

Trade Liberalization and Mental Health: Evidence from China *

Bilge Erten[†] Pinar Keskin[‡] Rodrigo Pinto[§] Huihua Xie[¶] and Lianming Zhu^{||}

January 13, 2026

Abstract

Mental health conditions pose serious challenges to children's ability to learn and regulate their emotions, directly impacting their human capital development, with depressive disorders being among the most common. We leverage a change in international trade policy to examine how early childhood economic shocks influence adolescent mental health, with a specific focus on the prevalence of depression among teenagers in China. The United States granted China permanent normal trade relations (PNTR) in 2002, which substantially increased access of Chinese producers to U.S. market. Exploiting variation in tariff uncertainty faced by Chinese regions pre-2002, we find that the affected regions experienced a relative increase in average income per worker and structural transformation after the policy change. Children born in more exposed regions experience a significant relative improvement in their mental health outcomes during adolescence. We elucidate several potential mechanisms, including parental income, early childhood investments, and at-home care provision. We further show that selective migration, parental absence, or fertility decline are unlikely to account for the changes we document. Our findings highlight the importance of potential mental health improvements stemming from early exposure to trade reforms that expand access to global markets.

JEL Codes: F16, I15, J13, C21

*We thank Achyuta Adhvaryu, David Atkin, Prashant Bharadwaj, Kristin Butcher, Jianfei Cao, Randall P. Ellis, Martin Fiszbein, Jonathan Gruber, Brian Kovak, Jetson Leder-Luis, Brian McCaig, Dilip Mookherjee and seminar participants at Aarhus University, Boston University, Deakin University, the ILO Brown Bag Lunch Series, the Society of the Economics of the Household (SEHO) Conference at UCL, University of Melbourne, University of New Hampshire, Wellesley College, and Wilfrid Laurier University for their comments and suggestions. Any errors are our own.

[†]Northeastern University and IZA. Department of Economics, 312A Lake Hall, 360 Huntington Avenue, Boston, MA 02115. E-mail: b.erten@northeastern.edu

[‡]Wellesley College and IZA. Department of Economics, Pendleton East, 106 Central Street, Wellesley, MA 02481. E-mail: pinar.keskin@wellesley.edu

[§]University of South Florida, NBER and HCEO. Department of Economics, 4202 E. Fowler Ave, CMC 342 Tampa, FL 33620. E-mail: rodrigopinto@usf.edu

[¶]Zhejiang University. School of Economics, 866 Yuhangtang Rd., Xihu District, Hangzhou 310058, China. E-mail: huihuaxie@zju.edu.cn

^{||}Osaka University. Institute of Social and Economic Research, 6-1 Mihogaoka, Ibaraki, Osaka 567-0047, Japan. E-mail: lianmingzhu@iser.osaka-u.ac.jp

1 Introduction

Mental health conditions present serious challenges to children's and adolescents' abilities to learn, behave, and regulate their emotions, directly impacting their human capital development. Depressive disorders are among the most common health conditions, with approximately 20 percent of adolescents experiencing at least one major depressive episode ([National Institute of Mental Health 2022](#)).¹ Mental disorders are more prevalent among adolescents in low- and middle-income countries ([World Health Organization 2019](#)).² In particular, the incidence of adolescent depression in China has increased since the early 2000s and currently affects 20-22 percent of its youth ([Chinese Academy of Sciences 2020](#); [Li et al. 2019](#)).³ Despite substantial literature documenting the impacts of early-life economic shocks on later-life outcomes,⁴ little is known about the impact of economic shocks on adolescent mental illness. This paper uses a change in international trade policy to investigate how these economic shocks in early childhood affect adolescent mental health, and, in particular, the incidence of depression among teenagers in the context of China.

In January 2002, the United States passed a bill granting China permanent normal trade relations (PNTR). This trade liberalization increased the access of Chinese regions to US markets. Before PNTR, Chinese exports were subject to low normal trade relations (NTR) tariff rates. Still, these rates required annual renewal from the US Congress, creating considerable uncertainty as to whether Chinese exports would be subject to higher non-NTR rates reserved for nonmarket economies if the renewal failed ([Pierce and Schott 2016](#)). The passage of PNTR eliminated this uncertainty and consequently increased the access of Chinese firms to the US market.

The economic impact of the PNTR bill varied across Chinese counties depending on the industry composition of the local economy ([Erten and Leight 2021](#)). The Chinese counties whose industries were more exposed to reductions in tariff uncertainty experienced increased employment in manufacturing and service sectors and decreased agricultural activities. Exposed counties also benefited from a rise in local GDP, increased exports, and higher foreign direct investment. However, the aftermath of economic shocks on mental health is understudied. Specifically, it is unclear

¹According to World Health Organization (WHO) estimates, depression is projected to become the leading cause of global disease by 2030 ([Mathers and Loncar 2006](#)). Depressed adolescents are more likely to perform poorly at school, have impaired social relationships, have substance abuse problems, and experience disability and premature death ([Keenan-Miller et al. 2007](#); [Fletcher 2010](#); [Thapar et al. 2012](#)).

²The economic losses from depressive disorders are estimated to generate losses of 50 million years lived with disability, more than 80 percent of which correspond to low- and middle-income countries ([World Health Organization 2017](#)).

³Consequently, the National Health Commission of China released the first action plan for the prevention and control of depression among adolescents and other vulnerable groups in 2020 ([National Health Commission of China 2020](#); [Li et al. 2021](#)).

⁴[Almond et al. \(2018a\)](#) provide an overview of the recent literature on early-life conditions and adult health. [Heckman \(2012a\)](#) provides a developmental approach to health focusing on the costs and benefits of interventions over the life cycle. [Currie \(2020\)](#) reviews empirical studies on childhood mental conditions and their long-term consequences.

whether economic shocks in early childhood affect the incidence of common mental illnesses, such as depression, during adolescence.⁵

Economic shocks resulting from trade liberalization have a substantial impact on labor markets. Changes in parental employment and wages, particularly during the early stages of the child's life, can significantly influence adolescent mental health outcomes through several channels, including household income and time allocation. For example, parents residing in areas that benefit from trade liberalization may experience an increase in their income, allowing them to make more investments in early childhood. These early life investments may include better childcare provision, more breastfeeding time, more vaccinations, and better nutritional intake. Higher earning opportunities for male workers may allow their female partners to work less and spend more time with their children during their early years, potentially improving their mental health during adolescence.

We use a nationally representative Chinese household survey to examine how changes in early-life circumstances induced by trade liberalization affect mental health outcomes in adolescence. We focus on early-life exposure to PNTR. Our analysis is motivated by extensive literature that shows significant and persistent effects of early childhood interventions on health and economic outcomes at later ages (Heckman 2006, 2007, 2012a; Almond and Currie 2011; Almond et al. 2018a).⁶ Our primary data source is the China Family Panel Studies (CFPS), a nationally representative survey of Chinese families, and our primary outcome is the Epidemiologic Depression Scale (CES-D8), a measure of depression internationally validated for use in nonclinical settings. Geographical regions are determined by prefectures, which are the second administrative division of China below provinces.

We implement a generalized difference-in-differences (DiD) identification strategy to examine whether the mental health outcomes of cohorts born in prefectures more exposed to PNTR differ from the same cohorts born in less exposed prefectures. We include controls for the initial demographic and economic characteristics of prefectures where children were born, other trade policy changes, and fixed effects that absorb time-invariant attributes of prefectures of birth and aggregate shocks affecting all prefectures in a given year of birth.

⁵Studies from the psychology literature indicate that the onset of major depressive disorder (MDD) typically occurs during adolescence (Wilson et al. 2015; Kessler et al. 2005; Costello et al. 2003). Longitudinal studies following adolescents through adulthood such as the Oregon Adolescent Depression Project document that adolescents with MDD experience worse outcomes than unaffected youths in relationship quality, school and work functioning, and physical health, as well as greater psychiatric comorbidity and suicidality during adulthood (Rohde et al. 2013; Marmorstein et al. 2014; Hammen et al. 2008). This suggests that the benefits of the early treatment of depression during adolescence are critical for the psychological well-being of individuals in adulthood.

⁶The evidence from neuroscience literature indicates that brain development in the first years of life plays a crucial role in mental disorders given the presence of greater plasticity and neurogenesis. For example, total brain volume doubles in the first year of life while this increase decreases to 15 percent by the second year (Knickmeyer et al. 2008). Moreover, childhood experiences immediately after birth shape neural circuits in the brain that mediate socioemotional behaviors more than any other later-life period (Knudsen et al. 2006).

We find that cohorts born in prefectures more exposed to the trade liberalization policy of PNTR experienced a significant relative improvement in their mental health during adolescence. We show that these declines took place for the cohorts born after the policy change, with estimates implying that an interquartile shift in prefectures' exposure to PNTR in infancy is associated with a 0.19 standard deviation improvement in adolescent mental health. Our findings also show that these improvements in mental health result from a reduction in the incidence of severe depression. We find null effects of the policy change on the incidence of mild depression. The reduction in severe but not mild depression suggests nonlinear responses in both measurement and physiology: mild symptoms are transient and noisily measured, whereas chronic depressive states, linked to sustained stress activation, respond more sharply to improvements in early-life environments (Kessler et al. 2003a; McEwen 2007; Heckman 2012b).

We examine three potential channels that could explain our findings: parental income and employment, early-life investments, and migration and fertility responses. We use the China Statistical Yearbook data to document that average income per worker increases in prefectures more exposed to PNTR. Next, we use data from the China Population Census to examine the effects of the trade policy change on employment status outcomes. Our findings show that women living in prefectures more exposed to PNTR policy experienced a relative decline in their labor force participation. In contrast, men in more affected regions did not experience a significant change in their labor force participation while they shifted from agriculture toward manufacturing and services. We then explore the consequences of these parental income and employment changes on early life investments in children.

Using China Health and Nutrition Survey (CHNS), we find that children living in prefectures more exposed to the PNTR policy have benefited from improved early-life investments. These children experienced longer breastfeeding, higher protein intake, and increased prenatal visits and vaccinations. We also find a significant decline in the number of days and hours these children are cared for by people outside the household, suggesting an increased mother-child interaction.⁷ Finally, our findings are not explained by factors such as selective migration, parental absence, or fertility decline in response to the policy change.

Our empirical analysis contributes to several strands of literature. First, it adds to a growing body of research on the economics of child and adolescent mental health. Recent evidence has demonstrated that mental health conditions that emerge during adolescence have long-lasting effects (Currie and Stabile 2006, 2009; Currie et al. 2010). For example, Currie et al. (2010) uses

⁷While we document clear improvements in early-life nutrition and nurturing following PNTR exposure, these gains do not translate into detectable changes in cognitive or physical outcomes. This pattern aligns with evidence that socioemotional and mental health outcomes are more sensitive to early environmental conditions than cognitive indicators, whose developmental trajectories allow for later remediation through schooling and compensatory inputs (Zhou and Wang 2023; Almond et al. 2018b; Conti et al. 2016).

Canadian Data to compare children with mental health disorders to their siblings. They find that attention deficit disorder at early ages substantially increases the probability of going on welfare in adulthood. [Currie \(2020\)](#) further highlights the need to focus on the mental health of the “missing middle” years of adolescence, which have been significantly understudied. Our work contributes to filling this gap in the mental literature by examining the impacts of early childhood conditions on adolescent mental health outcomes.

A closely related recent paper to our work is [Gao et al. \(2024\)](#), which focuses on rural residents who migrated to cities most impacted by China’s WTO accession. Their findings show that migrant parents were more likely to leave daughters behind in rural areas, which leads to a range of negative outcomes for their daughters, including worse mental health. Our findings complement their study by focusing on residents in regions directly exposed to the trade policy shock, where new economic opportunities arose. For this reason, we find no significant changes in the likelihood of parents leaving their children behind. Instead, the positive employment opportunities and rising income levels in exposed regions account for the improvements in adolescents’ mental health after the policy change.

Our work also contributes to a substantial literature that exploits natural events, such as disease and famine outbreaks, to study the impact of early-life conditions on mental health outcomes ([Almond et al. 2018a](#); [Currie 2020](#)).⁸ This literature draws on traumatic events to support the fetal origins hypothesis that nutrition in early life has lasting effects on health, wellbeing and economic outcomes. Using health insurance claim records in Taiwan, [Liu et al. \(2016\)](#) find that fetal exposure to severe typhoons in Taiwan significantly increases the risk of adult mental health disorders—particularly mood disorders and antidepressant use—with stronger effects observed among women. Our work is in line with [Adhvaryu et al. \(2019\)](#), who reveal that favorable circumstances in early life, driven by positive commodity price shocks, result in a substantial decrease in severe mental distress in adulthood. Our study complements this literature by examining the effects of policy changes in a rapidly industrializing developing country on health outcomes later in life, rather than relying on natural disasters.

Finally, our study contributes to a growing international trade literature that documents the effects of trade policy on a range of health and economic outcomes, including mortality and marriage market outcomes ([Autor et al. 2019](#); [Pierce and Schott 2020](#)), self-reported health assessments ([McManus and Schaur 2016](#); [Bombardini and Li 2020](#)), labor market outcomes ([Pierce and Schott 2016](#); [McCaig and Pavcnik 2018](#); [Li 2018](#); [Dix-Carneiro and Kovak 2019](#)), intimate partner violence ([Erten and Keskin 2021](#)), crime ([Dell et al. 2019](#); [Dix-Carneiro et al. 2018](#)), and local

⁸For instance, exposure to the Dutch “Hunger Winter” during World War II or to the Six-Day War in Israel during the fetal period has been found to be associated with an increase in the likelihood of experiencing schizophrenia ([Susser et al. 1998](#); [Malaspina et al. 2008](#)).

public goods provision (Feler and Senses 2017). Our analysis broadens the understanding on the consequences of trade liberalization by considering its early-life exposure and by focusing on an outcome—adolescent mental health—that has not been previously studied by this literature.

The rest of the paper is organized as follows. Section 2 describes the data used in our analysis. Section 3 outlines our empirical strategy, and Section 4 presents our results for key outcomes and discusses mechanisms. Section 5 concludes.

2 Data

In this section, we describe the data sources employed in our analysis. In addition, where necessary, we explain how the main variables of interest are constructed.

2.1 Mental Health

Our primary measure of mental health is constructed using the 8-question CES-D. These data were collected as part of the CFPS, a nationally representative biennial survey designed to complement the Panel Study of Income Dynamics in the United States. We use two waves of CFPS data collected in 2016 and 2018.⁹

The CES-D scale was developed by Radloff in 1977 as a validated instrument to measure depression in nonclinical settings (Radloff 1977). It has been widely used in large health surveys, such as the National Health Interview Survey and the National Household Survey on Drug Abuse, and has been validated and used in more than 30 countries. The Chinese version of the CES-D scale has been widely adopted in previous research (Greenberger et al. 2000; Chen et al. 2009; Zhou et al. 2018), and its reliability and validity have been extensively tested among Chinese adolescents (Rankin et al. 1993; Zhang and Norvilitis 2002; Chen et al. 2009).

The questionnaire consists of 8 statements about several mental states experienced over the previous week. Respondents are asked to rate each item from 0 to 3, ranging from “never” to “all of the time.” More specifically, respondents rate the following statements regarding how they felt over the week prior to the interview: (i) I felt depressed; (ii) I felt that everything I did took considerable effort; (iii) My sleep was restless; (iv) I felt happy (reverse coded); (v) I felt lonely, (vi) I enjoyed life (reverse coded); I felt sad, and (vii) I could not get “going.”

Using the validated cutoff points (Rushton et al. 2002; Steffick et al. 2000), we create two indicator variables to measure incidence of depression: (i) mild depression, which takes a value

⁹The CFPS has three more rounds from 2010, 2012, and 2014. While the 2012 wave used the 20-item CES-D scale, the 2010 and 2014 rounds used the 6-item Kessler Psychological Distress Scale (K6). To have a consistent measure of mental health assessment across survey rounds and include comparable cohort sizes for treatment and control groups, we use the 8-item CES-D scale evaluated by respondents aged 11 and above in the 2016 and 2018 waves of the CFPS.

of one if the CES-D score ranges from 7 to 9, and (ii) severe depression, which takes a value of one if the CES-D score is greater than or equal to 10. In addition, we compute the first principle component of the reversed eight mental health indicators included in the CES-D and standardize it to construct a mental health index. Higher index values reflect better mental health.

The CES-D8 questions were answered by individuals at or above the age of 10. For our analysis, we retain individuals born in China with nonmissing responses for the prefecture of birth and CES-D questions and who were born between 1998 and 2005, with ages ranging from 11 to 20 at the time of the survey. We focus primarily on the transitional stage from childhood to early adulthood, which is a sensitive period for the formation of noncognitive skills (Cunha and Heckman 2007).¹⁰ We describe the definition of the treatment and control groups in Section 3. This leaves us with a sample of 4,749 individuals for mental health outcomes.

Moreover, the CFPS includes information on physical health outcomes. We create a physical health indicator which is assigned a value of 1 if the respondent did not feel any physically uncomfortable over the past two weeks and was not hospitalized in the previous year due to illness or injury. Finally, the CFPS presents respondents with two sets of cognitive tests to evaluate their cognitive ability. While one of these tests focuses on assessing the verbal ability of respondents, the other evaluates their math ability. We compute the average score for the verbal and math questions. The scores are then standardized using the sample mean and variance for each wave to create an index for cognitive function. A higher index value indicates a higher cognitive ability.

Panel A of Table 1A provides summary statistics for the CFPS data in our sample. We observe that 15 percent of adolescents experience mild depression and 6 percent experience severe depression. The average age of adolescents in our sample is 15, and approximately 53 percent of the participants are male.

2.2 CHNS, City Statistical Yearbooks, and Census Data

We use data from the CHNS, which was conducted by the Carolina Population Center at the University of North Carolina at Chapel Hill and the National Institute for Nutrition and Health (NINH) at the Chinese Center for Disease Control and Prevention (CCDC). The survey uses a multistage, random cluster process to draw samples in 52 prefectures of 11 Chinese provinces representing broad geographic and economic variation. We use six waves in our analysis: the 2000, 2004, 2006, 2009, 2011, and 2015 waves.¹¹

¹⁰This period also coincides with the onset of puberty (Jaworska and MacQueen 2015) and the malleability of the prefrontal cortex, which is the brain region governing emotion and self-regulation (Dahl 2004).

