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ABSTRACT

Early Childhood Conditions and Adolescent Mental Health*

We investigate how early life circumstances induced by trade liberalization affect adolescent mental health in China, exploiting variation in tariff uncertainty faced by prefecture economies pre-2001. Our model differs from the classic difference-indifferences design in that it considers a moderator variable determining the intensity with which the treatment affects the outcomes. Our findings show that children born in prefectures more exposed to an exogenous change in international trade policy experienced a significant decline in the incidence of severe depression during adolescence. We find that the estimated relationships are robust to controls for initial prefecture attributes and other policy changes. Improvements in parental income, early childhood investments, and care provision in formal early childhood education programs are likely operative channels of impact.

JEL Classification: F16, I15, J13, C21

Keywords: mental health, trade reform, China, early life investments

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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: 17 to 22 percent of adolescents experience at least one major depressive episode, and 4 to 6 percent experience symptoms of severe depression (National Institute of Mental Health 2022; Centers for Disease Control and Prevention 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 Chinese 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 disorders. A significant barrier to addressing this question is to find plausibly exogenous sources of variation in early economic conditions. This paper uses a change in international trade policy to investigate how economic shocks in early childhood affect adolescent mental health, and, in particular, the incidence of depression among teenagers.

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

[1]According 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).

[2]The 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).

[3]Consequently, 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. (2018) provide an overview of the recent literature on early-life conditions and adult health. Heckman (2012) 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.

The economic impact of the PNTR bill varied across Chinese regions depending on the industry composition of the local economy (Erten and Leight 2021). Regions whose industries were more exposed to trade effects experienced increased employment in manufacturing and service sectors and decreased agricultural activities.[5] Exposed regions also benefited from a rise in local GDP, increased exports, and higher foreign direct investments. However, the aftermath of an economic shock of this nature on mental health is understudied. Specifically, it is unclear whether economic shocks in early childhood affect the incidence of mental disorders, such as depression, during adolescence.[6]

Economic shocks resulting from trade liberalization have a substantial impact on labor markets. Changes in economic conditions, particularly during the early stages of the child's life, can significantly influence adolescent mental health outcomes through several channels. 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 include better prenatal care, more breastfeeding time, more vaccinations, and improvements in nutritional intake. An increase in parental resources may also allow parents to send their children to formal early childhood education programs.

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. While we focus on early-life exposure to PNTR, we trace the impact of exposure to PNTR across the full age spectrum from birth to late adolescence. 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 2006a, 2007a, 2012; Almond and Currie 2011; Almond et al. 2018).[7] Our primary data source is the

[5] Erten and Leight (2021) use panel data for Chinese counties from 1996 to 2013 to exploit the regional variation in tariff uncertainty faced by local labor markets before 2001.

[6] Studies 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). While the estimates of MDD incidence in childhood range from 1–3 percent, the estimates of its incidence in adolescence increase to 4–6 percent, which are close to the levels observed in adulthood. 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 evidence suggests that the benefits of early treatment of depression during adolescence are critical for the psychological well-being of individuals in adulthood.

[7] Evidence from the 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 experiences in any other life period (Knudsen

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.[8]

We implement a difference-in-differences (DiD) identification strategy to examine whether the mental health outcomes of cohorts born in prefectures more exposed to PNTR differ from cohorts born in less exposed prefectures.[9] Our model differs from the canonical DiD design, which compares the outcome difference between treatment and control groups before and after the intervention. Our model accounts for the trade exposure of each prefecture, which plays the role of a moderator variable that dictates the intensity with which the PNTR intervention impacts the outcomes. We term this model a moderated difference-in-differences (MDiD) model.

MDiD models are popular among empirical works capable of assessing information on how a treatment affects outcomes. Recent examples of the use of such designs in the literature on early childhood interventions include the following: Adhvaryu, Fenske, and Nyshadham (2019), who studies the impact of cocoa price variation during early life on mental well-being in adulthood; Anders, Barry, and Smithz (2021), who examines the impact of early childhood education on criminal activity; and Barr and Smith (2021), who evaluates the effect of nutritional assistance in early childhood on violent behavior in adulthood. Another recent work that employs moderator variables is Khanna, Murathanoglu, Theoharides, and Yang (2022), who leverage the impact of the 1997 financial crisis to study the labor market outcomes of migrants in the Philippines. The economic impact of the crisis is moderated by the average exchange rate shock of each province.

