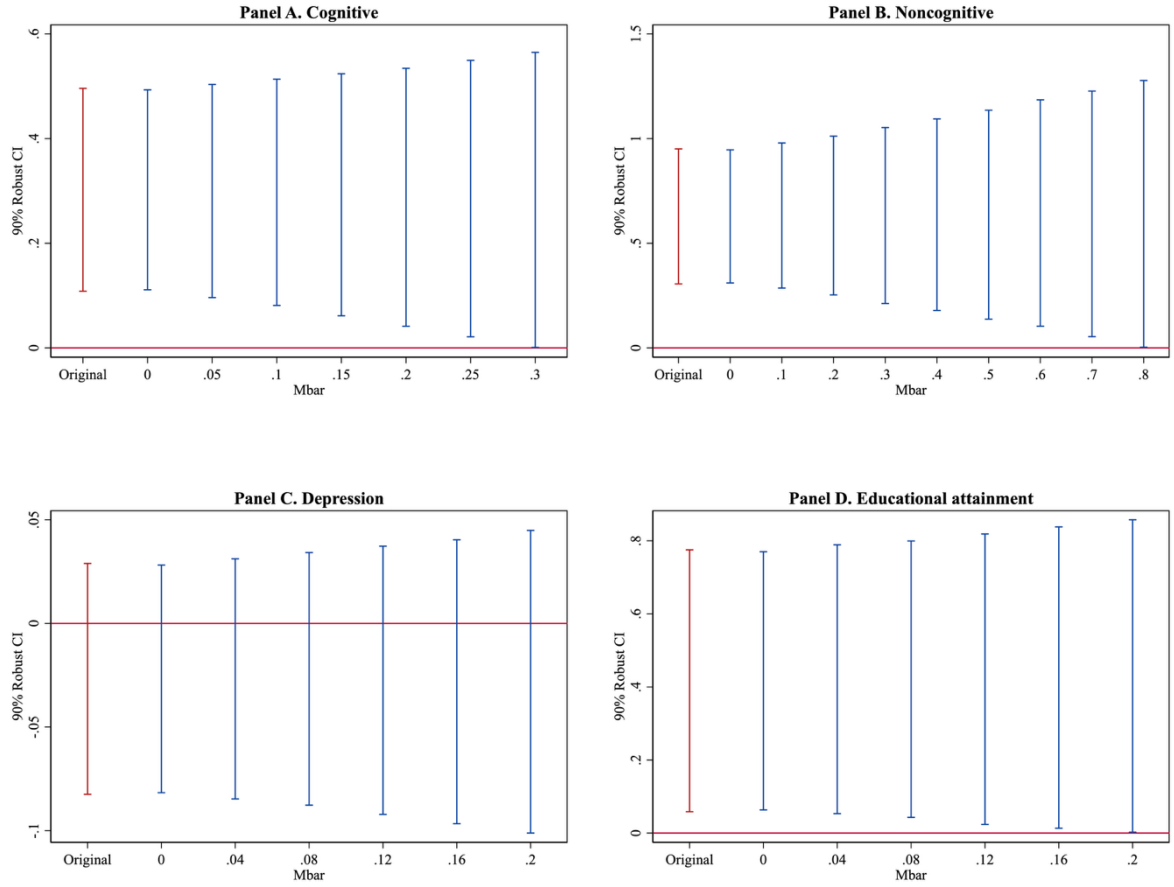


Figure C.5 Honest DID estimates

Notes: This figure plots 90% robust confidence intervals of the estimated coefficients for post-treatment periods under the honest DID framework. The outcome variables are shown in the title of each subgraph. The horizontal axis shows the value of $\bar{M}$, which represents the maximum degree of deviation from the parallel-trends assumption allowed in the post-treatment period relative to the largest violation in the pre-treatment period.

C.6 Placebo and permutation tests

1. Time-shifted placebo test

To further validate the parallel trends assumption, we conduct a time-shifted placebo test that examines whether our results could be driven by pre-existing differences in developmental trajectories across cohorts. Following the approach of Huang and Liu (2022) and Zhao et al. (2025), we artificially shift the timing of library establishment backward by 7, 8, 9, and 10 years relative to the actual implementation dates in each county. We then construct "placebo"

exposure indicators analogous to our main treatment measures but applied to individuals aged 20-29 at the time of survey. If our baseline results reflect genuine library effects rather than spurious cohort-specific trends, the placebo exposure indicators should yield null estimates.