¹¹The most common prefecture form is the so-called “prefecture-level city” (*dijishi*). There are also prefectures that are not prefecture-level cities, and the term “county-level city” (*xianjishi*) is the official name for such jurisdictions. County-level cities have judicial rights but not legislative rights over their own local laws and are usually governed by prefecture-level divisions. Most county-level cities were created in the 1980s and 1990s by replacing more densely

The CHNS includes information on early life investments in and the nutrition intake of children. In particular, the Pregnancy History File (PHF) of the CHNS provides information on child-bearing for women who have ever married and were pregnant during the survey period. From this dataset, we create two measures of early life investments in children: the total number of months of breastfeeding that each child received, and whether the respondent reported that a child received a specific vaccine. Moreover, during the child survey, the child’s caregivers were asked to report the total caloric intake for each child aged 0-12 in the past three days and how many grams of protein, carbohydrate, and fat were consumed by each child in the past three days. We retain children with nonmissing responses for the prefecture of residence and who were born between 1998 and 2005. This left us with a sample of 699 children from the PHF data and 1,940 children from the child survey.¹²

In addition, we construct three weight-related outcomes using height and weight data for children aged 0–12, following the WHO 2006 Child Growth Standards: BMI-for-age z -scores, and indicators for overweight and obesity. Specifically, overweight and obesity are defined as having z -scores above +2 SD and +3 SD, respectively, using weight-for-height for children under five and BMI-for-age for those aged five to twelve. The summary statistics are tabulated in Panel B of Table 1A. The average child in our sample is 121 centimeters tall and weighs 25 kilograms.

Moreover, we use information on childcare provision and prenatal visits of pregnant women from the CHNS data. In particular, our childcare measurements include the number of hours per day and the number of days per week that a child is cared for by people outside the household for children aged 0 to 6 years; the summary statistics are tabulated in Panel A of Table 1B. Panel B of Table 1B presents the summary statistics for prenatal visits of mothers who had become pregnant since the previous wave of the survey.

In addition, we use data from the China City Statistical Yearbooks from 1995 to 2015 on total labor income and the total labor income per worker in each prefecture. Panel D of Table 1B presents the summary statistics for these outcomes. These data allow us to corroborate reform-induced positive income effects documented using GDP per capita at the county level (Erten and Leight 2021).

Finally, we use data from the China population census by combining the 1990, 2000, and 2010 census waves and the 2005 and 2015 one-percent population censuses. The census contains detailed information on region of residence, employment status, industry, demographic characteristics, and educational attainment. We aggregate the individual-level data to the prefecture level

populated counties. Such county-level cities are not “cities” in the strictest sense of the word, since they are usually much larger than a metropolitan area and cover rural areas many times the size of their urban, built-up area. Both metropolitan and rural areas of China are covered in this paper, and we refer to them as prefectures.

¹²Since the PHF surveyed only women who were pregnant during the CHNS sample period, the sample for early life investments is smaller than the nutrition sample.

and calculate the share of total employed (and its composition by agriculture, manufacturing and service sectors), unemployed, and out-of-labor-force individuals to the marriageable and working-age population for women and men separately.¹³ As presented in Panel E of Table 1B, the average employment rate is 77 percent for women and 91 percent for men.

2.3 Measuring Exposure to PNTR at the Prefecture Level

China's accession to the WTO was the culmination of a complex and lengthy process of negotiation. Before accession, China's NTR status in the US market required a risky annual renewal by Congress; if the renewal failed, Chinese exports would be subject to the much higher rates reserved for nonmarket economies. For example, in 2000, the average US NTR tariff was 4 percent, but China would have faced an average non-NTR tariff of 31 percent had its status been revoked. The US granted PNTRs status to China in 2002, but the status of Chinese exports in other markets did not change at that point. China's WTO membership significantly reduced uncertainty about US trade policy for China, generating a substantial increase in Chinese exports to US markets.

We utilize variation across Chinese prefectures in their concentrations in different industries in 1990 and variation across industries in the gap between the lower tariffs applied to most-favored-nation tariffs and the higher nonmarket rates. On average, a prefecture covers approximately 1.4×10^4 square kilometers and had a population of 3.7 million in 2000. We use the prefecture as the geographic unit of the local labor market for the following two main reasons. First, commuting ties are strong within prefectures in China but weak across prefectures. For this reason, a prefecture in China is similar to a commuting zone (CZ), a geographic unit for defining a local labor market in the United States.¹⁴ Another reason is that economic activities are more integrated within prefectures. The target-based performance evaluation system in China incentivizes top local bureaucrats (city mayors and Party secretaries) to implement various policies, such as investment and environmental policies, within prefecture boundaries.¹⁵

For each prefecture, we calculate a variable denoted "NTR gap" that is equal to the weighted average of the tariff gap across local industries operating in the prefecture; employment weights are used and constructed using each industry's share of local employment in 1990. Intuitively, a prefecture with a high NTR gap was exposed to high uncertainty before PNTR because its key industries risked facing high tariffs. Therefore such prefectures benefited more from the removal

¹³In China, the legal marriage age is 20 for women and 22 for men, and the retirement age is 55 for women and 60 for men.

¹⁴The concept of the CZ was developed by Tolbert and Sizer (1996) and used by Autor et al. (2013).

¹⁵Another example of government policies implemented at the prefecture level is the household registration (*hukou*) system. Interprefecture migration is limited due to the *hukou* system in China. Less than 5 percent of the working-age population changed their prefecture of residence between 2000 and 2005.

of uncertainty over tariffs.

$$NTRGap_p = \sum_j S_{jp}^{1990} \times NTRGap_j \quad (1)$$

where $NTRGap_p$ denotes the NTR gap for prefecture p , S_{ip}^{1990} denotes the share of employment by industry j in prefecture p in 1990, and $NTRGap_j$ denotes the NTR gap for industry j , which is the difference between the higher tariff rate that would have applied in the case of the revocation of China’s NTR status and the lower NTR rate, $NTRGap_j = NonNTRRate_j - NTRRate_j$.¹⁶

Since each prefecture’s sectoral composition prior to WTO accession is used to construct the employment shares, the NTR gap does not reflect endogenous changes in employment composition driven by trade policy uncertainty. Moreover, almost all of the variation in the NTR gap is explained by variation in non-NTR rates, which were set by the Smoot–Hawley Tariff Act of 1930, implying that NTR gaps did not change in response to current economic conditions in the US or China. Prefectures characterized by a larger NTR gap experienced a more significant reduction in trade policy uncertainty after 2001 and therefore were more likely to undergo greater expansion in export-oriented industries. Since the PNTR rates became effective for China as of January 1, 2002, our analysis characterizes all years from 2002 onward as the post-reform period.

Figure 1 illustrates the regional variation in the NTR gap across prefectures. Darker prefectures faced the most significant declines in tariff uncertainty, while lighter prefectures faced smaller declines. Across prefectures, the NTR gap averages 10.6 percent and has a standard deviation of 2.2 percent, with an interquartile range from 9.4 to 10.9 percent.

2.4 Other Control Variables

We use data from the CFPS and CHNS to control for individual-level characteristics of adolescents in our analysis. The CFPS and CHNS contain rich data on demographic and socioeconomic characteristics, such as data on gender, date of birth, marital status, and educational attainment. Panels A and B of Table 1A provide summary statistics for children’s demographic characteristics that we control for in our analysis, including age and gender of the child, parents’ completion of middle school, and parents’ age.

We also control for initial (1990) values of the following prefecture-level characteristics interacted with year dummies: log of GDP per capita, to account for differences in initial levels of economic development; the share of employment in manufacturing, to account for differences in industrial prefectures from prefectures specializing in other sectors; the share of population with

¹⁶We use the industry-level NTR gap data constructed by [Pierce and Schott \(2016\)](#) using ad valorem equivalent NTR and non-NTR rates. The NTR gap for industry j is the average NTR gap across the three-digit Chinese industry classification (CIC) tariff lines for that industry. We use the NTR gaps for 1999 following [Pierce and Schott \(2016\)](#) and [Erten and Leight \(2021\)](#). These NTR gaps are almost identical to those for 2000 or 2001; accordingly, the results are robust to using data from other years.

middle school education, to account for differences in human capital; the average population age; and the distance to nearest port, which accounts for differences in accessing international trade routes across locations.¹⁷ Data sources and variable definitions are described in detail in Appendix A.1.

We further control for other ongoing policy reforms during the period of trade policy uncertainty that might have affected children’s outcomes. First, we control for trade policy reforms in China, which include changes to output, input and external tariffs, export licenses, MFA quotas, and barriers to investment in China. We proxy barriers to investment in China using an input relationship-specificity index proposed by Nunn (2007). The index captures the extent to which holdup problems affect production, measured as the value share of inputs classified as relationship-specific, i.e., goods that are neither reference priced nor sold on exchange markets. Moreover, we control for trade policy changes in the US, including the NTR rate itself. Finally, we further control for China’s FDI liberalization policy using the 3-digit industry-level index from ?, aggregated to the prefecture level using 1990 employment shares. The data source and definition of each of these variables are described in detail in Appendix A.2.

3 Empirical Strategy

We use a difference-in-differences approach to analyze the effect of reduced trade policy uncertainty driven by PNTR on child mental health outcomes. More specifically, we examine whether the trajectory of child mental health outcomes in cities characterized by relatively large gaps between NTR tariff rates and non-NTR rates is different following China’s accession to the WTO in 2001.

We compare mental health of children born in cities that are exposed to larger reductions in trade policy uncertainty with those born in less exposed cities before and after the WTO accession. Recognizing the significance of the initial years of life for later life outcomes (Knudsen et al. 2006; Heckman 2012a; Richter et al. 2017), we compare children born in 2002–2005 (i.e., within 4 years after WTO accession) to those born in 1998–2001 (i.e., within 4 years before WTO accession). We employ ordinary least squares (OLS) to estimate the following specification:

$$Y_{ipbt} = \beta Post_b \times NTRGap_p + Z'_{pb}\lambda + X'_{ipbt}\theta + \delta_b + \delta_p + \delta_t + \varepsilon_{ipbt} \quad (2)$$

¹⁷Data on GDP per capita data come from the China City Statistical Yearbook of 1990. Data on manufacturing employment share are calculated from the 1990 population census. Data on education attainment and age are calculated from the 1990 population census. We first collect information about each prefecture’s latitude and longitude from China Data Online to calculate its distance to its nearest port. Geographical information on port locations (specific coordinates) is extracted from the World Port Index. We can then calculate the prefecture’s distance to its nearest port.

where subscript i denotes individuals, p the prefecture of birth, b the year of birth, and t the survey year. The dependent variable Y_{ipbt} is the outcome of individual i , born in prefecture p in year b , and observed in survey year t . This variable can be a mental health outcome, such as the incidence of mild or severe depression, or other individual outcomes observed during adolescence. The key variable is the interaction of the prefecture of birth NTR gap, $NTRGap_p$, and a post-PNTR dummy, $Post_b$, indicates if individual i experienced the PNTR policy during infancy. The DiD coefficient, β , captures the impact of exposure to PNTR in the year of birth on mental health outcomes during adolescence.

The specification (2) controls for several baseline prefecture characteristics and other trade policies interacted with birth year fixed effects, Z_{pb} , to flexibly isolate their potential differential impacts on outcomes of interest. The prefecture-of-birth initial characteristics include the log of per capita GDP, the share of employment in the manufacturing sector, the share of population with middle school education, the average population age, the log of the distance to the nearest port observed in 1990. The other trade policies include China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy, and contract intensity observed in 2001 as explained in Section 2 and Appendix A.1. The variable X_{ipt} denotes individual-level controls, including age, gender, indicator variables for whether the mother and father completed middle school, father’s age and mother’s age.

The specification also includes birth year fixed effects, δ_b , accounting for aggregate shocks that affected all prefectures in a given birth year of birth, and birth prefecture fixed effects, δ_p , which net out characteristics of birth prefectures that are time-invariant. We also add survey year fixed effects, δ_t , which absorb macroeconomic shocks affecting all regions in a particular survey year. We cluster standard errors at the prefecture-of-birth level to account for serial correlation in outcomes within birth prefectures. Our preferred specification includes the entire set of these control variables; however, we also show that the results are robust to adding them gradually.

The CFPS dataset contains information on the mental health of respondents aged 10 and older. In our analysis, we focus on adolescents aged 11 and above in the 2016 and 2018 rounds of the CFPS in our baseline analysis. Since these data provide four treatment cohorts born from 2002–2005, we compare them to four control cohorts born from 1998–2001. Hence, we compare four cohorts born before the policy change to four cohorts born after the policy change covering the birth years of 1998–2005 in our baseline results.¹⁸

Throughout the analysis, we report estimated treatment effects together with standard errors clustered at the prefecture-of-birth level and conventional two-sided single-hypothesis p -values. We account for multiple hypothesis testing by reporting q -values generated by the adaptive false

¹⁸As a robustness check, we also limit our sample by focusing on cohorts born in 1999–2004 (three cohorts born before and after the policy change) and show that the results are consistent with our baseline estimates.

discovery rate (FDR) procedure of [Benjamini et al. \(2006a\)](#). This method refines the standard Benjamini–Hochberg step-up procedure by first estimating the proportion of true null hypotheses and then tightening the rejection thresholds while controlling for the expected proportion of rejections that are Type I errors, FDR.

Additionally, we estimate the following event study specification to test whether pre-existing trends in the outcomes of interest drive our results. In particular, we implement the following specification to examine whether individuals born in prefectures with higher NTR gaps experienced differential changes in their outcomes after the change in international trade policy versus before:

$$Y_{ipbt} = \sum_{b=1998}^{2005} \beta_b 1\{B_i = b\} \times NTRGap_p + Z'_{pb} \lambda + X'_{ipbt} \theta + \delta_b + \delta_p + \delta_t + \varepsilon_{ipbt} \quad (3)$$

where B_i denotes the birth year of individual i , $NTRGap_p$ stands for the time-invariant prefecture-level NTR gap in Eq. (1), and the control variables are the same as those in Eq. (2). The summation on the right-hand side contains the interaction of year-of-birth dummies (excluding 2001) with the treatment intensity $NTRGap_p$. This specification is commonly used to test the common trend assumption. If the common trend assumption holds, this difference should be statistically insignificant for the periods before the intervention. In our empirical setting, the estimates for β_b evaluate the relationship between the mental health outcome and the NTR gap for each year of birth b . Conditioned on the baseline variables Z, X , we expect the relationship between the NTR gap and mental health to be weak and statistically insignificant for the periods that precede the PNTR policy.

Finally, we employ a quantile regression analysis to assess whether the PNTR had heterogeneous effects across the baseline distribution of children’s mental health. Specifically, we implement the recentered quantile regression approach of [Firpo et al. \(2009\)](#) and estimate:

$$RIF(Y_{ipbt}; Q_\tau) = \alpha_\tau + \beta_\tau (\text{Post}_b \times NTRGap_p) + \lambda'_\tau Z_{pb} + \theta'_\tau X_{ipbt} + \delta_p + \delta_b + \delta_t + \varepsilon_{ipbt, \tau}, \quad (4)$$

where $RIF(Y_{ipbt}; Q_\tau)$ denotes the recentered influence function of the standardized mental health index Y_{ipbt} evaluated at the τ -th quantile of its unconditional distribution. The covariates and fixed effects on the right-hand side are defined analogously to Eq. (2). The coefficient β_τ captures the causal effect of PNTR exposure on the τ -th quantile of the unconditional distribution of the mental health index. Standard errors are computed using a nonparametric bootstrap with 250 replications.

4 Results

4.1 Mental Health Outcomes

We report our primary estimates of Eq. (2) in Table 2. The coefficient estimates in Panel A show no evidence of a statistically significant relationship between PNTR and the incidence of mild depression. The estimates are all very close to zero, indicating null effects. In Panel B, the coefficient estimates are negative and statistically significant across all specifications, indicating that relative to those born in less exposed prefectures, adolescents born in prefectures more exposed to the policy change experienced declines in their incidence of severe depression. As discussed in Section 3, the most rigorous specifications including individual controls and year-of-birth fixed effects interacted with prefecture initial characteristics and with other trade policies, reported in column 3, are the preferred specifications. The coefficient estimate in column (3) implies that an interquartile shift in prefectures' exposure to PNTR is associated with a decline in the probability of experiencing severe depression by 5.12 percentage points (0.032×1.6) in cohorts born in more PNTR-exposed prefectures relative to the same cohorts born in less affected regions in China. Finally, the estimates in Panel C indicate that adolescents born in prefectures more exposed to the policy change experienced a relative improvement in their mental health. In terms of magnitudes, our estimates indicate that an interquartile increase in exposure to PNTR leads to a improvement in the mental health index by 0.19 standard deviations.

The asymmetry of null effects on mild depression (Panel A) but sizable declines in severe depression (Panel B) is consistent with both measurement and biological arguments. Mild symptoms are transient and measured with lower reliability, limiting detectable change, whereas severe depression reflects persistent impairment captured with greater precision (Kessler et al. 2003a). Biologically, reductions in early-life stress and improved investments generate nonlinear gains in stress physiology, yielding larger marginal benefits at higher baseline severity (McEwen 2007).

We also test whether the estimated effects for the mental health outcomes appeared only for the cohorts born after the policy change. The event study estimates presented in Figure 2 show that for the period prior to the policy change, the coefficient estimates on the interaction of the year of birth with the NTR gap are indistinguishable from zero. This absence of differential pre-existing trends between adolescents born in prefectures more exposed to PNTR and those born in less affected prefectures offers support for the common trends assumption in our difference-in-differences strategy.¹⁹ In contrast, for the cohorts born after the implementation of PNTR in 2002,

¹⁹Appendix Table A1 reports the pre-trend analysis of the mental health outcomes, following the methodology of Roth (2022). For each outcome, we present (i) the number of pre-treatment periods, (ii) how many pre-period coefficients are individually significant, (iii) the maximum absolute t-value among pre-periods, (iv) the p-value from a joint significance test of all pre-period coefficients, and (v) the t-statistic of the fitted linear slope through pre-period

the coefficient estimates for severe depression shift down and become significantly different from zero at the 5-percent level. Similarly, the estimates for mental health index move upwards in the post-PNTR years of birth and become statistically significantly different from zero one year after the policy change.

Given the heterogeneity observed between mild and severe depression, we further investigate whether the policy led to distributional shifts across the mental health spectrum using a quantile regression approach as specified in Equation 4. The results, shown in Figure 3, reveal that the estimated effects are concentrated among individuals at the lower end of the mental health distribution—that is, those with poorer baseline mental health. This pattern suggests that the policy primarily benefited individuals who were already more vulnerable to mental health challenges, rather than shifting the entire distribution uniformly. These results are consistent with our main findings in Table 2 and Figure 2, which show that the PNTR significantly reduces the incidence of severe depression, without having a significant impact on mild depression. Together, these results imply that the gains in mental well-being were driven by improvements among those most adversely affected.