Despite its popularity, the causal content of MDiD models has been largely overlooked. To address this gap, our paper offers some methodological contributions to the DiD literature. We analyze the causal parameters of the MDiD model, establish the necessary

et al. 2006).

[8]The most common prefecture form is the so-called prefecture-level city (*dijishi*). There are also prefectures that are not prefecture-level cities, 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 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.

[9]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.

assumptions to identify causal effects, and outline potential pitfalls of the MDiD estimator. Our analysis is consistent with recent research exploring the causal implications of DiD models that differ from the canonical design.[10] In particular, the MDiD model is distinct but closely related to the DiD model with a continuous treatment studied by Callaway, Goodman-Bacon, and Sant'Anna (2021). We benefit from their work extensively.

We find that cohorts born in prefectures more exposed to the trade liberalization policy of PNTR experienced a significant decline in the incidence of severe depression during adolescence. We show that these declines are most prominent for those born after the reform, with estimates implying that a one standard deviation increase in exposure to PNTR is associated with a 4.7 percentage-points decline in the probability of experiencing severe depression, which corresponds to a reduction of 52 percent of the pre-PNTR outcome mean.

We examine three potential channels that could explain our findings: parental income, early-life investments, and migration and fertility responses. Using the China Health and Nutrition Survey (CHNS), we document that households in prefectures more exposed to PNTR experience an increase in their household income compared to households in less exposed prefectures after the reform. We then explore the consequences of these parental income changes for early life investments in children. Our results show that children living in prefectures more exposed to the PNTR policy benefited from improved early-life investments. These children are more likely to have been breastfed, had a higher caloric intake, and benefited from increased prenatal visits and early childhood vaccinations. We also find that these children are more likely to have attended early childhood education programs instead of being taken care of at home by family members. Finally, our findings are not explained by factors such as selective migration, parental absence, or fertility declines 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) use 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

[10]Recent works in this growing literature include the following: de Chaisemartin and D'Haultfoeuille (2018); Borusyak et al. (2022); Goodman-Bacon (2021); de Chaisemartin and D'Haultfoeuille (2020); Sun and Abraham (2021).

been significantly understudied. Our work contributes to filling this gap in the mental health literature by examining the impacts of early childhood conditions on adolescent mental health outcomes.

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. 2018; Currie 2020).[11] 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. 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. Our work is in line with that of Adhvaryu et al. (2019), who reveals that favorable circumstances in early life, driven by positive commodity price shocks, result in a substantial decrease in severe mental distress in adulthood.

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

The rest of the paper is organized as follows. Section 2 describes the data used, and Section 3 investigates the requirements for causal inference through DiD designs with moderation. Section 4 outlines our empirical strategy, and Section 5 presents our results for key outcomes and discusses mechanisms. Section 6 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.

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

2.1 Mental Health

Our primary measure of mental health is constructed from 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.[12]

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); (vii) I felt sad; and (viii) 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 the incidence of depression: (i) any depression, which takes a value of one if the CES-D score is 7 or above, and (ii) severe depression, which takes a value of one if the CES-D score is greater than or equal to 10.

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 1982 and 2006, who were at or below the age of 20 at the time of the PNTR introduction. 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).[13] We describe the definition of the treatment and control groups in Section 4. This leaves us with a sample of 14,521

[12]The 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, we use the 8-item CES-D scale evaluated by respondents aged 10 and above in the 2016 and 2018 waves of the CFPS.

[13]This 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).

individuals for mental health outcomes.

In addition, the CFPS includes information on physical health outcomes. We create a physical health index by taking a simple average of z scores for the following two measures: (i) an indicator variable that takes a value of one if the respondent felt physically uncomfortable over the past two weeks and (ii) an indicator variable that takes a value of one if the respondent was hospitalized in the previous year due to illness or injury. Higher values of the index reflect better physical health.

Finally, the CFPS presents respondents with two sets of cognitive tests to evaluate their cognitive ability. While one of these tests focuses on assessing respondents' verbal ability, the other evaluates their math ability. Using this information, we construct a cognitive function index by taking a simple average of z scores for the verbal and math test scores. Higher values of the index again reflect higher cognitive function.