Figure C.6 presents the results from this time-shifted placebo analysis. Each panel displays estimated coefficients and 95% confidence intervals for placebo exposure measures under different time shifts, with all specifications including the full set of covariates from Equation (1). Across all four outcome variables, the placebo estimates are consistently small in magnitude and statistically insignificant, providing strong support for the parallel trends assumption.

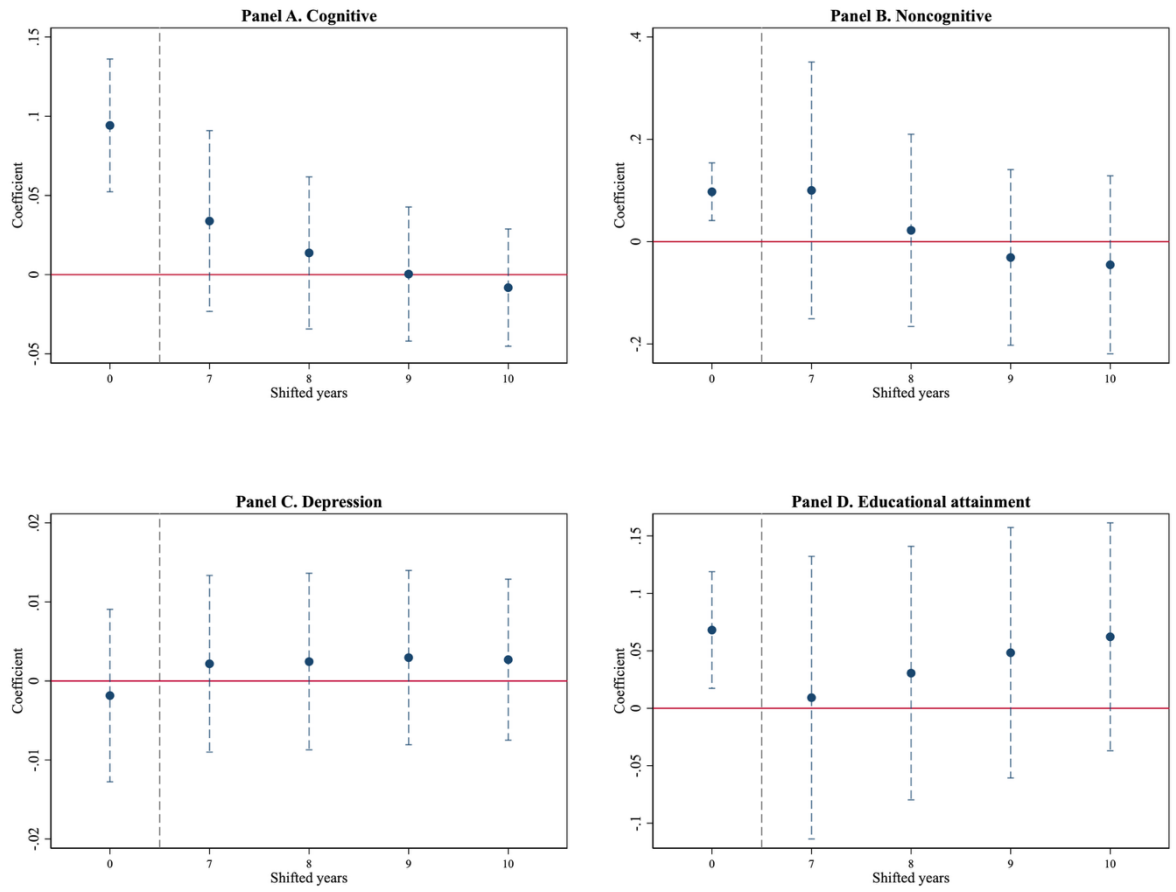


Figure C.6 Time-shifted placebo test

Notes: This figure presents results from the time-shifted placebo test. In each specification, we assume that the public library reform occurred 7, 8, 9, or 10 years earlier than its actual implementation year in each county. The outcome variables are shown in the title of each subgraph. The points plot the estimated coefficients of the placebo exposure variable, constructed analogously to the main exposure measure for the 20–29 age group. The vertical bars represent 95 percent confidence intervals based on standard errors clustered at the county level. All regressions include the full set of covariates described in Equation (1).