We next show that our estimates are robust both to using different definitions of the NTR gap in Appendix Table A2 and to estimating alternative regression specifications in Appendix Table A3. More specifically, the results in Appendix Table A2 show that the estimates for depression outcomes are robust to reconstructing the NTR gap by excluding industries with the highest (Panel A) or lowest value on the NTR gap (Panel B), to winsorizing the NTR gap at the 5th and 95th percentiles (Panel C), and to reconstructing the NTR gap by excluding nontradable industries and using only the share of tradable industries in calculating the NTR gap (Panel D).

We further show that our results are robust to accounting for technological shocks, including the increased adoption of industrial robots (Liu et al. 2024). Specifically, we use data for imports of industrial robots from Chinese customs data, and create a shift-share measure of exposure using the 1990 employment shares at the 3-digit industries across prefectures.²⁰ Results reported in Panel A in Table Table A4 show that our estimates are not sensitive to controlling for this exposure to industrial robots.

To address potential concerns that cohorts born immediately before the PNTR reform may

estimates. Columns (6) and (7) report the minimal detectable slopes corresponding to the power levels $\gamma = 0.5$ and $\gamma = 0.8$, i.e., the magnitudes of pre-trends that a conventional pretest would identify 50% or 80% of the time. Across all outcomes, none of the pre-period coefficients are statistically significant, and the joint p -values indicate that we cannot reject the null hypothesis that all pre-period coefficients are jointly zero, suggesting an absence of systematic pre-treatment differences. The slopes of the fitted lines of pre-treatment coefficients are all statistically insignificant, and well below the minimal detectable slopes at $\gamma = 0.5$ and $\gamma = 0.8$, indicating that even moderate violations of parallel trends would likely remain undetected given the sample size. Taken together, these results support the plausibility of the parallel-trends assumption for all outcomes.

²⁰Please see Appendix A.1 for the construction of this measure.

have been partially exposed to the policy in utero or during infancy, we further restrict the sample to cohorts that were clearly unexposed and clearly exposed. Specifically, in Appendix Table A4, Panel B, we drop births in 2001–2002 and compare cohorts born in 1999–2000 to those born in 2003–2004. This specification ensures that the control group was fully unexposed to PNTR and that the treatment group experienced the reform only after birth. The results remain robust: the estimated effects on severe depression and the mental health index are similar in magnitude and significance to our baseline estimates.

In addition, we conduct placebo analyses by assigning false PNTR implementation years and using only pre-PNTR cohorts from CFPS 2010–2014 waves. Specifically, Appendix Table A4, Panels C–E, reports estimates from specifications that falsely assume implementation in 1995, 1996, and 1997, treating individuals born immediately after each placebo year as the treated cohort. We restrict the sample to adolescents born before 2002 and aged 11–20 at the time of interview, and we re-estimate our baseline equations while adding the full set of controls stepwise. Across all specifications, the placebo coefficients are small in magnitude and statistically indistinguishable from zero for all mental health outcomes, providing no evidence of pre-trends or spurious correlations.²¹

Further robustness checks in Appendix Table A3 indicate that the estimates for depression outcomes are robust to weighting the regression by the 1990 prefecture population, controlling for province-specific birth year linear trends, reducing the sample to include cohorts born between 1999 and 2004 and extending the sample to cohorts born between 1997 and 2006. Finally, we examine whether the policy change led to any significant heterogeneous treatment effects by child gender. The results reported in Appendix Table A5 show that these effects do not vary by gender.

We next assess whether the estimated effects on adolescent mental health differ between rural and urban settings, which vary in both sample representation and socioeconomic context. We implement this by interacting exposure to PNTR with an indicator for whether the prefecture’s rural population share in 2000 is above the median. The results, reported in Panel B of Appendix Tables A5, show no statistically significant differences between more rural and more urban prefectures for mild depression, severe depression, or the mental health index. The estimated effects are similar in magnitude and direction across both settings, suggesting that our main findings are not driven

²¹The CFPS administered two mental health instruments across the 2010–2014 waves for respondents aged ten and above. The 2010 and 2014 waves used the six-item Kessler Psychological Distress Scale (K6), while the 2012 wave employed the 20-item Center for Epidemiologic Studies Depression Scale (CES-D20). We harmonize outcomes following validated scoring rules. For K6, each item is coded 0–4 and summed (range 0–24), with mild depression defined by $5 \leq K6 < 13$ and severe depression by $K6 \geq 13$ (Kessler et al. 2002, 2003b; Furukawa et al. 2003; Fushimi et al. 2012; Kang et al. 2015). For CES-D20, each item is coded 0–3 and summed (range 0–60), with mild depression defined by $16 \leq CESD20 < 24$ and severe depression by $CESD20 \geq 24$ (Radloff 1977; Andresen et al. 1994). To construct a harmonized mental health index, we compute the first principal component of the reverse-coded K6 or CES-D20 items within each wave and standardize it to mean zero and unit variance, such that higher values indicate better mental health.

by rural–urban disparities in exposure or data coverage.²²

4.2 Mechanisms

In this section, we examine mechanisms that may explain how early exposure to the policy change may have reduced the risk of severe depression in adolescence. We divide our analysis into four subsections by focusing on the effects of the trade policy reform on the following outcomes: (a) parental income and employment, (b) early life investments, (c) migration, and (d) fertility.

4.2.1 Parental Income and Employment

One potential mechanism through which early exposure to PNTR might have led to decreased depression in adolescence is via an improvement in parental income. Children born into households in PNTR-exposed prefectures after the policy change were likely to have more resources due to both the higher income of parents producing tradable goods and the positive local labor demand effects stimulating the income of households in nontradable sectors. Such positive income effects during gestation and infancy could have large developmental effects that persist over time, leading to better mental health outcomes in adolescence.

Using data from the China City Statistical Yearbooks, we test whether there was an improvement in average labor income per worker in prefectures that were differentially affected by the trade reform after it was implemented.²³ We estimate the following specification:

$$Y_{pt} = \delta_t + \delta_p + \beta(Post_t \times NTRGap_p) + Z'_{pt}\lambda + v_{pt} \quad (5)$$

where subscript p denotes the prefecture and t denotes the year. The dependent variable Y_{pt} is log labor income per worker in a given prefecture p in time period t . The $NTRGap_p$ is defined in Eq. (1) and the post-PNTR dummy, $Post_t$, is equal to one for the period after 2001. The terms δ_t and δ_p are year and prefecture fixed effects, respectively. Additional controls at the prefecture-year level Z'_{pt} include year fixed effects interacted with other trade policies and initial prefecture characteristics as described in Section 3, and standard errors are clustered at the prefecture level.

Table 3 reports the results. The coefficient estimates are positive and statistically significant, indicating that total labor income and labor income per worker exhibit an increase in prefectures more exposed to PNTR than in less affected regions after the policy change. These results are

²²We also apply the same rural–urban heterogeneity test to the main mechanism outcomes discussed later in the paper (Appendix Tables A6, A7, A8). The interaction effects remain small and statistically insignificant throughout, suggesting that the mechanisms operate similarly across rural and urban settings.

²³In the absence of a reliable and consistent measure of parental income at the individual level from survey data, we use prefecture-level information from the China City Statistical Yearbooks on labor income and the number of workers employed.

consistent with the finding in [Erten and Leight \(2021\)](#) that counties with greater exposure to PNTR exhibited higher GDP per capita after the change in China's PNTR status.

A related potential mechanism could be that exposure to PNTR may have shifted workers across sectors or differentially affected the employment statuses of men and women, resulting in different time allocations between work outside and inside the home. If the amount of time that parents spend with children increases, these early investments could bring about lasting changes in children's mental health ([Chang et al. 2019](#); [Milkie et al. 2015](#)).

Using multiple rounds of census data from 1990 to 2015, we estimate Eq. (5) to test whether the reform affected labor market outcomes by using the share of the working-age population in a particular employment category as the dependent variable. Each estimation is weighted by the 1990 prefecture population and standard errors are clustered at the prefecture level. The results in Panel A of Table 4 show that women in prefectures more exposed to PNTR experienced a relative decline in their probability of employment and a relative increase in nonparticipation (or in not being in the labor force [NILF]). These changes are driven by a significant decline in the agricultural employment of women. While some women found new employment opportunities in manufacturing and services, these increases did not offset the decline in agricultural work. In contrast, the estimates in Panel B show that men in more exposed prefectures experienced a relative increase in their probability of employment and a relative decline in nonparticipation as a result of increasing employment opportunities in manufacturing and services.

4.2.2 Early Life Investments

Positive income shocks and shifts in parental time allocation induced by a policy change may, in turn, result in greater early life investments in the child, such as the frequency of prenatal visits, the duration of breastfeeding and the number of vaccinations, by shifting the household's intertemporal budget, creating incentives for parents to reinforce infants' endowments, and increasing mothers' available time for child-related activities ([Heckman 2007](#)). Moreover, increases in parental income may improve the nutrition intake of children by relaxing parents' budget constraints. For example, they may allow parents to purchase more expensive food items such as meat and other sources of protein. Finally, women might allocate more of their time to childcare when their labor force participation declines in response to the trade reform. These improvements in early life investments may also positively impact the mental health outcomes of adolescents.

Table 5 reports the results for changes in early life investments in response to the trade policy change.²⁴ The estimate in column 1 is positive and significant, indicating a relative increase in

²⁴Because the CFPS does not collect information on early-life investments such as breastfeeding, vaccinations, or childcare, we rely on complementary evidence from the CHNS for these mechanisms. While this results in smaller and distinct samples, the CHNS is the only nationally representative dataset that provides these measures in this time

prenatal visits of pregnant mothers in response to the policy change in more affected regions.²⁵ The estimates in columns 2 and 3 show that infants born after the policy change in more affected prefectures are breastfed longer and receive more vaccines than infants in other prefectures.²⁶ The estimates for individual vaccines reported in columns 4–9 indicate that the probability of receiving all vaccines significantly increases, with the exception of polio, for which the estimate is positive but imprecisely estimated. We interpret these results as evidence of prenatal care and early life investments as a channel for the estimated impacts of early life income shocks on adolescent mental health.

In Table 6, we present estimates for whether the policy change affected the nutrition intake and development of children. The estimates in Panel B indicate that children in more exposed prefectures experienced an increase in their protein intake relative to children living in less affected prefectures after the reform. In Panel F, we also observe a positive impact of the reform on the average weight of children born in more affected regions compared to children born in less affected regions. We interpret these results as evidence of child nutrition and development being a potential channel for the estimated impacts of early life income shocks on adolescent mental health. In addition, to explore potential “double burdens” of malnutrition, we extend the analysis to weight-related child development outcomes—BMI-for-age z-scores, an indicator for being overweight, and an indicator for obesity—reported in Table 6, Panels G–I. Across these outcomes, the estimates show no statistically significant changes in these outcomes. The absence of effects indicates that while the reform improved children’s nutrient intake and physical growth, it did not lead to excessive weight gain or higher prevalence of overweight or obesity. Hence, the policy appears to have alleviated undernutrition without generating an offsetting rise in overnutrition, consistent with China’s nutritional transition stage during the early 2000s.

A related mechanism through which trade liberalization may influence child health is by altering the local food environment. Specifically, the PNTR could have affected China’s food import patterns, expanding access to both higher-quality protein sources and calorie-dense processed period. We therefore interpret the CHNS results as supportive evidence consistent with the mechanisms suggested by the CFPS analysis.

²⁵To estimate the impact of contemporary exposure to the trade reform on the number of prenatal visits, we estimate the following specification using data from the 1991–2015 CHNS:

$$Y_{ipt} = \delta_t + \delta_p + \beta(Post_{it} \times NTRGap_p) + Z'_{pt}\lambda + X'_{ipt}\gamma + v_{ipt} \quad (6)$$

where the dependent variable Y_{ipt} is a health outcome for woman i living in prefecture p in year t . The $NTRGap_p$ is defined in Eq. (1). Policy exposure is captured by the interaction of the $NTRGap_p$ and a post-PNTR dummy, $Post_{it}$, equal to one for the period after 2001. The terms δ_t and δ_p are year and prefecture fixed effects, respectively. The set of individual controls X'_{ipt} includes the woman’s age and middle school completion status. Additional controls at the prefecture-year level Z'_{pt} include year fixed effects interacted with other trade policies and prefecture initial characteristics as described in Section 3. We cluster standard errors at the prefecture level.

²⁶We estimate Eq. (2) with the only difference being that we use the prefecture of residence for regional variation since the CHNS does not include prefecture-of-birth information.

foods. Using annual prefecture-level customs data from 1997–2014, Appendix Table A9 examines whether local food imports responded to the PNTR. Panel A shows that food imports per capita rose significantly in prefectures more exposed to the policy, while Panel B indicates positive but statistically insignificant changes in overall goods imports. Taken together, these results suggest that increased availability of imported food may have contributed to the nutritional improvements associated with the PNTR, as documented in Table 6.

In Table 7, we test whether the reform affected childcare arrangements using CHNS data. One consequence of women’s lower participation in the labor market might be that they could spend more time with their children, becoming primary caregivers. Indeed, the estimates in Panel A show that compared to children in less exposed prefectures, children born in prefectures more affected by the policy change experienced a significant decline in the hours of care per day that they received from people outside of the household on a typical day. Similarly, the estimates in Panel B indicate that children in more exposed regions experienced a relative decline in days of care per week received from people outside of the household in a typical week. We interpret these results together to indicate more time spent on care provision at home as a channel for the estimated impacts of early-life income shocks on adolescent mental health.²⁷ These results are also consistent with previous research from the US context showing that increases in maternal time driven by reductions in women’s labor market opportunities are associated with improved child health outcomes, including reductions in emotional difficulties (Page et al. 2019). As complementary evidence, Appendix Table A10 shows some suggestive evidence using data from CFPS that parenting styles have improved in response to the policy change.²⁸ Together, these findings suggest that changes in parental time allocation and improvements in parenting quality may represent key mechanisms through which the policy enhanced children’s emotional well-being.

We also explore whether the policy change affected physical health, cognitive function, or school dropout rates during adolescence.²⁹ This is also motivated by recent research that shows

²⁷Direct time-use data on mother–child interaction are not available in the CHNS. We therefore interpret the observed decline in external childcare hours and days as indirect evidence of increased maternal time at home. This limitation should be kept in mind when interpreting the results on the maternal care channel.

²⁸Parenting styles are constructed using the 14-item Parental Bonding Instrument (PBI) reported in the CFPS for our sample children aged 10–15 in the 2010, 2012, and 2014 waves. The PBI captures parent–child bonding along two core dimensions—care and overprotection/strictness. Following Parker et al. (1979), we apply exploratory factor analysis to extract these two latent dimensions and dichotomize each at the median to classify parenting into four types: (i) high care and low overprotection (“optimal/authoritative” parenting), (ii) high care and high overprotection (“affectionate constraint”), (iii) low care and high overprotection (“affectionless control”), and (iv) low care and low overprotection (“neglectful” parenting). The PBI is one of the most extensively validated instruments for assessing parental warmth and control, with strong reliability across cultures (Enns et al. 2002; Liu et al. 2011). A large body of developmental psychology and health economics research shows that optimal parenting—characterized by emotionally supportive, warm, and non-intrusive parental behavior—promotes greater emotional security, reduces socio-emotional difficulties, and improves adolescent mental health and overall psychological well-being (Jabeen et al. 2013; Steinberg et al. 2013; Uji et al. 2014).

²⁹While dropping out of school is an extreme outcome to proxy school performance, we do not observe any other

that Chinese cities more exposed to trade liberalization sent more students to U.S. universities (Khanna et al. 2025). The coefficient estimates in Appendix Table A11 show no evidence of a significant change physical health, cognitive function or school dropout probability in response to early exposure to PNTR after the reform. These findings highlight that even though improvements in childhood nutrition do not have lasting effects on the physical health or cognition, they may nevertheless have long term effects in reducing the risk of severe depression among adolescents.

Taken together, our evidence indicates that PNTR-induced improvements in early-life conditions primarily operated through socioemotional pathways rather than cognitive or somatic ones. As mentioned, cohorts exposed in infancy experienced longer breastfeeding, higher protein intake, more prenatal care, and less reliance on outside childcare—changes that plausibly enhance stress regulation and emotional well-being—while we detect no lasting effects on cognition, physical health, or school dropout. This divergence accords with sensitive-period models in which circuits governing affective regulation are especially plastic in early childhood, whereas cognitive skills accumulate over a longer horizon and can be buffered by later schooling and compensatory inputs (Knudsen et al. 2006; Cunha and Heckman 2007). Empirically, early health shocks depress adolescent socioemotional outcomes with little effect on cognitive test scores (Zhou and Wang 2023), and short-run nutritional or environmental improvements often yield limited cognitive gains (Conti et al. 2016). Consequently, the mental health gains that we document are most consistent with early nurturing and reduced stress as the dominant channels producing effects.

4.2.3 Adult Mental Health

Another potential mechanism through which changes in early childhood circumstances induced by the trade reform may impact adolescent mental health is that PNTR exposure may affect the mental health of parents themselves, which in turn changes the home environment and child outcomes. We also examine whether the PNTR shock affected parents' own mental health using the CFPS and CHNS data. Appendix Tables A12–A13 report the estimated effects on mothers' and fathers' mental health outcomes, respectively, including diagnosed mental conditions, mild and severe depression, and a composite mental health index constructed from CES-D items. We find no statistically significant effects on mothers' mental health. For fathers, the estimates point to a modest deterioration in mental health, consistent with increased work intensity and stress associated with labor reallocation across sectors. Importantly, these adult patterns do not mirror the improvements we document in adolescents' mental health, suggesting that our main results are unlikely to be explained by changes in parental psychological status.

appropriate performance indicators in the CFPS dataset.

4.2.4 Migration

A well-documented issue in the literature is the phenomenon of “left behind” children, which has been intensified by China’s restrictive *hukou* system. This system makes it challenging for migrant parents to bring their children when they relocate for work, leaving many children in rural areas while their parents seek employment elsewhere (Heckman and Yi 2012; Tong et al. 2019; Gao et al. 2024), which can lead to mental distress among the left-behind children. In our case, concerns about increased parental migration are mitigated by the fact that our treated prefectures were primarily on the receiving end of domestic migration due to improved economic conditions following the PNTR. Prior research supports this, showing that the reduction in trade policy uncertainty increased internal in-migration to affected prefectures (Facchini et al. 2019). Nonetheless, to ensure robustness, we test whether the PNTR policy influenced parental absence using three indicators: (i) whether parents were absent for at least one week during their child’s early years (ages 0-3) based on CFPS data, (ii) whether the mother was not living in the household and was seeking employment elsewhere (CHNS data), and (iii) whether the father was not living in the household and was seeking employment elsewhere (CHNS data). Results in Appendix Table A14 show no significant evidence that the policy change affected these parental absence outcomes.