Panel A of Table 1A provides summary statistics for the CFPS data in our sample. We observe that 26 percent of adolescents experience any depression and 8 percent experience severe depression. The average age of adolescents in our sample is 23, and approximately 55 percent of the participants are male.

2.2 CHNS 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 eight waves in our analysis: the 1993, 1997, 2000, 2004, 2006, 2009, 2011, and 2015 waves.

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 prenatal checkups, childbearing and childrearing for women who have ever married and were pregnant during the survey period. From this dataset, we create three measures of early life investments in children: whether the mother did any prenatal checkups for the most recent pregnancy, whether the mother breastfed the child, and whether the child received a specific vaccine. Moreover, during the child survey, the child's caregivers were asked to report the nutrition intakes (total calories, protein, carbohydrate, and fat) in the past 3 days, childcare arrangements and early childhood education for children aged 0 to 6 years. In particular, our childcare measurements include whether the child is cared for by people outside the household over the past week; the number of hours per day and the number of days per week that a child is cared for by people outside the household; whether the child is attending nursery school; whether the child is attending kindergarten; and whether the child is cared for by a nanny, relatives, a neighbor, grandparents or at another facility. We retain children with nonmissing responses for the prefecture of residence and who were born between 1982 and 2006. This leaves us with a sample of 1,429 children with vaccination records from PHF and 3,446 children with nutrition intake from the child survey.[14] The summary statistics are tabulated in Panel B of Table 1A.

In addition, we use household income per capita from the CHNS from 1993 to 2015 to corroborate reform-induced positive income effects documented using GDP per capita by Erten and Leight (2021). Panel C of Table 1B presents the summary statistics for thehousehold income measure.

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 an individual's region of household registration, region of residence, demographic characteristics, and fertility records for women under the age of 45. We aggregate from the individual-level data to the prefecture level and calculate the share of immigrants and emigrants (and its composition by gender), number of births per 1,000 women, total number of children and share of women who had children for women aged 20 to 45 years old in each prefecture.[15] As presented in Panel E of Table 1B, the average immigration rate is 6 percent, the average emigration rate is 8 percent and the average number of children is 4,011 in each prefecture.

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 PNTR 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.

[14]Since 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.

[15]The legal marriage age in China is 20 for female.

We utilize variation across Chinese prefectures in the concentrations of different industries in 1990 and variation across industries in the gap between the lower tariffs applied to most-favored-nation tariffs and the higher non-market economy 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.[16] 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.[17]

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 from 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 of uncertainty over tariffs.

$$NTR\,Gap_p = \sum_j S_{jp}^{1990} \times NTR\,Gap_j \tag{1}$$

where $NTR\,Gap_p$ denotes the NTR gap for prefecture p, S_{ip}^{1990} denotes the share of employment by industry j in prefecture p in 1990, and $NTR\,Gap_j$ 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, $NTR\,Gap_j = Non\,NTR\,Rate_j - NTR\,Rate_j$.[18]

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

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

[17] Another example of a government policy 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. In Section 5.2, we further show that cross-prefecture migrations are not affected by the PNTR policy change.

[18] We 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 i 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.

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.

In our model, the variable $NTR\,Gap_p$ in (1) plays the role of a moderator variable that affects the impact of the reduction in tariff uncertainty induced by PNTR on the local economy of the Chinese prefectures, depending on the initial industrial composition. Since the PNTR rates became effective for China as of January 1, 2002, our analysis characterizes all years from 2002 onward as the postreform period. We use a standardized NTR gap measure in our estimations for each interpretation consistent with our econometric model presented in Section 3. Figure 2 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. Overall, there is a substantial variation in exposure to the reduction in tariff uncertainty across Chinese prefectures.

2.4 Other Control Variables

The CFPS and CHNS contain rich data on demographic and socioeconomic characteristics, such as gender, date of birth, marital status, and educational attainment. Panels A and B of Table 1A also provide summary statistics for children's demographic characteristics that we control for in our analysis. For the CFPS sample of children, 55 percent are boys, and the fathers of 53 percent and the mothers of 36 percent of the children completed middle school education. We observe that 55 percent of the CHNS sample children are boys; 67 and 55 percent have fathers and mothers, respectively, who have completed middle school education.