2. Permutation test

To verify the validity of our statistical inference, we implement a non-parametric permutation test that circumvents parametric assumptions about the error structure. This approach addresses potential concerns regarding serial and spatial correlation in our county-level panel data. We randomly select 89 counties from our 160-county sample as placebo treated units, matching the actual number of treated counties. Each placebo county receives a randomly assigned fictitious establishment year, with the yearly distribution of placebo public libraries mirroring the actual implementation pattern. We construct placebo exposure measures based on these random assignments and re-estimate our baseline specifications for all four outcomes. Repeating this procedure 1,000 times generates an empirical distribution of placebo effects under the null hypothesis of no treatment effect. If early-life public library exposure genuinely affects children's development, the actual coefficient should occupy the tail of the placebo distribution.

Figure C.7 presents the permutation test results. Each panel displays the distribution of 1,000 placebo coefficients, with the vertical dashed blue line marking the actual estimate from Table 3. For cognitive ability (Panel A), noncognitive ability (Panel B), and educational attainment (Panel C), the actual coefficients lie in the extreme tails of their respective placebo distributions, with implied p-values closely aligned with those from conventional clustered inference. For depression (Panel D), consistent with our insignificant baseline result, the actual estimate falls within the central mass of the placebo distribution. These results confirm that the observed improvements in child development outcomes reflect genuine causal impacts rather than statistical artifacts.

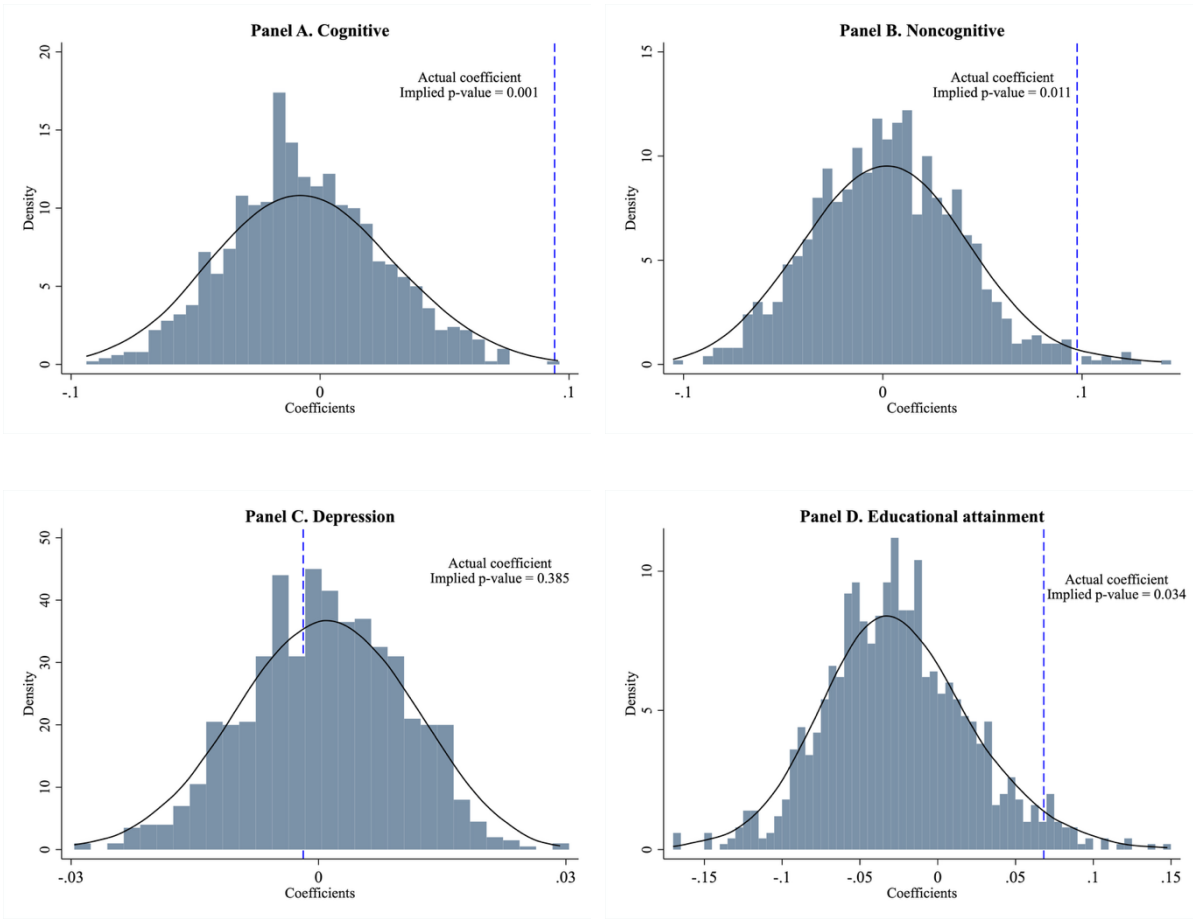


Figure C.7 Permutation placebo test

Notes: These figures show the distribution of placebo treatment effects of early-life exposure to public libraries on years of education from 1,000 random assignments of the year a county first established a public library. The vertical dashed line indicates the main estimate reported in Table 3. The implied p-value is the proportion of placebo estimates that are greater than or equal to the main estimate.