Another way migration patterns could affect our estimates is if new migrants, despite the *hukou* system, moved to the more affected regions with their young children, resulting in a potentially selected sample. Appendix Table A15 indicates that migrants are negatively selected in terms of educational attainment and minority status: they are less likely to complete junior high school, have fewer years of schooling, and are more likely to belong to minority ethnic groups. Given that children from lower socioeconomic backgrounds generally experience worse mental health outcomes in China (Niu et al. 2024), this compositional change would likely bias our results downward, making our estimates a lower bound of the true impact.

4.2.5 Fertility

We next test whether PNTR had a significant impact on fertility outcomes. If parents in more exposed regions had higher income levels due to PNTR, they might have reduced their desired number of children, allowing them to invest more per child. Alternatively, having more income might have allowed parents to pay the fine for having a second child, allowing them to have more children. Table A16 provides estimates for two outcomes observed in multiple rounds of census data from 1990 to 2015 at the prefecture level: (i) the number of births over the past 12 months per 1,000 women and (ii) the number of children for women living in a prefecture. We find no evidence of a significant impact of PNTR on these fertility outcomes. In sum, we conclude that our results are not explained by selective migration or fertility in response to the trade policy change.

5 Conclusion

This study uses the 2002 US bill that granted China PNTR as a source of economic variation to estimate the causal effects of economic changes in early life conditions induced by trade liberalization on adolescent mental health. The study employs a generalized difference-in-differences approach to evaluate how the effects of early life circumstances induced by trade liberalization affect adolescent mental health in China.

Using a nationally representative sample of households in China, we find that cohorts born in prefectures more exposed to the trade liberalization policy of PNTR experienced a significant relative improvement in their mental health during adolescence. We show that these declines took place for the cohorts born after the policy change, with estimates implying that an interquartile shift in prefectures' exposure to PNTR in infancy is associated with a 0.19 standard deviation improvement in adolescent mental health. Our findings also show that these improvements in mental health result from a reduction in the incidence of severe depression. We find null effects of the policy change on the incidence of mild depression.

Exploring potential channels, we document that prefectures more exposed to the trade reform exhibit relative increases in average income per worker and a relative increase in women's nonparticipation in the workforce. In more exposed prefectures, we also find evidence of an increase in prenatal visits of mothers relative to less affected regions after the policy change. Children born in more exposed prefectures experienced an improvement in early life investments and nutritional intake relative to their counterparts in less affected regions after the policy change. The increased nonparticipation of women coincides with a decline in the probability of children receiving child-care outside of home. These results suggest that the probability of children being taken care of at home, potentially by their mothers, increased after the policy change in more affected prefectures. These findings also draw attention to the importance of designing parental leave policies that provide sufficient time for parents to spend with their newborns.

Recent evidence from adolescent mental health studies indicates that cognitive behavioral therapy (CBT), alone or combined with antidepressant medication, can reduce depressive symptoms by 43 to 70 percent within 12 weeks ([Lewandowski et al. 2013](#); [Kennard et al. 2009](#); [March et al. 2004](#)). Given the substantial improvements in mental health outcomes of adolescents (approximately 0.2 standard deviations) in response to improving access to advanced country markets by one standard deviation at the local labor market level, this evidence highlights the need for comprehensive economic policies that enhance employment and income opportunities for households, which could help prevent depression later in life. The urgency for such policies becomes even clearer when considering the economic burden of depression in China, estimated at US\$6.26 billion annually, with 84% of the cost stemming from productivity losses ([Hu et al. 2007](#)).

Overall, these results contribute to the growing body of research on adolescent mental health, documenting the importance of positive shocks in terms of income and parental time reallocation in improving later life outcomes. Such shocks can significantly influence a wide range of children's outcomes that shape their life trajectories. This is particularly important in developing countries in which these dimensions of shocks have received less attention and resource constraints for improving mental health are more binding. Our findings also highlight the importance of potential mental health improvements stemming from early exposure to trade reforms that enhance access to advanced country markets. Previous estimates for the welfare importance of these reforms, while already large, are underestimated to the extent that they do not account for mental health.

References

- Adhvaryu, Achyuta, James Fenske, and Anant Nyshadham**, “Early life circumstance and adult mental health,” *Journal of Political Economy*, 2019, 127 (4), 1516–1549.
- Almond, Douglas and Janet Currie**, “Human capital development before age five,” in “Handbook of Labor Economics,” Vol. 4, Elsevier, 2011, pp. 1315–1486.
- , —, and **Valentina Duque**, “Childhood circumstances and adult outcomes: Act II,” *Journal of Economic Literature*, 2018, 56 (4), 1360–1446.
- , —, and —, “Childhood circumstances and adult outcomes: Act II,” *Journal of Economic Literature*, 2018, 56 (4), 1360–1446.
- Andresen, Elena M, Judith A Malmgren, William B Carter, and Donald L Patrick**, “Screening for depression in well older adults: Evaluation of a short form of the CES-D,” *American Journal of Preventive Medicine*, 1994, 10 (2), 77–84.
- Autor, David, David Dorn, and Gordon Hanson**, “The China syndrome: The impact of import competition on U.S. labor markets,” *American Economic Review*, 2013, 103 (6), 2121–2168.
- , —, and —, “When work disappears: Manufacturing decline and the falling marriage market value of young men,” *American Economic Review: Insights*, 2019, 1 (2), 161–78.
- Benjamini, Yoav, Abba M. Krieger, and Daniel Yekutieli**, “Adaptive linear step-up procedures that control the false discovery rate,” *Biometrika*, 2006, 93 (3), 491–507.
- , —, and —, “Adaptive Linear Step-Up Procedures That Control the False Discovery Rate,” *Biometrika*, 2006, 93 (3), 491–507.
- Bombardini, Matilde and Bingjing Li**, “Trade, pollution and mortality in China,” *Journal of International Economics*, 2020, 125, 103321.
- Chang, Fang, Yuxi Jiang, Prashant Loyalka, James Chu, Yaojiang Shi, Annie Osborn, and Scott Rozelle**, “Parental migration, educational achievement, and mental health of junior high school students in rural China,” *China Economic Review*, 2019, 54, 337–349.
- Chinese Academy of Sciences**, “Report on National Mental Health Development in China,” Chapter 8, Institute of Psychology, Chinese Academy of Sciences 2020.
- Conti, Gabriella, James J. Heckman, and Rodrigo Pinto**, “The health effects of two influential early childhood interventions,” *Economic Journal*, 2016, 126 (596), F28–F65.
- Costello, E Jane, Sarah Mustillo, Alaattin Erkanli, Gordon Keeler, and Adrian Angold**, “Prevalence and development of psychiatric disorders in childhood and adolescence,” *Archives of General Psychiatry*, 2003, 60 (8), 837–844.
- Cunha, Flavio and James Heckman**, “The technology of skill formation,” *American economic review*, 2007, 97 (2), 31–47.
- Currie, Janet**, “Child health as human capital,” *Health Economics*, 2020, 29 (4), 452–463.
- and **Mark Stabile**, “Child mental health and human capital accumulation: the case of ADHD,” *Journal of Health Economics*, 2006, 25 (6), 1094–1118.
- and —, “Mental Health in Childhood and Human Capital,” in “The Problems of Disadvantaged Youth: An Economic Perspective,” University of Chicago Press, 2009.
- , —, **Phongsack Manivong, and Leslie L Roos**, “Child health and young adult outcomes,” *Journal of Human Resources*, 2010, 45 (3), 517–548.

- Dahl, Ronald E**, “Adolescent brain development: a period of vulnerabilities and opportunities. Keynote address,” *Annals of the New York Academy of Sciences*, 2004, 1021 (1), 1–22.
- Dell, Melissa, Benjamin Feigenberg, and Kensuke Teshima**, “The violent consequences of trade-induced worker displacement in Mexico,” *American Economic Review: Insights*, 2019, 1 (1), 43–58.
- Dix-Carneiro, Rafael and Brian K Kovak**, “Margins of labor market adjustment to trade,” *Journal of International Economics*, 2019, 117, 125–142.
- , **Rodrigo R Soares, and Gabriel Ulyssea**, “Economic shocks and crime: Evidence from the Brazilian trade liberalization,” *American Economic Journal: Applied Economics*, 2018, 10 (4), 158–95.
- Enns, MW1, BJ Cox, and I Clara**, “Parental bonding and adult psychopathology: results from the US National Comorbidity Survey,” *Psychological Medicine*, 2002, 32 (6), 997–1008.
- Erten, Bilge and Jessica Leight**, “Exporting out of agriculture: The impact of WTO accession on structural transformation in China,” *Review of Economics and Statistics*, 2021, 103 (2), 364–380.
- **and Pinar Keskin**, “Trade-offs? The Impact of WTO Accession on Intimate Partner Violence in Cambodia,” *forthcoming in Review of Economics and Statistics*, 2021.
- , **Jessica Leight, and Lianming Zhu**, “The Impact of FDI Liberalization on Structural Transformation and Demographic Change: Evidence from China,” *mimeo*, 2024.
- Facchini, Giovanni, Maggie Y Liu, Anna Maria Mayda, and Minghai Zhou**, “China’s “Great Migration”: The impact of the reduction in trade policy uncertainty,” *Journal of International Economics*, 2019, 120, 126–144.
- Feler, Leo and Mine Z Senses**, “Trade shocks and the provision of local public goods,” *American Economic Journal: Economic Policy*, 2017, 9 (4), 101–43.
- Firpo, Sergio, Nicole M. Fortin, and Thomas Lemieux**, “Unconditional quantile regressions,” *Econometrica*, 2009, 77 (3), 953–973.
- Fletcher, Jason M**, “Adolescent depression and educational attainment: results using sibling fixed effects,” *Health Economics*, 2010, 19 (7), 855–871.
- Furukawa, Toshiaki A, Ronald C Kessler, Tim Slade, and Gavin Andrews**, “The performance of the K6 and K10 screening scales for psychological distress in the Australian National Survey of Mental Health and Well-Being,” *Psychological Medicine*, 2003, 33 (2), 357.
- Fushimi, Masahito, Seiji Saito, Tetsuo Shimizu, Yasutsugu Kudo, Masayuki Seki, and Katsuyuki Murata**, “Prevalence of psychological distress, as measured by the Kessler 6 (K6), and related factors in Japanese employees,” *Community Mental Health Journal*, 2012, 48 (3), 328–335.
- Gao, Xuwen, Wenquan Liang, Ahmed Mushfiq Mobarak, and Ran Song**, “Daughters Left Behind: How Trade Liberalization Harms Girls in China when Government Restricts Migration,” *Working Paper*, 2024.
- Greenberger, Ellen, Chuansheng Chen, Steven R Tally, and Qi Dong**, “Family, peer, and individual correlates of depressive symptomatology among US and Chinese adolescents,” *Journal of Consulting and Clinical Psychology*, 2000, 68 (2), 209.

- Hammen, Constance, Patricia A Brennan, Danielle Keenan-Miller, and Nathaniel R Herr,** “Early onset recurrent subtype of adolescent depression: Clinical and psychosocial correlates,” *Journal of Child Psychology and Psychiatry*, 2008, 49 (4), 433–440.
- Heckman, James J,** “Skill formation and the economics of investing in disadvantaged children,” *Science*, 2006, 312 (5782), 1900–1902.
- , “The economics, technology, and neuroscience of human capability formation,” *Proceedings of the National Academy of Sciences*, 2007, 104 (33), 13250–13255.
- , “The developmental origins of health,” *Health Economics*, 2012, 21 (1), 24.
- Heckman, James J.,** “The developmental origins of health,” *Health Economics*, 2012, 21 (1), 24–29.
- Heckman, James J and Junjian Yi,** “Human capital, economic growth, and inequality in China,” Technical Report, National Bureau of Economic Research 2012.
- Jabeen, Farah, M Anis ul Haque, and Muhammad Naveed Riaz,** “Parenting styles as predictors of emotion regulation among adolescents.,” *Pakistan Journal of Psychological Research*, 2013, 28 (1).
- Jaworska, Natalia and Glenda MacQueen,** “Adolescence as a unique developmental period,” *Journal of Psychiatry & Neuroscience: JPN*, 2015, 40 (5), 291.
- Keenan-Miller, Danielle, Constance L Hammen, and Patricia A Brennan,** “Health outcomes related to early adolescent depression,” *Journal of Adolescent Health*, 2007, 41 (3), 256–262.
- Kennard, Betsy D, Greg N Clarke, V Robin Weersing, Joan Rosenbaum Asarnow, Wael Shamseddeen, Giovanna Porta, Michele Berk, Jennifer L Hughes, Anthony Spirito, Graham J Emslie et al.,** “Effective components of TORDIA cognitive-behavioral therapy for adolescent depression: Preliminary findings.,” *Journal of Consulting and Clinical Psychology*, 2009, 77 (6), 1033.
- Kessler, R C, Gavin Andrews, L J Colpe, Eva Hiripi, Mroczek D K, SL T Norman, E E Walters, and A M Zaslavsky,** “Short screening scales to monitor population prevalences and trends in non-specific psychological distress,” *Psychological Medicine*, 2002, 32 (6), 959.
- Kessler, Ronald C., Patricia Berglund, Olga Demler, Robert Jin, Doreen Koretz, Kathleen R. Merikangas, A. John Rush, Ellen E. Walters, and Philip S. Wang,** “The epidemiology of major depressive disorder: Results from the National Comorbidity Survey Replication (NCS-R),” *JAMA*, 2003, 289 (23), 3095–3105.
- Kessler, Ronald C, Patricia Berglund, Olga Demler, Robert Jin, Doreen Koretz, Kathleen R Merikangas, A John Rush, Ellen E Walters, and Philip S Wang,** “The epidemiology of major depressive disorder: Results from the National Comorbidity Survey Replication (NCS-R),” *Jama*, 2003, 289 (23), 3095–3105.
- , – , – , – , – , **Kathleen R Merikangas, and Ellen E Walters,** “Lifetime prevalence and age-of-onset distributions of DSM-IV disorders in the National Comorbidity Survey Replication,” *Archives of General Psychiatry*, 2005, 62 (6), 593–602.
- Khandelwal, Amit, Peter K. Schott, and Shang-Jin Wei,** “Trade liberalization and embedded institutional reform: Evidence from Chinese exporters,” *American Economic Review*, 2013, 103 (6), 2169–2195.

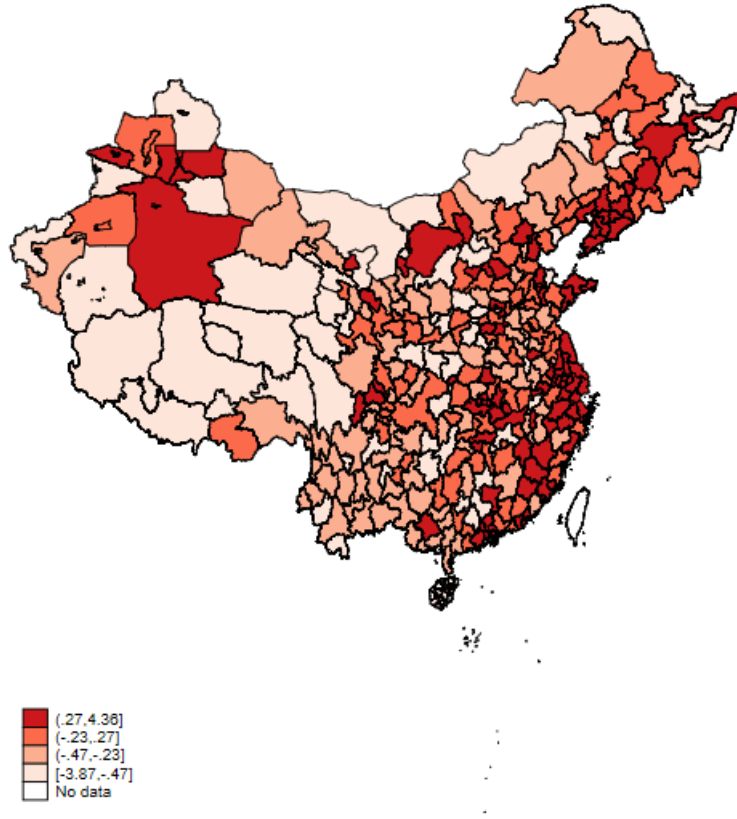
- Khanna, Gaurav, Kevin Shih, Ariel Weinberger, Mingzhi Xu, and Miaojie Yu**, “Trade liberalization and Chinese students in US higher education,” *Review of Economics and Statistics*, 2025, 107 (5), 1291–1309.
- Knickmeyer, Rebecca C, Sylvain Gouttard, Chaeryon Kang, Dianne Evans, Kathy Wilber, J Keith Smith, Robert M Hamer, Weili Lin, Guido Gerig, and John H Gilmore**, “A structural MRI study of human brain development from birth to 2 years,” *Journal of neuroscience*, 2008, 28 (47), 12176–12182.
- Knudsen, Eric I, James J Heckman, Judy L Cameron, and Jack P Shonkoff**, “Economic, neurobiological, and behavioral perspectives on building America’s future workforce,” *Proceedings of the National Academy of Sciences*, 2006, 103 (27), 10155–10162.
- kun Kang, Yu, Wan jun Guo, Hao Xu, Yue hui Chen, Xiao jing Li, Zheng ping Tan, Na Li, Ze ren Gesang, Ying mei Wang, Chang bo Liu et al.**, “The 6-item Kessler psychological distress scale to survey serious mental illness among Chinese undergraduates: Psychometric properties and prevalence estimate,” *Comprehensive Psychiatry*, 2015, 63, 105–112.
- Lewandowski, R Eric, Mary C Acri, Kimberly E Hoagwood, Mark Olfson, Greg Clarke, William Gardner, Sarah Hudson Scholle, Sepheen Byron, Kelly Kelleher, Harold A Pincus et al.**, “Evidence for the management of adolescent depression,” *Pediatrics*, 2013, 132 (4), e996–e1009.
- Li, Bingjing**, “Export expansion, skill acquisition and industry specialization: Evidence from China,” *Journal of International Economics*, 2018, 114, 346–361.
- Li, Jia-Yu, Jing Li, Jing-Hong Liang, Sheng Qian, Rui-Xia Jia, Ying-Quan Wang, and Yong Xu**, “Depressive symptoms among children and adolescents in China: A systematic review and meta-analysis,” *Medical Science Monitor: International Medical Journal of Experimental and Clinical Research*, 2019, 25, 7459.
- Li, Wen, Yuan Yang, Zi-Han Liu, Yan-Jie Zhao, Ling Zhang, Teris Cheung, Chee H Ng, and Yu-Tao Xiang**, “The first national action plan on depression in China: Progress and challenges,” *The Lancet Regional Health–Western Pacific*, 2021, 7.
- Liu, Elaine M, Jin-Tan Liu, and Tzu-Yin Hazel Tseng**, “Fetal origins of mental health: Evidence from natural disasters in Taiwan,” *Working Paper*, 2016.
- Liu, Jianghong, Libo Li, and Fan Fang**, “Psychometric properties of the Chinese version of the Parental Bonding Instrument,” *International Journal of Nursing Studies*, 2011, 48 (5), 582–589.
- Liu, Qiren, Sen Luo, and Robert Seamans**, “Pain or anxiety? The health consequences of rising robot adoption in China,” *Economics Letters*, 2024, 236, 111582.
- Malaspina, Dolores, C Corcoran, KR Kleinhaus, MC Perrin, S Fennig, D Nahon, Y Friedlander, and S Harlap**, “Acute maternal stress in pregnancy and schizophrenia in offspring: a cohort prospective study,” *BMC Psychiatry*, 2008, 8 (1), 1–9.
- March, John, Susan Silva, Stephen Petrycki, John Curry, Karen Wells, John Fairbank, Barbara Burns, Marisa Domino, Steven McNulty, Benedetto Vitiello et al.**, “Fluoxetine, cognitive-behavioral therapy, and their combination for adolescents with depression: Treatment for Adolescents With Depression Study (TADS) randomized controlled trial,” *Jama*, 2004, 292 (7), 807–820.