Initial differences in prefectures' characteristics might have influenced children's outcomes and thus might contaminate our estimates. To alleviate this concern, we include interactions of the post-PNTR indicator with the following prefecture-level characteristics: GDP per capita, average years of education, average population age, fertility rate, and child population. 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, 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. Finally, we control for trade policy changes in the US, including the NTR rate itself. The data source and definition of each of these variables are described in detail in Appendix A.2.

3 On Causal Inference using the MDiD Model

We estimate a moderated difference-in-differences (MDiD) model in which trade exposure, $NTR\,Gap_p$ in Eq. (1), plays the role of a moderator variable M that dictates the impact of the PNTR intervention D on mental health outcomes Y.

The MDiD design departs from the conventional two-treatment DiD model by introducing a moderator variable M. This model is advantageous in economic contexts where an observable variable determines the extent of the intervention's effect on outcomes.[19] MDiD designs are widely popular in the empirical DiD literature[20] and are commonly evaluated by the following two-way fixed effect (TWFE) regression:

$$Y_{it} = \theta_t + \eta_i + \beta_{DiD} \cdot W_i \cdot Post_t + v_{it}, \tag{2}$$

where $W_i = M_i \cdot D_i$ is the treatment intensity, θ_t denotes time fixed effects, η_i stands for unit fixed effects, Y_{it} denotes the outcome for unit i in period t, v_i is the unobserved error term, and β_{DiD} is the DiD parameter of interest. The term $W_i \cdot Post_t$ is the interaction between the treatment intensity W_i and the posttreatment period indicator $Post_t$.

The MDiD model shares some similarities with the DiD model with a continuous treatment studied by Callaway et al. (2021). Both models allow for some variation in the treatment intensity, however, the source of the variation differs. In the continuous case, the treatment D takes a value of zero for the untreated units and takes a continuous value d for treated units. In the case of MDiD, the treatment is binary, and the variation in treatment intensity stems from the heterogeneity in the moderator variable M across the units.

[19] There are plenty of economic examples in which a moderator variable arises. For instance, the effect of a rent-control policy on real estate prices depends on the share of dwellings being rented. The share of households with children moderates the effect of improving public schools on housing prices. Furthermore, the effects on crime of a firearm confiscation policy depend on the share of the population that possesses guns.

[20] Recent examples of studies that use this design include Adhvaryu et al. (2019); Anders et al. (2021); Barr and Smith (2021); Khanna et al. (2022).

Despite its usage, the requirements for causal inference by means of the MDiD design have yet to be fully explored. Therefore, we investigate the assumptions that ensure a causal interpretation of the estimates produced by the two-period MDiD model. Some notation is in order.

Our DiD setup consists of three observed variables indexed by units $i \in \mathcal{I}$ for two time periods t and $t-1$. As mentioned, we use $Y_{it} \in \mathbb{R}$ for the outcome, $D_i \in \{0,1\}$ for the treatment indicator, and $M_i \in \mathcal{M}$ for the moderator variable. No unit i is treated in period $t-1$ (preintervention). Units i such that $D_i = 1$ are treated in period t (postintervention). We use $\Delta Y_{it} \equiv Y_{it} - Y_{it-1}$ for the outcome time-difference for unit i. The observed data $(Y_{it}, Y_{it-1}, D_i, M_i, \Delta Y_{it})$ denote the realized values of the random variables $(Y_t, Y_{t-1}, D, M, \Delta Y_t)$ for unit $i \in \mathcal{I}$.

There are two counterfactuals of interest: $Y_{it}(d)$ is the potential outcome of unit i in period t when the treatment D is fixed to $d \in \{0,1\}$, and $Y_{it}(d,m)$ is the potential outcome of unit i in period t when the treatment D is fixed to $d \in \{0,1\}$ and the moderator M is fixed to $m \in \mathcal{M}$.[21] We assume two regularity conditions:

Assumption A.1. $Y_{it-1} = Y_{it-1}(0) = Y_{it-1}(0, M_i)$, and $Y_{it} = Y_{it}(D_i) = Y_{it}(D_i, M_i)\ \forall\ i$.