C.7 Spillover effects

A potential concern is that public library establishment in one county may generate spillover effects on children in neighboring counties. Geographic spillovers could arise if families in untreated counties travel across borders to access library services, or if knowledge diffusion occurs through social networks spanning county boundaries. Such spillovers would bias our estimates: positive spillovers to control counties would attenuate our treatment effects

toward zero, while negative spillovers (resource displacement) could inflate them. Following Zhao et al. (2025), we implement two complementary tests to assess whether cross-county spillovers threaten our inference.

First, we employ **a ring-based design**. We partition counties into treated counties (those establishing public libraries during 2002–2015) and exploited counties (those never establishing public libraries). For each exploited county, we calculate the shortest distance to the nearest treated county and construct concentric rings at 50, 75, and 100 kilometers around treated county centroids. We estimate:

$$Y_{icbt} = \alpha + \beta_1 Exposure_{cb}^{exploited} + \beta_2 Exposure_{cb}^{actual} + \gamma X_{icbt} + \eta_b CB_c + \delta_c + \lambda_b + \tau_{pt} + \varepsilon_{icbt} \quad \text{(C5)}$$

where $Exposure_{cb}^{exploited}$ measures placebo exposure for children in exploited counties. If an exploited county falls within a specified distance ring around a treated county, this variable equals the years between birth and age five during which the nearest treated county's library operated; otherwise, it equals zero. For treated counties, $Exposure_{cb}^{exploited}$ equals zero. $Exposure_{cb}^{exploited}$ captures true public library exposure: zero for exploited counties and actual early-life exposure for treated counties. Columns (1)-(3) of Table C.13 report results for distance thresholds of 50, 75, and 100 kilometers. Apart from a weakly significant effect on cognitive ability at the 50 km threshold, the estimated spillover effects are consistently small and statistically insignificant across all outcomes, indicating negligible cross-county spillovers.

Second, we implement **a border-based design** focusing on counties sharing administrative boundaries. We restrict exploited counties to those bordering at least one treated county. For these border-adjacent counties, $Exposure_{cb}^{exploited}$ equals the years of early-life exposure calculated from the public library establishment timing in the adjacent treated county. If an exploited county borders multiple treated counties, we use the earliest library opening date among all adjacent counties. Exploited counties not bordering any treated county receive zero. Column (4) of Table C.13 shows that the border-based spillover estimate is negligible

and statistically insignificant for all outcomes except educational attainment, where the marginally significant coefficient (−0.033, p<0.10) is economically small and negative, suggesting minimal spillover effects.

Table C.13 Spillover effects

	(1)	(2)	(3)	(4)
	50 km	75 km	100 km	Border
Panel A. Cognitive ability				
Exploited library	-0.054*	0.002	0.001	-0.039
	(0.031)	(0.030)	(0.030)	(0.025)
Observations	9,792	9,792	9,792	9,792
Panel B. Noncognitive ability				
Exploited library	-0.018	-0.032	-0.016	-0.028
	(0.029)	(0.022)	(0.022)	(0.036)
Observations	3,136	3,136	3,136	3,136
Panel C. Depression				
Exploited library	0.003	-0.000	-0.001	0.009
	(0.006)	(0.005)	(0.005)	(0.006)
Observations	11,321	11,321	11,321	11,321
Panel D. Educational attainment				
Exploited library	-0.032	0.037	0.032	-0.033*
	(0.026)	(0.033)	(0.033)	(0.018)
Observations	14,800	14,800	14,800	14,800
Individual and household controls	Yes	Yes	Yes	Yes
County base controls × cohort FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Province × survey FE	Yes	Yes	Yes	Yes

Notes: Each column reports results from a separate regression testing for potential cross-county spillover effects of library establishment. Following Zhao et al. (2026), we apply two identification strategies. Columns (1)–(3) use a ring-based design with distance bands of 50, 75, and 100 kilometers around treated counties to test for effects on nearby untreated areas. Column (4) employs a border-based design that creates a placebo exposure measure for counties adjacent to treated ones, while controlling for actual treatment status. Individual- and household-level controls include the child's age, sex, parental education, parental age, household size, total household income, and agricultural *hukou* status. County-level controls used to

construct interactions with cohort fixed effects include GDP, population size, sex ratio, age structure, average years of education, urbanization rate, and the number of hospital and clinic beds. All regressions include county fixed effects, cohort fixed effects, and province-by-survey-wave fixed effects. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix D. Contribution to the Literature