- Marmorstein, Naomi R, William G Iacono, and Lisa Legrand**, “Obesity and depression in adolescence and beyond: reciprocal risks,” *International Journal of Obesity*, 2014, 38 (7), 906–911.
- Mathers, Colin D and Dejan Loncar**, “Projections of global mortality and burden of disease from 2002 to 2030,” *PLoS Medicine*, 2006, 3 (11), e442.
- McCaig, Brian and Nina Pavcnik**, “Export markets and labor allocation in a low-income country,” *American Economic Review*, 2018, 108 (7), 1899–1941.
- McEwen, Bruce S.**, “Physiology and neurobiology of stress and adaptation: Central role of the brain,” *Physiological Reviews*, 2007, 87 (3), 873–904.
- McManus, T Clay and Georg Schaur**, “The effects of import competition on worker health,” *Journal of International Economics*, 2016, 102, 160–172.
- Milkie, Melissa A, Kei M Nomaguchi, and Kathleen E Denny**, “Does the amount of time mothers spend with children or adolescents matter?,” *Journal of Marriage and Family*, 2015, 77 (2), 355–372.
- National Health Commission of China**, “Notice of the general office of the national health commission on the exploration and implementation of special services for the prevention and treatment of depression and Senile dementia (in Chinese),” <http://www.nhc.gov.cn/jkj/s7914/202009/a63d8f82eb53451f97217bef0962b98f.shtml> 2020. Retrieved March 16, 2022.
- National Institute of Mental Health**, “Major Depression,” <https://www.nimh.nih.gov/health/statistics/major-depression> 2022. Retrieved June 27, 2024.
- Niu, Yingnan, Xiaolin Guo, He Cai, and Liang Luo**, “The relation between family socioeconomic status and depressive symptoms among children and adolescents in mainland China: a meta-analysis,” *Frontiers in Public Health*, 2024, 11, 1292411.
- Nunn, Nathan**, “Relationship-specificity, incomplete contracts, and the pattern of trade,” *The Quarterly Journal of Economics*, 2007, 122 (2), 569–600.
- Page, Marianne, Jessamyn Schaller, and David Simon**, “The effects of aggregate and gender-specific labor demand shocks on child health,” *Journal of Human Resources*, 2019, 54 (1), 37–78.
- Parker, Gordon, Hilary Tupling, and Laurence B Brown**, “A parental bonding instrument,” *British Journal of Medical Psychology*, 1979, 52 (1), 1–10.
- Pierce, Justin and Peter Schott**, “The surprisingly swift decline of U.S. manufacturing employment,” *American Economic Review*, 2016, 106 (7), 1632–1662.
- Pierce, Justin R and Peter K Schott**, “Trade liberalization and mortality: evidence from US counties,” *American Economic Review: Insights*, 2020, 2 (1), 47–64.
- Radloff, Lenore Sawyer**, “The CES-D scale: A self-report depression scale for research in the general population,” *Applied Psychological Measurement*, 1977, 1 (3), 385–401.
- Rankin, Sally H, Michael E Galbraith, and Sharon Johnson**, “Reliability and validity data for a Chinese translation of the Center for Epidemiological Studies-Depression,” *Psychological Reports*, 1993, 73 (3_suppl), 1291–1298.

- Rauch, James E**, “Networks versus markets in international trade,” *Journal of International Economics*, 1999, 48 (1), 7–35.
- Richter, Linda M, Bernadette Daelmans, Joan Lombardi, Jody Heymann, Florencia Lopez Boo, Jere R Behrman, Chunling Lu, Jane E Lucas, Rafael Perez-Escamilla, Tarun Dua et al.**, “Investing in the foundation of sustainable development: pathways to scale up for early childhood development,” *The lancet*, 2017, 389 (10064), 103–118.
- Rohde, Paul, Peter M Lewinsohn, Daniel N Klein, John R Seeley, and Jeff M Gau**, “Key characteristics of major depressive disorder occurring in childhood, adolescence, emerging adulthood, and adulthood,” *Clinical Psychological Science*, 2013, 1 (1), 41–53.
- Roth, Jonathan**, “Pretest with caution: Event-study estimates after testing for parallel trends,” *American Economic Review: Insights*, 2022, 4 (3), 305–322.
- Rushton, Jerry L, Michelle Forcier, and Robin M Schectman**, “Epidemiology of depressive symptoms in the National Longitudinal Study of Adolescent Health,” *Journal of the American Academy of Child & Adolescent Psychiatry*, 2002, 41 (2), 199–205.
- Steffick, Diane E et al.**, “Documentation of affective functioning measures in the Health and Retirement Study,” *Ann Arbor, MI: University of Michigan*, 2000.
- Steinberg, Laurence, Nina S Mounts, Susie D Lamborn, and Sanford M Dornbusch**, “Authoritative parenting and adolescent adjustment across varied ecological niches,” in “Adolescents and Their Families,” Routledge, 2013, pp. 129–146.
- Susser, Ezra, Hans W Hoek, and Alan Brown**, “Neurodevelopmental disorders after prenatal famine: the story of the Dutch Famine Study,” *American Journal of Epidemiology*, 1998, 147 (3), 213–216.
- Thapar, Anita, Stephan Collishaw, Daniel S Pine, and Ajay K Thapar**, “Depression in adolescence,” *The Lancet*, 2012, 379 (9820), 1056–1067.
- Tolbert, Charles M and Molly Sizer**, “US commuting zones and labor market areas: A 1990 update,” Technical Report 1996.
- Tong, Lian, Qiong Yan, and Ichiro Kawachi**, “The factors associated with being left-behind children in China: Multilevel analysis with nationally representative data,” *PloS One*, 2019, 14 (11), e0224205.
- Uji, Masayo, Ayuko Sakamoto, Keiichiro Adachi, and Toshinori Kitamura**, “The impact of authoritative, authoritarian, and permissive parenting styles on children’s later mental health in Japan: Focusing on parent and child gender,” *Journal of Child and Family Studies*, 2014, 23 (2), 293–302.
- wei Hu, Teh, Yanling He, Mingyuan Zhang, and Ningshan Chen**, “Economic costs of depression in China,” *Social Psychiatry and Psychiatric Epidemiology*, 2007, 42 (2), 110–116.
- Wilson, Syla, Brian M Hicks, Katherine T Foster, Matt McGue, and William G Iacono**, “Age of onset and course of major depressive disorder: associations with psychosocial functioning outcomes in adulthood,” *Psychological Medicine*, 2015, 45 (3), 505–514.
- World Health Organization**, “Depression and other common mental disorders,” Technical Report, World Health Organization Geneva, Switzerland 2017.
- , “The WHO special initiative for mental health (2019-2023): universal health coverage for mental health,” Technical Report, World Health Organization 2019.

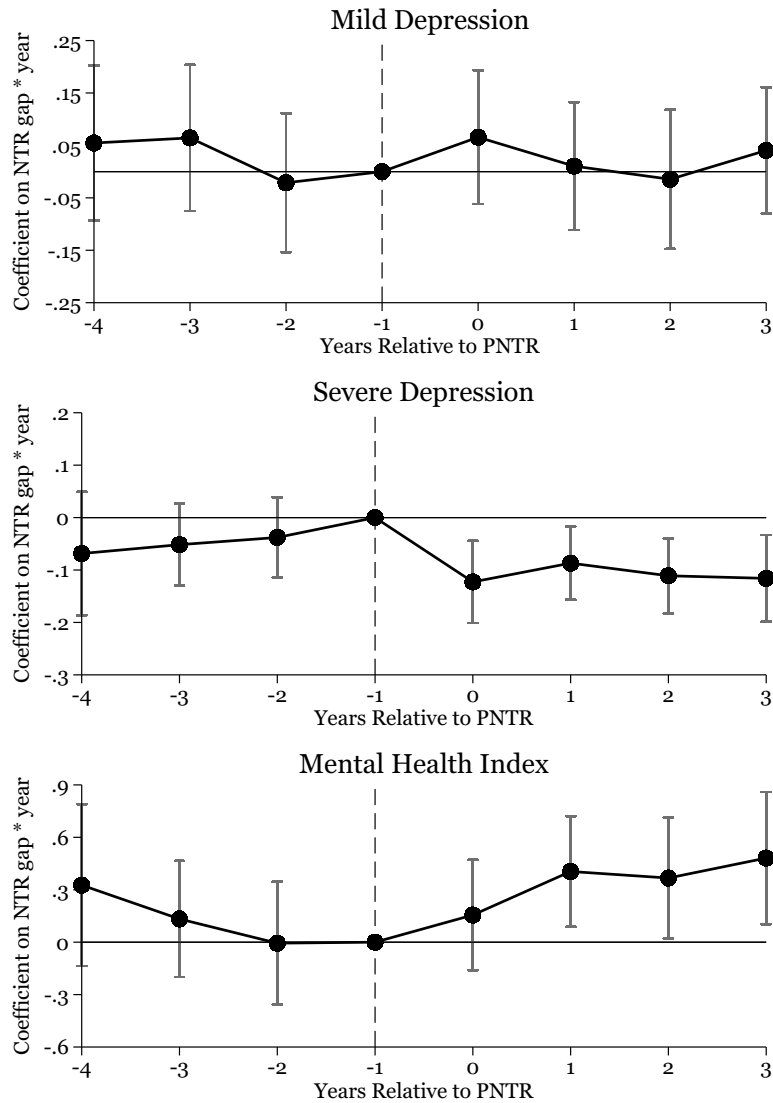
- yan Chen, Zhi, Xiao dong Yang, and Xin ying Li**, “Psychometric features of CES-D in Chinese adolescents.,” *Chinese Journal of Clinical Psychology*, 2009.
- Zhang, Jie and Jill M Norvilitis**, “Measuring Chinese psychological well-being with Western developed instruments,” *Journal of Personality Assessment*, 2002, 79 (3), 492–511.
- Zhou, Mi, Guangsheng Zhang, Scott Rozelle, Kaleigh Kenny, and Hao Xue**, “Depressive symptoms of Chinese children: prevalence and correlated factors among subgroups,” *International Journal of Environmental Research and Public Health*, 2018, 15 (2), 283.
- Zhou, Weina and Shun Wang**, “Early childhood health shocks, classroom environment, and social-emotional outcomes,” *Journal of Health Economics*, 2023, 87, 102698.

FIGURE 1: PREFECTURE-LEVEL EXPOSURE TO PNTR



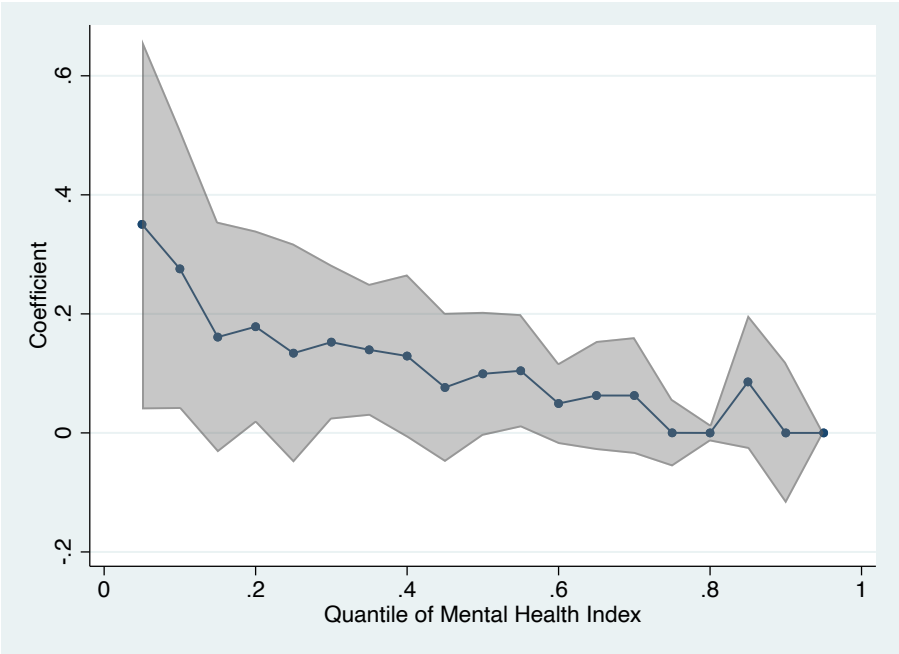
Note: This figure plots prefecture-level exposure to PNTR, computed as the employment-share weighted-average NTR gap across all of the Chinese three-digit industries in 1999. Employment data are from the 1990 population census. Data on the industry-level NTR gap are from [Pierce and Schott \(2016\)](#).

FIGURE 2: EVENT STUDY: EXPOSURE TO PNTR AND MENTAL HEALTH



Note: This figure plots the coefficients and 95% confidence intervals from an event-study regression that compares the incidence of mild depression (Panel A), severe depression (Panel B) and mental health index (Panel C) in prefectures that are more exposed to the PNTR shock to those that are less exposed in each year of the survey before and after the policy change. The omitted category is for one year before the PNTR policy. Data are from the 2016–2018 CFPS.

FIGURE 3: DISTRIBUTIONAL EFFECTS OF EXPOSURE TO PNTR ON MENTAL HEALTH INDEX



Note: This figure plots the distributional effects of exposure to PNTR on the mental health index constructed using the CES-D scores.

TABLE 1A: SUMMARY STATISTICS

	Obs (1)	Mean (2)	S.D. (3)	Min (4)	Max (5)
Panel A: CFPS 2016-2018: Cohorts born between 1998 and 2005					
<i>Health and cognition outcomes</i>					
Mild depression	4,749	0.147	0.354	0.000	1.000
Severe depression	4,749	0.060	0.238	0.000	1.000
Mental health index	4,746	0.000	1.000	-6.550	1.227
Physical health	4,724	0.000	1.000	-4.816	0.439
Cognitive function index	3,929	-0.000	1.000	-1.469	1.106
Dropout before high school	4,749	0.115	0.320	0.000	1.000
<i>Demographic characteristics</i>					
Age	4,749	15.324	2.540	11.000	20.000
Male	4,749	0.530	0.499	0.000	1.000
Father's age	4,749	43.665	5.456	29.000	83.000
Mother's age	4,749	41.796	5.336	28.000	82.000
Completed middle school- Father	4,749	0.526	0.499	0.000	1.000
Completed middle school- Mother	4,749	0.404	0.491	0.000	1.000
Panel B: CHNS 2000-2015: Cohorts born between 1998 and 2005					
<i>PHF: early life investment</i>					
Months of breast-feeding	412	11.216	5.064	0.000	36.000
No. of vaccinations	698	2.066	2.449	0.000	9.000
BCG vaccination	699	0.228	0.420	0.000	1.000
Hepatitis B vaccination	698	0.278	0.448	0.000	1.000
DPT vaccination	699	0.289	0.454	0.000	1.000
Encephalities B vaccination	698	0.234	0.423	0.000	1.000
Measles vaccination	698	0.254	0.435	0.000	1.000
Polio vaccination	698	0.364	0.481	0.000	1.000
<i>Child sample: nutrition intake over the past 3 days (children ages 0-12)</i>					
Calories	1,940	1322.320	515.613	238.167	4646.358
Protein	1,940	43.622	19.831	5.762	174.500
Carbohydrate	1,940	180.081	73.346	25.084	472.531
Fat	1,940	47.420	29.411	1.482	422.754
<i>Child sample: child development (children ages 0-12)</i>					
Height (cm)	1,883	120.689	19.743	66.000	172.500
Weight (kg)	1,940	24.993	10.916	8.500	90.600
BMI-for-Age (z-score)	1,883	0.576	2.438	-4.730	48.680
Overweight	1,940	0.024	0.152	0.000	1.000
Obese	1,940	0.013	0.113	0.000	1.000

Notes: Panels A and B present the summary statistics for child development variables and demographic characteristics from the 2016 and 2018 CFPS samples and from the 2000, 2004, 2006, 2009, 2011 and 2015 CHNS samples, respectively. All variables are summarized at the child level.