Table D.1 Estimated effects compared to similar public infrastructure interventions

(1) Country	(2) Intervention/policy	(3) Dep. var	(4) Effect size	(5) Relevant Population	(6) Empirical Design	(7) Source
Panel A. Years of Schooling						
Indonesia	INPRES primary school construction program (1973–1978)	Years of schooling	+0.035 years	Men born 1950–1972 (fully exposed cohort: born 1968–1972)	DID using cohort-district variation in exposure intensity	Duflo (2001)
India	Rural paved road construction under PMGSY (2001–2015)	Years of schooling	+0.07 years	Students in grades 6–8 in previously unconnected rural villages	Fuzzy RD design exploiting population eligibility thresholds for road access	Adukia et al. (2020)
China	Public health insurance expansion under the New Cooperative Medical Scheme (NCMS) (2003–2008)	Years of schooling	+0.17 year	Rural children aged 10–19 (born in 1991-2007)	DID exploiting county-by-cohort variation in early-life exposure (in utero to age 5)	Huang and Liu (2023)
Panel B. Cognitive and Non-Cognitive Skills						
U.S.	Public library capital investment (2010-2017)	Reading test scores	+0.01 to +0.04 SD	Students in grades 3–8 in school districts near the invested libraries	DID at school-district-grade level; exposure defined as ≥$200 per student investment	Gilpin et al. (2024)
U.S.	School spending (capital & non-capital, 1965-2015)	Math & reading test scores (average)	+0.032 SD/$1,000	K–12 students nationwide	Meta-analysis of 31 quasi-experimental studies (IV, RD, DID)	Jackson and Mackevicius (2024)
China	NCMS Expansion (2003–2008)	Cognitive skills (Chinese test scores)	+0.054 SD	/	/	Huang and Liu (2023)
China	Rural tap water infrastructure expansion under the Rural Drinking Water Program (1980s–2010)	Cognitive skills (avg. math & Chinese scores)	+0.132 SD	Rural children aged 10–15 (born in 1995–2000)	DID exploiting cohort-community variation in tap water connection timing	Chen et al. (2022)

Notes: All reported effect magnitudes are converted to represent the impact of one additional year of exposure, ensuring comparability with the baseline specification in our study. For multi-year interventions, we assume linear accumulation of treatment effects across exposure years, expressing educational outcomes in years of schooling and cognitive outcomes in standard-deviation (SD) units. The estimate for Duflo (2001) is derived by linearly scaling the coefficient difference reported in Table 5, where the impact of school construction rises from –0.035 (0 years exposed) to 0.14 (10 years exposed). This implies an average gain of 0.0175 years of schooling per exposure year per 1,000 children; when multiplied by the mean exposure intensity (1.98 schools per 1,000 children), the resulting effect equals approximately 0.035 years of schooling per year of exposure. For Adukia et al. (2019), the main coefficient in Table 2 indicates that rural road connection increases middle-school enrollment by 7 percent over three years. Assuming linearity and a typical three-year schooling cycle, this corresponds to a total gain of 0.21 years of schooling (= 0.07 × 3), or around 0.07 years of schooling per year of road exposure. For Huang and Liu (2023), full early-life exposure (from conception through age five, a six-year window) to the NCMS increases total schooling by 1.041 years and Chinese test scores by 0.321 SD; dividing these coefficients by the six-year exposure period yields incremental effects of $1.041/6 \approx 0.17$ years of schooling and $0.321/6 \approx 0.05$ SD per additional year of program exposure.

Table D.2 Benefit-cost ratios (BCR) compared to similar public infrastructure investments

(1) Country	(2) Intervention/policy	(3) BCR	(4) Source
U.S.	Public library capital investment (2002-2004)	1: 1.23	Bhatt (2010)
U.S.	HighScope Perry Preschool Project (1962–1967)	1: 6	García et al. (2023)
Indonesia	INPRES primary school construction program (1973–1978)	1: 10.89	Akresh et al. (2023)
China	Public health insurance expansion under the New Cooperative Medical Scheme (NCMS) (2003–2008)	1: 0.54	Huang and Liu (2023)

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