TABLE 1B: SUMMARY STATISTICS

	Obs (1)	Mean (2)	S.D. (3)	Min (4)	Max (5)
Panel A: CHNS 2000-2015: Cohorts born between 1997 and 2006 (Cont')					
<i>Childcare from people outside the household (children ages 0-6)</i>					
Hours per day in a typical day	866	3.099	4.740	0.000	24.000
Days per week in a typical week	1,025	1.779	2.523	0.000	7.000
<i>Parental absence for seeking employment elsewhere (children ages 0-6)</i>					
Father was not living in the household	1,347	0.013	0.115	0.000	1.000
Mother was not living in the household	1,348	0.001	0.027	0.000	1.000
<i>Demographic characteristics (children ages 0-12)</i>					
Age	1,940	6.703	2.864	0.000	12.000
Male	1,940	0.561	0.496	0.000	1.000
Father's age	1,940	35.796	5.991	22.000	70.000
Mother's age	1,940	34.128	5.567	20.000	61.000
Father completed middle school	1,940	0.753	0.432	0.000	1.000
Mother completed middle school	1,940	0.670	0.470	0.000	1.000
Panel B: CHNS 1991-2015					
<i>Women pregnant during the sample period of survey</i>					
Prenatal visits	1,840	5.111	4.986	0.000	78.000
Panel C: City Statistical Yearbook 1995-2015					
Labor income per worker (in log)	5,628	8.419	2.555	-0.085	14.233
Panel D: Census 1990-2015					
<i>Female sample: share of margins of labor market adjustment to females aged 20-55</i>					
Total employment	1,655	0.766	0.127	0.251	0.990
Employment in agriculture	1,655	0.461	0.236	0.000	0.981
Employment in manufacturing	1,655	0.100	0.099	0.000	0.713
Employment in service	1,655	0.204	0.104	0.004	0.778
Unemployed	1,655	0.035	0.029	0.000	0.222
Not in the labor force	1,655	0.200	0.110	0.010	0.746
<i>Male sample: share of margins of labor market adjustment to males aged 22-60</i>					
Total employment	1,655	0.909	0.055	0.603	0.990
Employment in agriculture	1,655	0.485	0.217	0.003	0.961
Employment in manufacturing	1,655	0.120	0.094	0.000	0.695
Employment in service	1,655	0.304	0.130	0.024	0.898
Unemployed	1,655	0.027	0.024	0.000	0.205
Not in the labor force	1,655	0.065	0.036	0.008	0.306

Notes: Panel C presents the summary statistics of the number of prenatal visits from the 1991, 1993, 1997, 2000, 2004, 2006, 2009, 2011 and 2015 CHNS pregnant women samples. Panel D presents the summary statistics of labor income and labor income per worker (in log) from the 1995-2015 City Statistical Yearbooks. Panel E presents the summary statistics of employment variables separately for the female working age population (aged 20 to 55 years) and male working age population (aged 22 to 60 years) from the 1990, 2000, 2005, 2010, and 2015 population census samples. All variables are summarized at the prefecture level.

TABLE 2: IMPACT OF PNTR ON ADOLESCENT MENTAL HEALTH OUTCOMES

	(1)	(2)	(3)
Panel A: Mild depression			
Post × NTR gap	0.004 (0.012)	-0.001 (0.015)	-0.001 (0.015)
<i>p</i> -value	[0.736]	[0.955]	[0.955]
<i>q</i> -value	⟨0.326⟩	⟨0.486⟩	⟨0.467⟩
Observations	4749	4749	4749
Pre-treatment mean	0.17	0.17	0.17
Panel B: Severe depression			
Post × NTR gap	-0.024*** (0.008)	-0.031*** (0.010)	-0.032*** (0.010)
<i>p</i> -value	[0.003]	[0.001]	[0.001]
<i>q</i> -value	⟨0.011⟩	⟨0.005⟩	⟨0.004⟩
Observations	4749	4749	4749
Pre-treatment mean	0.07	0.07	0.07
Panel C: Mental health index			
Post × NTR gap	0.069** (0.033)	0.116*** (0.038)	0.117*** (0.038)
<i>p</i> -value	[0.041]	[0.003]	[0.003]
<i>q</i> -value	⟨0.044⟩	⟨0.005⟩	⟨0.004⟩
Observations	4746	4746	4746
Pre-treatment mean	-0.06	-0.06	-0.06
Birth prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Birth year fixed effects × Other trade policies	Yes	Yes	Yes
× Initial prefecture characteristics		Yes	Yes
Individual characteristics			Yes

Notes: Data are from the 2016–2018 CFPS. This table reports results of the DiD regressions of adolescent mental health outcomes on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions in column 1 control for survey year fixed effects, birth year fixed effects, prefecture of birth fixed effects, and the birth year fixed effects interacted with other trade policies including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. Regressions in column 2 further control for the birth year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. Regressions in column 3 further control for individual characteristics including age, gender, indicator variables for whether the mother and father completed middle school, father’s age and mother’s age. Standard errors, in parentheses, are clustered at the prefecture of birth level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE 3: IMPACT OF PNTR ON LABOR INCOME PER WORKER

	(1)	(2)
Post \times NTR gap	0.101*** (0.016)	0.048*** (0.015)
Observations	5628	5628
Pre-treatment mean	7.90	7.90
Prefecture fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Year fixed effects		
\times Other trade policies	Yes	Yes
\times Initial prefecture characteristics		Yes

Notes: Labor income per worker comes from the 1995–2015 China City Statistical Yearbooks. This table reports results of the DiD regressions of labor income per worker on interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions in columns 1 control for prefecture fixed effects, year fixed effects, and the year fixed effects interacted with other trade policies including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. Regressions in columns 2 further control for year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. Standard errors are clustered at the prefecture level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE 4: IMPACT OF PNTR ON FEMALE AND MALE EMPLOYMENT STATUS

	All sectors (1)	Agri (2)	Manu (3)	Service (4)	Unemployed (5)	NILF (6)
Panel A: Female						
Post × NTR gap	-0.009** (0.004)	-0.046*** (0.005)	0.016*** (0.003)	0.020*** (0.003)	0.000 (0.001)	0.009** (0.004)
<i>p</i> -value	[0.017]	[0.000]	[0.000]	[0.000]	[0.773]	[0.019]
<i>q</i> -value	⟨0.012⟩	⟨0.001⟩	⟨0.001⟩	⟨0.001⟩	⟨0.148⟩	⟨0.012⟩
Observations	1655	1655	1655	1655	1655	1655
Pre-treatment mean	0.82	0.59	0.09	0.14	0.02	0.16
Panel B: Male						
Post × NTR gap	0.004* (0.002)	-0.040*** (0.005)	0.015*** (0.003)	0.029*** (0.005)	0.000 (0.001)	-0.004** (0.001)
<i>p</i> -value	[0.054]	[0.000]	[0.000]	[0.000]	[0.998]	[0.016]
<i>q</i> -value	⟨0.026⟩	⟨0.001⟩	⟨0.001⟩	⟨0.001⟩	⟨0.200⟩	⟨0.013⟩
Observations	1655	1655	1655	1655	1655	1655
Pre-treatment mean	0.94	0.61	0.11	0.22	0.02	0.04
Prefecture fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects						
× Other trade policies	Yes	Yes	Yes	Yes	Yes	Yes
× Initial prefecture characteristics	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data are from the 1990, 2000, 2005, 2010, and 2015 population censuses in China. This table reports results of the DiD regressions of margins of local labor market adjustment on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. All regressions control for prefecture fixed effects, year fixed effects, and the year fixed effects interacted with other trade policies including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, and contract intensity. They also control for the survey year fixed effects interacted with initial prefecture characteristics including GDP per capita, the share of employment in manufacturing, and the distance to the nearest port. Regressions in Panel A consider marriageable and working-age women (ages 20-55). Regressions in Panel B consider marriageable and working-age men (ages 22-60). Regressions are weighted by the 1990 prefecture population. Standard errors, in parentheses, are clustered at the prefecture level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE 5: IMPACT OF PNTR ON EARLY LIFE INVESTMENTS

	Prenatal visits (1)	Months of breastfeeding (2)	Number of vaccinations (3)	BCG vaccination (4)	Hepatitis B vaccination (5)
Post × NTR gap	1.194*	1.818**	0.711***	0.142***	0.138***
	(0.691)	(0.742)	(0.205)	(0.043)	(0.043)
<i>p</i> -value	[0.090]	[0.018]	[0.001]	[0.002]	[0.002]
<i>q</i> -value	⟨0.048⟩	⟨0.019⟩	⟨0.006⟩	⟨0.006⟩	⟨0.006⟩
Observations	1840	465	698	698	698
Pre-treatment mean	3.26	11.77	1.85	0.15	0.20
Prefecture fixed effects	Yes	Yes	Yes	Yes	Yes
Survey year fixed effects	Yes				
Birth year fixed effects		Yes	Yes	Yes	Yes
Survey/Birth year fixed effects					
× Other trade policies	Yes	Yes	Yes	Yes	Yes
× Initial prefecture characteristics	Yes	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes	Yes
	DPT vaccination (6)	Encephalitis B vaccination (7)	Measles vaccination (8)	Polio vaccination (9)	
Post × NTR gap	0.186***	0.029	0.128***	0.122**	
	(0.042)	(0.043)	(0.040)	(0.054)	
<i>p</i> -value	[0.000]	[0.506]	[0.003]	[0.028]	
<i>q</i> -value	⟨0.001⟩	⟨0.159⟩	⟨0.006⟩	⟨0.025⟩	
Observations	698	698	698	698	
Pre-treatment mean	0.23	0.26	0.22	0.35	
Prefecture fixed effects	Yes	Yes	Yes	Yes	
Birth year fixed effects	Yes	Yes	Yes	Yes	
Birth year fixed effects					
× Other trade policies	Yes	Yes	Yes	Yes	
× Initial prefecture characteristics	Yes	Yes	Yes	Yes	
Individual characteristics	Yes	Yes	Yes	Yes	

Notes: Prenatal visit information comes from data of pregnant women for the sample period of 1991-2015 CHNS. The regression in column (1) controls prefecture and survey year fixed effects and survey year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. The regression also controls for other trade policies including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity and individual characteristics including the child’s age and gender, mother’s age and an indicator variable for whether the mother completed middle school. Information on breastfeeding duration and vaccinations reported in columns (2)–(9) comes from data on children born from 1998 to 2005 included in the CHNS PHF. Regressions control for the child’s birth year fixed effects, prefecture fixed effects, birth year fixed effects interacted with other trade policies including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. Regressions also control for the birth year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port and individual characteristics including the child’s age and gender, mother’s age and an indicator variable for whether the mother completed middle school. Standard errors, in parentheses, are clustered at the prefecture level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE 6: IMPACT OF PNTR ON NUTRITION INTAKE AND CHILD DEVELOPMENT OUTCOMES

	(1)	(2)	(3)
Panel A: Total Calories			
Post × NTR gap	78.329*** (20.589)	74.138** (32.090)	49.820* (26.938)
<i>p</i> -value	[0.000]	[0.025]	[0.07]
<i>q</i> -value	(0.002)	(0.031)	(0.118)
Observations	1940	1940	1940
Pre-treatment mean	1400.37	1400.37	1400.37
Panel B: Protein			
Post × NTR gap	4.006*** (0.738)	3.679*** (1.186)	2.912*** (1.049)
<i>p</i> -value	[0.000]	[0.003]	[0.008]
<i>q</i> -value	(0.001)	(0.008)	(0.019)
Observations	1940	1940	1940
Pre-treatment mean	45.72	45.72	45.72
Panel C: Carbohydrate			
Post × NTR gap	5.920* (2.968)	3.499 (4.532)	0.261 (4.047)
<i>p</i> -value	[0.052]	[0.444]	[0.949]
<i>q</i> -value	(0.036)	(0.200)	(0.463)
Observations	1940	1940	1940
Pre-treatment mean	193.70	193.70	193.70
Panel D: Fat			
Post × NTR gap	4.345*** (1.211)	5.107*** (1.524)	4.186*** (1.363)
<i>p</i> -value	[0.001]	[0.002]	[0.003]
<i>q</i> -value	(0.002)	(0.007)	(0.014)
Observations	1940	1940	1940
Pre-treatment mean	49.12	49.12	49.12
Prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Birth year fixed effects × Other trade policies	Yes	Yes	Yes
× Initial prefecture characteristics		Yes	Yes
Individual characteristics			Yes

Notes: Nutrition intakes and child development outcomes come from data on children aged 0-12 who were born from 1998 to 2005 and included in the CHNS. This table reports results of the DiD regressions of nutrition intake on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. The regression in column 1 controls for birth year fixed effects, prefecture fixed effects, and the birth year fixed effects interacted with other trade policies including China's output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. The regression in column 2 further controls for the birth year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. The regression in column 3 further controls for individual characteristics including child age, child gender, indicator variables for whether the mother and father completed middle school, father's age and mother's age. Standard errors, in parentheses, are clustered at the prefecture of birth level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE 6: IMPACT OF PNTR ON NUTRITION INTAKE AND CHILD DEVELOPMENT OUTCOMES (continued)

	(1)	(2)	(3)
Panel E: Height			
Post × NTR gap	1.983*** (0.615)	2.572** (1.043)	0.536 (0.553)
<i>p</i> -value	[0.002]	[0.017]	[0.338]
<i>q</i> -value	<0.004>	<0.027>	<0.228>
Observations	1883	1883	1883
Pre-treatment mean	126.47	126.47	126.47
Panel F: Weight			
Post × NTR gap	1.481*** (0.517)	2.732*** (0.721)	1.965*** (0.473)
<i>p</i> -value	[0.006]	[0.000]	[0.000]
<i>q</i> -value	<0.007>	<0.004>	<0.002>
Observations	1940	1940	1940
Pre-treatment mean	27.81	27.81	27.81
Panel G: BMI for Age (z-score)			
Post × NTR gap	-0.004 (0.198)	0.340 (0.207)	0.315 (0.200)
<i>p</i> -value	[0.982]	[0.106]	[0.120]
<i>q</i> -value	<0.491>	<0.077>	<0.141>
Observations	1883	1883	1883
Pre-treatment mean	0.71	0.71	0.71
Panel H: Overweight			
Post × NTR gap	0.003 (0.007)	0.014 (0.011)	0.018 (0.011)
<i>p</i> -value	[0.660]	[0.200]	[0.124]
<i>q</i> -value	<0.395>	<0.129>	<0.141>
Observations	1940	1940	1940
Pre-treatment mean	0.01	0.01	0.01
Panel I: Obese			
Post × NTR gap	-0.000 (0.007)	-0.004 (0.010)	-0.002 (0.010)
<i>p</i> -value	[0.988]	[0.674]	[0.838]
<i>q</i> -value	<0.491>	<0.290>	<0.459>
Observations	1940	1940	1940
Pre-treatment mean	0.01	0.01	0.01
Prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Birth year fixed effects × Other trade policies	Yes	Yes	Yes
× Initial prefecture characteristics		Yes	Yes
Individual characteristics			Yes

Notes: See notes in the first part of the table (Panels A–D).

TABLE 7: IMPACT OF PNTR ON CHILDCARE PROVISION

	(1)	(2)	(3)
Panel A: Hours per day cared for by people outside the household in a typical day			
Post × NTR gap	-1.091*** (0.395)	-2.842*** (0.503)	-2.756*** (0.484)
<i>p</i> -value	[0.008]	[0.000]	[0.000]
<i>q</i> -value	<0.024>	<0.001>	<0.001>
Observations	880	880	880
Pre-treatment mean	3.94	3.94	3.94
Panel B: Days per week cared for by people outside the household in a typical week			
Post × NTR gap	-0.579** (0.248)	-1.071*** (0.282)	-1.016*** (0.287)
<i>p</i> -value	[0.024]	[0.000]	[0.001]
<i>q</i> -value	<0.024>	<0.001>	<0.002>
Observations	1025	1025	1025
Pre-treatment mean	2.18	2.18	2.18
Prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Birth year fixed effects × Other trade policies	Yes	Yes	Yes
× Initial prefecture characteristics		Yes	Yes
Individual characteristics			Yes

Notes: Childcare provision information comes from data on children aged 0-6 who were born from 1998 to 2005 and included in the CHNS. This table reports results of the DiD regressions of nutrition intake on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. The regression in column 1 controls for birth year fixed effects, prefecture fixed effects, and the birth year fixed effects interacted with other trade policies including China's output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. The regression in column 2 further controls for the birth year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. The regression in column 3 further controls for individual characteristics including child age, child gender, indicator variables for whether the mother and father completed middle school, father's age and mother's age. Standard errors, in parentheses, are clustered at the prefecture of birth level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

FOR ONLINE PUBLICATION

Appendix A Definition and Data Sources of Control Variables

A.1 Prefecture-Level Characteristics

The prefecture-level characteristics used as controls in the regressions are listed below. All the variables are measured in 1990. Data on GDP per capita are obtained from the China City Statistical Yearbook of 1990.

The following four variables are calculated using the 1990 China population census.

- Manufacturing employment share: The ratio of the population employed in manufacturing to the total working-age population. Data are calculated using the 1990 China population census.
- Distance to the nearest port: We first collect information about each prefecture's latitude and longitude from China Data Online to calculate its distance to its nearest port. Geographical information on port locations (specific coordinates) is extracted from the World Port Index. We can then calculate the prefecture's distance to its nearest port.

A.2 Policy Controls

Other policy controls used in the main regressions are listed below.

- Output tariff: Data on output tariff at the HS-6 product level are obtained from the World Integrated Trade Solution (WITS) database. The HS-6 product level data are aggregated to 3-digit industry classification in the 1990 census data, using a concordance table between the Chinese Industrial Classification (CIC) system and HS codes. The simple average tariff for each 3-digit industry is then computed. The prefectures' exposure to output tariff is measured using the 1990 employment-share-weighted-average tariff in 2001 across 3-digit industries in the prefecture as in Eq. (1), i.e., $\tau_p^o = \sum_j S_{jp}^{1990} \times \tau_{j,2001}^o$. Here, $\tau_{j,2001}^o$ is output tariff of industry j in 2001. The tariff measure τ_p^o is then interacted with the post-PNTR dummy and included in the specification.
- Input tariff: We first calculate the 3-digit industry-level input tariff as a weighted average of the industry-level output tariff, using as the weight the share of inputs in the output value from the China input-output table for 1997. Specifically, input tariff $\tau_j^i = \sum_k \tau_k^o \times \omega_{kj}$, where τ_k^o is output tariff of industry k , and ω_{kj} is the share of inputs from industry k used by industry j , using the 1997 China input-output table. The prefectures' exposure to input tariff as the 1990 employment-share-weighted-average input tariff in 2001 across 3-digit industries in the prefecture as in Eq. (1), i.e., $\tau_p^i = \sum_j S_{jp}^{1990} \times \tau_{j,2001}^i$. We then interact the input tariff τ_p^i with the post-PNTR dummy and include the interaction in the specification.

- External tariff: Data on industry-level external tariff is measured as a weighted average of the destination country's tariffs on China's imports, using China's exports to each destination country as the weight. Specifically, external tariff $\tau_j^e = \sum_d \tau_{dj}^e \times \frac{Y_{dj}}{Y_d}$, where τ_{dj}^e is country d 's tariffs on Chinese imports of industry j , Y_{dj} is China's exports of industry j to destination country d , and Y_d is China's exports to the destination country d . The export data come from the United Nations Comtrade Database. We then compute prefectures' exposure to the external tariff as the 1990 employment-share-weighted-average external tariff in 2001 across 3-digit industries in the prefecture as in Eq. (1), i.e., $\tau_p^e = \sum_j S_{jp}^{1990} \times \tau_{j,2001}^e$, and then interact the external tariff τ_p^e with the post-PNTR dummy and include it in the specification.
- Input relationship-specific index: We proxy barriers to investment in China using an input relationship-specificity index proposed by Nunn (2007). Based on the classifications in Rauch (1999), Nunn (2007) considers goods that are neither reference priced nor sold on exchange markets to be relationship-specific goods and computes the proportion of relationship-specific inputs, for each product in 1987 US input-output table. The 1987 IO industry is mapped to the HS 10-digit product level using concordance provided by the Bureau of Economic Analysis and then averaged to the HS-6 product level. The measure is converted to a 3-digit industry classification in the 1990 China census data, using a concordance table between CIC system and HS codes. We then calculate prefectures' exposure using the 1990 employment-share-weighted-average input relationship-specific index across 3-digit industries in the prefecture as in Eq. (1). The measure is interacted with the post-PNTR dummy and included in the specification.
- MFA exposure: We use data on the Multifiber Arrangement (MFA) "quota-bound" product at the HS 6-digit product level in year 2001 from Khandelwal et al. (2013). The HS 6-digit product level is mapped to the 3-digit Chinese industry level in the census 1990 using the concordance between CIC system and HS codes. Based on these 3-digit industry-level data, we construct a prefecture-level exposure to MFA using employment-share-weighted "quota-bound" product across 3-digit industries in the prefecture as in Eq. (1). The measure is interacted with the post-PNTR dummy and included in the specification.
- Export license: We use information on licensing of exports at the HS 6-digit product level from the Ministry of Foreign Trade and Economic Cooperation and the Ministry of Commerce as a measure of export license. The HS 6-digit product level is mapped to the 3-digit Chinese industry level in the census 1990 using the concordance between CIC system and HS codes. Based on these 3-digit industry-level data, we construct a prefecture-level exposure to export license using employment-share-weighted export license across 3-digit industries in the prefecture as in Eq. (1). The measure is interacted with the post-PNTR dummy and included in the specification.
- NTR rate: We use the U.S. import tariff rate at the HS-6 product level as a measure of NTR

tariff rates. The tariff data are obtained from the WITS database, and then aggregated up to the 3-digit industry classification in the 1990 census data using a concordance table between CIC system and HS codes. The prefecture-level exposure to NTR tariff is computed using employment-share-weighted US import tariff rates in 2001 across 3-digit industries in the prefecture as in Eq. (1). The measure is then interacted with the post-PNTR dummy and is included in the specification.

- FDI liberalization policy: Data on 3-digit industry-level FDI liberalization index ($FDI_{j,t}$) is obtained from the Catalogue for the Guidance of Foreign Investment Industries from 1995 to 2011. The index measures the extent to which industries were subject to FDI policy changes over time, see [Erten et al. \(2024\)](#) for details on the construction of the index. Prefectures' exposure to FDI policy changes are calculated as the 1990 employment-share-weighted-average FDI liberalization across 3-digit industries in the prefecture as in Eq. (1), i.e., $FDI_{pt} = \sum_j S_{jp}^{1990} \times FDI_{j,t}$.
- Robot and food imports: We use Chinese customs data to measure imports of industrial robot and food products. For industrial robots, we identify imports using 6-digit HS code 847950, which includes two 8-digit products: 84795010 (“industrial robots for multiple uses”) and 84795090 (“other industrial robots, including end-effectors”). For food imports, we use all 6-digit products under HS code 1 to 21. We map each 6-digit product code to its corresponding 4-digit Chinese industrial classification, and then aggregate the data to the 3-digit industry level. Prefectures' exposure to robot and food imports are computed as the 1990 employment-share-weighted-average robot and food imports across 3-digit industries in the prefecture as in Eq. (1), i.e., $Import_{pt}^o = \sum_j S_{jp}^{1990} \times Import_{j,t}^o$, where o denotes robot and food imports, respectively.

Appendix B Additional Tables

TABLE A1: Pre-Trend Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome Variable	# pre-periods	# significant	Max t	Joint p-value	t for slope	$\gamma = 0.5$	$\gamma = 0.8$
Mild depression	3	0	0.78	0.584	1.13	0.020	0.031
Severe depression	3	0	1.41	0.533	0.73	0.013	0.020
Mental Health Index	3	0	1.33	0.381	0.93	0.054	0.082

Notes: This table reports the pre-trend analysis of the depression outcomes, following the methodology of Roth (2022). For each outcome, we present the number of pre-treatment periods in column 1, how many pre-period coefficients are individually significant in column 2, the maximum absolute t -value among pre-periods in column 3, the p -value from a joint significance test of all pre-period coefficients in column 4, and the t -statistic of the fitted linear slope through pre-period estimates in column 5. Columns (7) and (8) provide the slopes corresponding to the tests on linear violations of parallel trends that conventional pretests would detect 50% or 80% of the time ($\gamma = 0.5$ and $\gamma = 0.8$).

TABLE A2: ROBUSTNESS CHECKS: ALTERNATIVE MEASURES OF THE NTR GAP

	Mild depression (1)	Severe depression (2)	Mental health index (3)
Panel A: NTR gap measured by excluding industries with the highest NTR gap			
Post × NTR gap	-0.000 (0.014)	-0.032*** (0.009)	0.115*** (0.037)
<i>p</i> -value	[0.980]	[0.001]	[0.003]
<i>q</i> -value	(1.000)	(0.004)	(0.016)
Observations	4749	4749	4746
Pre-treatment mean	0.17	0.07	-0.06
Panel B: NTR gap measured by excluding industries with the lowest NTR gap			
Post × NTR gap	0.002 (0.018)	-0.037*** (0.013)	0.135** (0.052)
<i>p</i> -value	[0.904]	[0.004]	[0.010]
<i>q</i> -value	(1.000)	(0.005)	(0.016)
Observations	4749	4749	4746
Pre-treatment mean	0.17	0.07	-0.06
Panel C: NTR gap winsorized at the 5/95 percentiles			
Post × NTR gap	0.002 (0.018)	-0.038*** (0.012)	0.133*** (0.049)
<i>p</i> -value	[0.908]	[0.002]	[0.008]
<i>q</i> -value	(1.000)	(0.004)	(0.016)
Observations	4749	4749	4746
Pre-treatment mean	0.17	0.07	-0.06
Panel D: NTR gap measured by excluding nontradable industries			
Post × NTR gap	0.001 (0.014)	-0.024** (0.010)	0.093** (0.040)
<i>p</i> -value	[0.956]	[0.019]	[0.023]
<i>q</i> -value	(1.000)	(0.014)	(0.017)
Observations	4749	4749	4746
Pre-treatment mean	0.17	0.07	-0.06
Birth prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes
Birth year fixed effects			
× Other trade policies	Yes	Yes	Yes
× Initial prefecture characteristics	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes

Notes: Data are from the 2016–2018 CFPS. This table reports results of the DiD regressions of adolescent mental health on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. All regressions control for birth year fixed effects, prefecture of birth fixed effects, survey year fixed effects, and the birth year fixed effects interacted with other trade policies, including China's output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. The regressions also control for the birth year fixed effects interacted with initial prefecture characteristics, including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port and individual characteristics, including age, gender, indicator variables for whether the mother and father completed middle school, father's age and mother's age. The knitwear industry has the highest NTR gaps and is excluded in Panel A. The water resources management industry, coal mining and washing industry, mineral mining and processing industry, and coking industry have the lowest NTR gaps and are excluded in Panel B. Standard errors are clustered at the prefecture of birth level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A3: ROBUSTNESS CHECKS: ALTERNATIVE SPECIFICATIONS AND ALTERNATIVE SAMPLES

	Mild depression (1)	Severe depression (2)	Mental health index (3)
Panel A: Regression weighted by the 1990 prefecture population			
Post × NTR gap	-0.016 (0.017)	-0.039*** (0.010)	0.151*** (0.053)
<i>p</i> -value	[0.361]	[0.000]	[0.005]
<i>q</i> -value	⟨1.000⟩	⟨0.002⟩	⟨0.016⟩
Observations	4749	4749	4746
Pre-treatment mean	0.17	0.07	-0.06
Panel B: Controlling for province of birth-year linear trends			
Post × NTR gap	0.005 (0.016)	-0.031*** (0.010)	0.109*** (0.039)
<i>p</i> -value	[0.771]	[0.003]	[0.006]
<i>q</i> -value	⟨1.000⟩	⟨0.004⟩	⟨0.016⟩
Observations	4598	4598	4595
Pre-treatment mean	0.16	0.06	-0.04
Panel C: Using cohorts born between 1999 and 2004			
Post × NTR gap	-0.002 (0.016)	-0.034*** (0.010)	0.123*** (0.042)
<i>p</i> -value	[0.916]	[0.001]	[0.004]
<i>q</i> -value	⟨1.000⟩	⟨0.004⟩	⟨0.016⟩
Observations	3502	3502	3499
Pre-treatment mean	0.17	0.06	-0.04
Panel D: Using cohorts born between 1997 and 2006			
Post × NTR gap	-0.004 (0.014)	-0.023*** (0.008)	0.101*** (0.038)
<i>p</i> -value	[0.955]	[0.001]	[0.003]
<i>q</i> -value	⟨1.000⟩	⟨0.004⟩	⟨0.016⟩
Observations	5901	5901	5895
Pre-treatment mean	0.17	0.07	-0.07
Birth prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes

Notes: Data are from the 2016–2018 CFPS. This table reports results of the DiD regressions of adolescent mental health on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. All regressions control for birth year fixed effects, prefecture of birth fixed effects, survey year fixed effects, and the birth year fixed effects interacted with other trade policies, including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. The regressions also control for the birth year fixed effects interacted with initial prefecture characteristics, including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port and individual characteristics, including age, gender, indicator variables for whether the mother and father completed middle school, father’s age and mother’s age. Regressions in Panel A are weighted by the 1990 prefecture population. Regressions in Panel B further control for prefecture year of birth linear trends. Regressions in Panel C refer to children born between 1999 and 2004. Regressions in Panel D refer to children born between 1997 and 2006. Standard errors are clustered at the prefecture of birth level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A4: ROBUSTNESS CHECKS: ADDITIONAL CONTROLS AND PLACEBO TIMING

	Mild depression (1)	Severe depression (2)	Mental health index (3)
Panel A: Controlling for Robots Revolution			
Post × NTR gap	-0.002 (0.014)	-0.024** (0.010)	0.101** (0.040)
<i>p</i> -value	[0.864]	[0.018]	[0.014]
<i>q</i> -value	⟨1.000⟩	⟨0.014⟩	⟨0.016⟩
Observations	4375	4375	4372
Pre-treatment mean	0.16	0.07	-0.04
Panel B: Excluding Partially Exposed Cohorts			
Post × NTR gap	-0.004 (0.017)	-0.025** (0.011)	0.120*** (0.045)
<i>p</i> -value	[0.805]	[0.026]	[0.009]
<i>q</i> -value	⟨1.000⟩	⟨0.016⟩	⟨0.016⟩
Observations	3606	3606	3604
Pre-treatment mean	0.17	0.07	-0.07
Panel C: Using Unaffected Cohorts and Placebo Timing-1995			
Post × NTR gap	-0.018 (0.012)	-0.005 (0.004)	0.028 (0.028)
<i>p</i> -value	[0.137]	[0.281]	[0.315]
<i>q</i> -value	⟨1.000⟩	⟨0.076⟩	⟨0.095⟩
Observations	10359	10359	10359
Pre-treatment mean	0.21	0.02	0.07
Panel D: Using Unaffected Cohorts and Placebo Timing-1996			
Post × NTR gap	-0.011 (0.013)	0.003 (0.004)	-0.019 (0.029)
<i>p</i> -value	[0.402]	[0.492]	[0.509]
<i>q</i> -value	⟨1.000⟩	⟨0.129⟩	⟨0.133⟩
Observations	10359	10359	10359
Pre-treatment mean	0.22	0.03	0.05
Panel E: Using Unaffected Cohorts and Placebo Timing-1997			
Post × NTR gap	-0.003 (0.013)	0.004 (0.004)	-0.022 (0.028)
<i>p</i> -value	[0.840]	[0.255]	[0.448]
<i>q</i> -value	⟨1.000⟩	⟨0.075⟩	⟨0.127⟩
Observations	10359	10359	10359
Pre-treatment mean	0.22	0.03	0.05
Birth prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes
Birth year fixed effects			
× Other trade policies	Yes	Yes	Yes
× Initial prefecture characteristics	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes

Notes: Data in Panels A and B are from the 2016–2018 CFPS, while Panels C–E are from 2010–2014 CFPS. This table reports results of the DiD regressions of adolescent mental health on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. All regressions control for birth year fixed effects, prefecture of birth fixed effects, survey year fixed effects, and the birth year fixed effects interacted with other trade policies, including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. The regressions also control for the birth year fixed effects interacted with initial prefecture characteristics, including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port and individual characteristics, including age, gender, indicator variables for whether the mother and father completed middle school, father’s age and mother’s age. Standard errors are clustered at the prefecture of birth level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A5: HETEROGENEOUS EFFECTS OF PNTR ON ADOLESCENT MENTAL HEALTH OUTCOMES

	Mild depression (1)	Severe depression (2)	Mental health index (3)
Panel A: Interact with “Female”			
Post × NTR gap	0.002 (0.017)	-0.035*** (0.010)	0.133*** (0.039)
<i>p</i> -value	[0.881]	[0.004]	[0.021]
<i>q</i> -value	⟨1.000⟩	⟨0.006⟩	⟨0.033⟩
Post × NTR gap × interaction	-0.015 (0.016)	-0.001 (0.013)	0.009 (0.043)
<i>p</i> -value	[0.755]	[0.982]	[0.153]
<i>q</i> -value	⟨1.000⟩	⟨0.964⟩	⟨0.114⟩
Observations	4749	4749	4746
Pre-treatment mean	0.17	0.07	-0.06
Panel B: Interact with Rural Indicator -Above the median share of the rural-urban population in 2000			
Post × NTR gap	-0.003 (0.016)	-0.030** (0.014)	0.107* (0.058)
<i>p</i> -value	[0.900]	[0.000]	[0.001]
<i>q</i> -value	⟨1.000⟩	⟨0.002⟩	⟨0.004⟩
Post × NTR gap × interaction	0.008 (0.036)	0.011 (0.018)	-0.070 (0.086)
<i>p</i> -value	[0.348]	[0.953]	[0.840]
<i>q</i> -value	⟨1.000⟩	⟨0.964⟩	⟨0.266⟩
Observations	4749	4749	4746
Pre-treatment mean	0.17	0.07	-0.06
Birth prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Birth year fixed effects			
× Other trade policies	Yes	Yes	Yes
× Initial prefecture characteristics	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes

Notes: Data are from the 2016–2018 CFPS. This table reports the results of the DiD regressions of adolescent mental health on the interaction of the prefecture-level NTR gap and a post-PNTR indicator and a triple interaction of that term with a female indicator in Panel A with an indicator for whether the mother completed middle school in Panel B, an indicator of parental absence for at least one week from ages 0-3 in Panel C, and an indicator of whether the initial share of the rural population is above the median. All regressions control for birth year fixed effects, prefecture of birth fixed effects, and the birth year fixed effects interacted with other trade policies, including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. The regressions also control for the birth year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port; a triple interaction of those terms with a heterogeneous group indicator; and individual characteristics including age, gender, indicator variables for whether the mother and father completed middle school, father’s age and mother’s age. Standard errors are clustered at the prefecture of birth level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A6: IMPACT OF PNTR ON FEMALE AND MALE EMPLOYMENT STATUS-RURAL-URBAN HETEROGENEITY

	All sectors (1)	Agri (2)	Manu (3)	Service (4)	Unemployed (5)	NILF (6)
Panel A: Female						
Post × NTR gap	-0.006*	-0.043***	0.016***	0.021***	0.001	0.005
	(0.003)	(0.006)	(0.004)	(0.003)	(0.001)	(0.003)
<i>p</i> -value	[0.083]	[0.000]	[0.000]	[0.000]	[0.390]	[0.109]
<i>q</i> -value	⟨0.165⟩	⟨0.001⟩	⟨0.001⟩	⟨0.001⟩	⟨0.295⟩	⟨0.192⟩
Post × NTR gap × Rural Indicator	-0.014	-0.012	-0.003	0.001	-0.002	0.016*
	(0.009)	(0.011)	(0.006)	(0.006)	(0.002)	(0.009)
<i>p</i> -value	[0.123]	[0.278]	[0.608]	[0.923]	[0.390]	[0.069]
<i>q</i> -value	⟨0.192⟩	⟨0.295⟩	⟨0.497⟩	⟨0.733⟩	⟨0.295⟩	⟨0.165⟩
Observations	1655	1655	1655	1655	1655	1655
Pre-treatment mean	0.82	0.59	0.09	0.14	0.02	0.16
Panel B: Male						
Post × NTR gap	0.004*	-0.038***	0.015***	0.026***	0.000	-0.004**
	(0.002)	(0.005)	(0.004)	(0.005)	(0.001)	(0.002)
<i>p</i> -value	[0.082]	[0.000]	[0.000]	[0.000]	[0.694]	[0.024]
<i>q</i> -value	⟨0.165⟩	⟨0.001⟩	⟨0.001⟩	⟨0.001⟩	⟨0.497⟩	⟨0.066⟩
Post × NTR gap × Rural Indicator	-0.002	-0.008	-0.007	0.013	-0.001	0.003
	(0.004)	(0.009)	(0.006)	(0.008)	(0.002)	(0.003)
<i>p</i> -value	[0.665]	[0.353]	[0.260]	[0.116]	[0.654]	[0.340]
<i>q</i> -value	⟨0.497⟩	⟨0.295⟩	⟨0.295⟩	⟨0.192⟩	⟨0.497⟩	⟨0.295⟩
Observations	1655	1655	1655	1655	1655	1655
Pre-treatment mean	0.94	0.61	0.11	0.22	0.02	0.04
Prefecture fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects						
× Other trade policies	Yes	Yes	Yes	Yes	Yes	Yes
× Initial prefecture characteristics	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data are from the 1990, 2000, 2005, 2010, and 2015 population censuses in China. This table reports results of the DiD regressions of margins of local labor market adjustment on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. All regressions control for prefecture fixed effects, year fixed effects, and the year fixed effects interacted with other trade policies including China's output and input tariffs, NTR rates, export licensing, MFA quotas, and contract intensity. They also control for the survey year fixed effects interacted with initial prefecture characteristics including GDP per capita, the share of employment in manufacturing, and the distance to the nearest port. Regressions in Panel A consider marriageable and working-age women (ages 20-55). Regressions in Panel B consider marriageable and working-age men (ages 22-60). Regressions are weighted by the 1990 prefecture population. Standard errors are clustered at the prefecture level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A7: IMPACT OF PNTR ON LABOR INCOME PER WORKER-RURAL-URBAN HETEROGENEITY

	Labor Income per Worker	
	(1)	(2)
Post \times NTR gap	0.078*** (0.017)	0.043*** (0.016)
<i>p</i> -value	[0.000]	[0.007]
<i>q</i> -value	\langle 0.001 \rangle	\langle 0.015 \rangle
Post \times NTR gap \times Rural Indicator	-0.092 (0.059)	-0.075 (0.057)
<i>p</i> -value	[0.120]	[0.192]
<i>q</i> -value	\langle 0.064 \rangle	\langle 0.107 \rangle
Observations	5628	5628
Pre-treatment mean	7.90	7.90
Prefecture fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Year fixed effects		
\times Other trade policies	Yes	Yes
\times Initial prefecture characteristics		Yes

Notes: Labor income per worker comes from the 1995–2015 China City Statistical Yearbooks. This table reports results of the DiD regressions of labor income per worker on interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions in columns 1 control for prefecture fixed effects, year fixed effects, and the year fixed effects interacted with other trade policies including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. Regressions in columns 2 further control for year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. Standard errors are clustered at the prefecture level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A8: IMPACT OF PNTR ON LOCAL FOOD ENVIRONMENT-RURAL-URBAN HETEROGENEITY

	Total Imports (in log)		Imports per Capita (in log)	
	(1)	(2)	(3)	(4)
Panel A: Local Food Imports				
Post × NTR gap	0.185	0.162	0.123**	0.106*
	(0.149)	(0.158)	(0.063)	(0.063)
<i>p</i> -value	[0.216]	[0.306]	[0.050]	[0.094]
<i>q</i> -value	⟨0.576⟩	⟨0.620⟩	⟨0.576⟩	⟨0.570⟩
Post × NTR gap × Rural Indicator	-0.282	-0.270	0.164	0.157
	(0.492)	(0.505)	(0.196)	(0.201)
<i>p</i> -value	[0.567]	[0.594]	[0.403]	[0.435]
<i>q</i> -value	⟨0.576⟩	⟨0.664⟩	⟨0.576⟩	⟨0.664⟩
Observations	5632	5632	5632	5632
Pre-treatment mean	9.85	9.85	2.74	2.74
Panel B: Local Good Imports				
Post × NTR gap	0.046	0.028	0.046	0.030
	(0.053)	(0.059)	(0.053)	(0.059)
<i>p</i> -value	[0.387]	[0.638]	[0.384]	[0.608]
<i>q</i> -value	⟨0.576⟩	⟨0.664⟩	⟨0.576⟩	⟨0.664⟩
Post × NTR gap × Rural Indicator	0.339	0.336	0.317	0.314
	(0.219)	(0.217)	(0.212)	(0.210)
<i>p</i> -value	[0.123]	[0.122]	[0.137]	[0.136]
<i>q</i> -value	⟨0.576⟩	⟨0.570⟩	⟨0.576⟩	⟨0.570⟩
Observations	5632	5632	5632	5632
Pre-treatment mean	17.15	17.15	6.96	6.96
Prefecture fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Year fixed effects				
× Other trade policies	Yes	Yes	Yes	Yes
× Initial prefecture characteristics		Yes		Yes

Notes: Data are from the 1997–2014 annual prefecture-level customs data in China. This table reports results of the DiD regressions of local food and all goods imports on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions in columns 1 and 3 control for prefecture fixed effects, year fixed effects, and the year fixed effects interacted with other trade policies including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. Regressions in columns 2 and 4 further control for year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. Standard errors are clustered at the prefecture of birth level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A9: IMPACT OF PNTR ON LOCAL FOOD ENVIRONMENT

	Total Imports (in log)		Imports per Capita (in log)	
	(1)	(2)	(3)	(4)
Panel A: Local Food Imports				
Post × NTR gap	0.169 (0.129)	0.114 (0.138)	0.131** (0.053)	0.113** (0.057)
<i>p</i> -value	[0.171]	[0.407]	[0.011]	[0.047]
<i>q</i> -value	⟨0.148⟩	⟨0.440⟩	⟨0.047⟩	⟨0.234⟩
Observations	5632	5632	5632	5632
Outcome mean	10.90	10.90	3.49	3.49
Panel B: Local Good Imports				
Post × NTR gap	0.076 (0.047)	0.061 (0.056)	0.073 (0.047)	0.062 (0.056)
<i>p</i> -value	[0.110]	[0.284]	[0.122]	[0.270]
<i>q</i> -value	⟨0.140⟩	⟨0.398⟩	⟨0.140⟩	⟨0.398⟩
Observations	5632	5632	5632	5632
Outcome mean	18.28	18.28	8.07	8.07
Prefecture fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Year fixed effects				
× Other trade policies	Yes	Yes	Yes	Yes
× Initial prefecture characteristics		Yes		Yes

Notes: Data are from the 1997–2014 annual prefecture-level customs data in China. This table reports results of the DiD regressions of local food and all goods imports on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions in columns 1 and 3 control for prefecture fixed effects, year fixed effects, and the year fixed effects interacted with other trade policies including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. Regressions in columns 2 and 4 further control for year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. Standard errors are clustered at the prefecture of birth level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A10: IMPACT OF PNTR ON PARENTING STYLES

	(1)	(2)	(3)
Panel A: Parenting style- Optimal			
Post \times NTR gap	0.035** (0.016)	0.045* (0.024)	0.048** (0.023)
<i>p</i> -value	[0.033]	[0.059]	[0.037]
<i>q</i> -value	\langle 0.153 \rangle	\langle 0.308 \rangle	\langle 0.177 \rangle
Observations	3130	3130	3130
Pre-treatment mean	0.16	0.16	0.16
Panel B: Parenting style- Affectionate			
Post \times NTR gap	-0.019 (0.026)	-0.031 (0.027)	-0.035 (0.028)
<i>p</i> -value	[0.456]	[0.252]	[0.216]
<i>q</i> -value	\langle 0.413 \rangle	\langle 0.403 \rangle	\langle 0.403 \rangle
Observations	3130	3130	3130
Pre-treatment mean	0.34	0.34	0.34
Panel C: Parenting style- Affectionless			
Post \times NTR gap	0.012 (0.019)	0.010 (0.024)	0.011 (0.025)
<i>p</i> -value	[0.527]	[0.688]	[0.662]
<i>q</i> -value	\langle 0.413 \rangle	\langle 0.621 \rangle	\langle 0.620 \rangle
Observations	3130	3130	3130
Pre-treatment mean	0.17	0.17	0.17
Panel D: Parenting style- Neglectful			
Post \times NTR gap	-0.028 (0.019)	-0.024 (0.022)	-0.025 (0.023)
<i>p</i> -value	[0.146]	[0.287]	[0.287]
<i>q</i> -value	\langle 0.281 \rangle	\langle 0.403 \rangle	\langle 0.403 \rangle
Observations	3130	3130	3130
Pre-treatment mean	0.32	0.32	0.32
Birth prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Birth year fixed effects			
\times Other trade policies	Yes	Yes	Yes
\times Initial prefecture characteristics		Yes	Yes
Individual characteristics			Yes

Notes: Data are from the 2016–2018 CFPS. This table reports results of the DiD regressions of parenting styles on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions in column 1 control for survey year fixed effects, birth year fixed effects, prefecture of birth fixed effects, and the birth year fixed effects interacted with other trade policies including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. Regressions in column 2 further control for the birth year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. Regressions in column 3 further control for individual characteristics including age, gender, indicator variables for whether the mother and father completed middle school, father’s age and mother’s age. Standard errors are clustered at the prefecture of birth level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A11: IMPACT OF PNTR ON ADOLESCENT PHYSICAL HEALTH, COGNITION AND SCHOOL DROPOUT RATES

	(1)	(2)	(3)
Panel A: Physical health			
Post \times NTR gap	0.002 (0.033)	0.020 (0.042)	0.020 (0.041)
<i>p</i> -value	[0.946]	[0.640]	[0.631]
<i>q</i> -value	$\langle 1.000 \rangle$	$\langle 1.000 \rangle$	$\langle 1.000 \rangle$
Observations	4724	4724	4724
Pre-treatment mean	-0.01	-0.01	-0.01
Panel B: Cognitive function index			
Post \times NTR gap	0.004 (0.005)	0.003 (0.006)	0.002 (0.006)
<i>p</i> -value	[0.415]	[0.610]	[0.712]
<i>q</i> -value	$\langle 0.709 \rangle$	$\langle 1.000 \rangle$	$\langle 1.000 \rangle$
Observations	3929	3929	3929
Pre-treatment mean	0.03	0.03	0.03
Panel C: School dropout rate			
Post \times NTR gap	-0.021 (0.013)	-0.018 (0.017)	-0.019 (0.017)
<i>p</i> -value	[0.107]	[0.309]	[0.257]
<i>q</i> -value	$\langle 0.476 \rangle$	$\langle 1.000 \rangle$	$\langle 1.000 \rangle$
Observations	4749	4749	4749
Pre-treatment mean	0.22	0.22	0.22
Birth prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Birth year fixed effects \times Other trade policies	Yes	Yes	Yes
\times Initial prefecture characteristics		Yes	Yes
Individual characteristics			Yes

Notes: Data are from the 2016–2018 CFPS. This table reports results of the DiD regressions of child physical health and cognitive function on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions in column 1 control for survey year fixed effects, birth year fixed effects, prefecture of birth fixed effects, and the birth year fixed effects interacted with other trade policies including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. Regressions in column 2 further control for the birth year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. Regressions in column 3 further control for individual characteristics including age, gender, indicator variables for whether the mother and father completed middle school, father’s age and mother’s age. Standard errors are clustered at the prefecture of birth level. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A12: IMPACT OF PNTR ON MOTHER'S MENTAL HEALTH

	(1)	(2)	(3)
Panel A: Doctor's diagnosis of Mental Conditions			
Post × NTR gap	0.000 (0.002)	0.001 (0.003)	0.001 (0.003)
<i>p</i> -value	[0.923]	[0.866]	[0.814]
<i>q</i> -value	⟨1.000⟩	⟨1.000⟩	⟨1.000⟩
Observations	1931	1931	1931
Pre-treatment mean	0.00	0.00	0.00
Panel B: Mild Depression			
Post × NTR gap	0.016 (0.020)	0.003 (0.021)	0.002 (0.021)
<i>p</i> -value	[0.426]	[0.889]	[0.907]
<i>q</i> -value	⟨1.000⟩	⟨1.000⟩	⟨1.000⟩
Observations	3859	3859	3859
Pre-treatment mean	0.38	0.38	0.38
Panel C: Severe Depression			
Post × NTR gap	0.016 (0.015)	0.016 (0.016)	0.016 (0.016)
<i>p</i> -value	[0.277]	[0.314]	[0.322]
<i>q</i> -value	⟨1.000⟩	⟨1.000⟩	⟨1.000⟩
Observations	3859	3859	3859
Pre-treatment mean	0.16	0.16	0.16
Panel D: Mental Health Index			
Post × NTR gap	-0.060 (0.042)	-0.053 (0.049)	-0.052 (0.048)
Observations	3861	3861	3861
Pre-treatment mean	-0.02	-0.02	-0.02
Survey year fixed effects	Yes	Yes	Yes
Prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
× Other trade policies	Yes	Yes	Yes
× Initial prefecture characteristics		Yes	Yes
Individual characteristics			Yes

Notes: Data in Panel A are from the 2016–2018 CFPS and data in Panels B–D are from the 2000–2015 CHNS. Regressions in column 1 control for survey year fixed effects, birth year fixed effects, prefecture of birth fixed effects, and the birth year fixed effects interacted with other trade policies including China's output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. Regressions in column 2 further control for the birth year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. Regressions in column 3 further control for individual characteristics including age, gender, indicator variables for whether the mother and father completed middle school, father's age and mother's age. Standard errors are clustered at the prefecture of birth level in Panel A and are clustered at the prefecture level in Panel B. Survey fixed effects only apply to Panel A. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A13: IMPACT OF PNTR ON FATHER'S MENTAL HEALTH

	(1)	(2)	(3)
Panel A: Doctor's diagnosis of Mental Conditions			
Post × NTR gap	0.006** (0.003)	0.006*** (0.002)	0.006** (0.002)
<i>p</i> -value	[0.023]	[0.006]	[0.011]
<i>q</i> -value	⟨0.037⟩	⟨0.020⟩	⟨0.035⟩
Observations	1932	1932	1932
Pre-treatment mean	0.00	0.00	0.00
Panel B: Mild Depression			
Post × NTR gap	0.043** (0.020)	0.039** (0.019)	0.039** (0.020)
<i>p</i> -value	[0.037]	[0.044]	[0.046]
<i>q</i> -value	⟨0.037⟩	⟨0.046⟩	⟨0.048⟩
Observations	3746	3746	3746
Pre-treatment mean	0.29	0.29	0.29
Panel C: Severe Depression			
Post × NTR gap	0.029** (0.013)	0.022 (0.015)	0.021 (0.015)
<i>p</i> -value	[0.023]	[0.154]	[0.154]
<i>q</i> -value	⟨0.037⟩	⟨0.071⟩	⟨0.074⟩
Observations	3746	3746	3746
Pre-treatment mean	0.11	0.11	0.11
Panel D: Mental Health Index			
Post × NTR gap	-0.103** (0.046)	-0.108* (0.055)	-0.108* (0.055)
Observations	3746	3746	3746
Pre-treatment mean	-0.02	-0.02	-0.02
Survey year fixed effects	Yes	Yes	Yes
Prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Birth year fixed effects			
× Other trade policies	Yes	Yes	Yes
× Initial prefecture characteristics		Yes	Yes
Individual characteristics			Yes

Notes: Data in Panel A are from the 2016–2018 CFPS and data in Panels B-D are from the 2000-2015 CHNS. Regressions in column 1 control for survey year fixed effects, birth year fixed effects, prefecture of birth fixed effects, and the birth year fixed effects interacted with other trade policies including China's output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. Regressions in column 2 further control for the birth year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. Regressions in column 3 further control for individual characteristics including age, gender, indicator variables for whether the mother and father completed middle school, father's age and mother's age. Standard errors are clustered at the prefecture of birth level in Panel A and are clustered at the prefecture level in Panel B. Survey fixed effects only apply to Panel A. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A14: IMPACT OF PNTR ON PARENTAL ABSENCE

	(1)	(2)	(3)
Panel A: Parents were absent for at least one week from ages 0-3			
Post × NTR gap	-0.007 (0.013)	-0.004 (0.017)	-0.004 (0.017)
<i>p</i> -value	[0.594]	[0.815]	[0.797]
<i>q</i> -value	<1.000)	<0.820)	<1.000)
Observations	4512	4512	4512
Pre-treatment mean	0.15	0.15	0.15
Birth prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes
Birth year fixed effects			
× Other trade policies	Yes	Yes	Yes
× Initial prefecture characteristics		Yes	Yes
Individual characteristics			Yes
Panel B: A parent was not living in the household and seeking employment elsewhere			
Panel B1: Mother			
Post × NTR gap	-0.001** (0.000)	-0.001 (0.000)	-0.000 (0.001)
<i>p</i> -value	[0.011]	[0.300]	[0.558]
<i>q</i> -value	<0.036)	<0.820)	<1.000)
Observations	1348	1348	1348
Pre-treatment mean	0.00	0.00	0.00
Panel B2: Father			
Post × NTR gap	0.001 (0.010)	-0.017 (0.016)	-0.011 (0.015)
<i>p</i> -value	[0.900]	[0.297]	[0.488]
<i>q</i> -value	<1.000)	<0.820)	<1.000)
Observations	1347	1347	1347
Pre-treatment mean	0.03	0.03	0.03
Prefecture fixed effects	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes
Birth year fixed effects			
× Other trade policies	Yes	Yes	Yes
× Initial prefecture characteristics		Yes	Yes
Individual characteristics			Yes

Notes: Data in Panel A are from the 2016–2018 CFPS and data in Panel B are from the 2000–2015 CHNS. Regressions in column 1 control for survey year fixed effects, birth year fixed effects, prefecture of birth fixed effects, and the birth year fixed effects interacted with other trade policies including China’s output and input tariffs, NTR rates, export licensing, MFA quotas, FDI liberalization policy and contract intensity. Regressions in column 2 further control for the birth year fixed effects interacted with initial prefecture characteristics including GDP per capita (in log), the share of employment in manufacturing, the share of population with middle school education, average population age and the distance to the nearest port. Regressions in column 3 further control for individual characteristics including age, gender, indicator variables for whether the mother and father completed middle school, father’s age and mother’s age. Standard errors are clustered at the prefecture of birth level in Panel A and are clustered at the prefecture level in Panel B. We report the two-sided single-hypothesis *p*-values in square brackets and the corresponding multiple-hypothesis adjusted *q*-values of [Benjamini et al. \(2006b\)](#) in angle brackets. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A15: DIFFERENCE BETWEEN MIGRANTS AND NON-MIGRANTS

	(1) Non-migrants	(2) Migrants	(3) Difference in means
Male	0.507 (0.500)	0.622 (0.485)	0.119*** (0.003)
Han ethnicity	0.899 (0.302)	0.931 (0.253)	-0.015*** (0.001)
Junior graduate dummy	0.872 (0.334)	0.858 (0.350)	-0.062*** (0.002)
Years of schooling	10.359 (3.164)	9.761 (2.720)	-1.766*** (0.017)
Observations	717,473	380,46	755,519

Notes: Data are from the 2015 population census. Columns (1) and (2) report the means for non-migrants and migrants. Column (3) reports the mean difference between the non-migrants and migrants, adjusted for prefecture fixed effects. Robust standard errors are in parentheses in column (3). ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE A16: IMPACT OF PNTR ON FERTILITY OUTCOMES

	Births per 1,000 women (1)	Number of children (2)
Post × NTR gap	-0.525 (0.429)	-300.141 (483.699)
Observations	1655	1655
Outcome mean	24.37	6346.42
Prefecture fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Year fixed effects		
× Other trade policies	Yes	Yes
× Initial prefecture characteristics	Yes	Yes

Notes: Data are from the 1990, 2000, 2005, 2010, and 2015 population censuses in China. This table reports results of the DiD regressions of fertility on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. All regressions control for prefecture fixed effects, year fixed effects, and the year fixed effects interacted with other trade policies including China's output and input tariffs, NTR rates, export licensing, MFA quotas, and contract intensity. The regressions also control for the survey year fixed effects interacted with initial prefecture characteristics including GDP per capita, the share of employment in manufacturing, and the distance to the nearest port. All regressions are weighted by 1990 prefecture population. Standard errors are clustered at the prefecture level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.