Assumption A.2. $0 < P(D = 1|M = m) < 1$ for all $m \in \mathcal{M}$.

Assumption A.1 simply expresses observed outcomes in terms of the outcome counterfactuals. The preintervention outcome in period $t-1$ is not treated, while the postintervention outcome depends on the treatment status D. This assumption is often called no anticipation since the preintervention outcome does not depend on the treatment status of the post-intervention period. Assumption A.2 is a full support condition stating that there exist treated and untreated units for each value of the moderator M.

The main effects for a binary treatment $D \in \{0,1\}$ are the average treatment effect $ATE_t = E(Y_t(1) - Y_t(0))$ and the average treatment-on-the-treated effect $ATT_t = E(Y_t(1) - Y_t(0)|D = 1)$. In the presence of a moderator, we are most interested in the conditional version of these effects:

$$ATT_t(m) = E(Y_t(1,m) - Y_t(0,m)|D = 1, M = m), \tag{3}$$

$$ATE_t(m) = E(Y_t(1,m) - Y_t(0,m)|M = m). \tag{4}$$

The parameter $ATT_t(m)$ is the treatment-on-the-treated effect when we fix the moderator at the value m conditioned on the units that share the moderator value of $M = m$. Similarly,

[21]See Heckman and Pinto (2015) for a discussion on the fixing operator and Heckman and Pinto (2022) for a recent survey on causality.

$ATE_t(m)$ is the average treatment effect for units i such that $M_i = m$. We also define the following marginal average effects:

$$MATT_t(m) = \left.\frac{\partial ATT_t(m')}{\partial m'}\right|_{m'=m} \quad \text{and} \quad MATE_t(m) = \left.\frac{\partial ATE_t(m')}{\partial m'}\right|_{m'=m}. \tag{5}$$

The marginal treatment-on-the-treated effect $MATT_t(m)$ and the marginal average treatment effect $MATE_t(m)$ measure the respective variation in ATT_t and ATE_t for a marginal change in the moderator M. Otherwise stated, they measure the slope of $ATT_t(m)$ and $ATE_t(m)$ with respect to M at the point $m \in \mathcal{M}$. The relationships between average, conditional, and marginal effects are not trivial. In Appendix B.1, we provide a detailed discussion of these parameters and their relationships.

The parallel trend assumption ensures the identification of causal effects in the canonical two-treatment, two-period DiD design. The analogous assumption in the presence of a moderator variable is:

Assumption A.3. (Conditional Parallel Trends)

$$E[Y_t(0) - Y_{t-1}(0)|D = 0, M = m] = E[Y_t(0) - Y_{t-1}(0)|D = 1, M = m]\, \forall\, m \in \mathcal{M}.$$

Assumption **A.3** is a conditional version of the standard parallel trends assumption of the canonical DiD design. It states that the temporal trend of the untreated counterfactuals conditioned on the moderator M is the same for both the treatment and the control groups. The assumption renders the identification of the ATT_t effects:

Theorem T.1. Under **A.1**, **A.2**, and **A.3** $ATT_t(m)$ and ATT_t are identified by:

$$ATT_t(m) = E[\Delta Y_t|D = 1, M = m] - E[\Delta Y_t|D = 0, M = m], \tag{6}$$

$$\text{and } ATT_t = \int ATT_t(m)\frac{P(D = 1|M = m)}{P(D = 1)}dF_M(m). \tag{7}$$

Proof. See Appendix C.1. □

The identification of the treatment-on-the-treated effect $ATT_t(m)$ stems from the standard arguments of the canonical DiD model. The effect is identified by the difference between the temporal change in outcomes for treated and control (untreated) units. While the conditional trend assumption identifies $ATT_t(m)$ and ATT_t, it does not render a causal interpretation of the differences between treatment-on-the-treated effects $ATT_t(m') - ATT_t(m)$. Furthermore, it does not identify the average treatment effect ATE_t, which requires a stronger parallel trend assumption:[22]

Assumption A.4. (Strong Parallel Trends) For all $m \in \mathcal{M}$ and each $d \in \{0,1\}$,

$$E[Y_t(d,m) - Y_{t-1}(0,m)|M = m] = E[Y_t(d,m) - Y_{t-1}(0,m)|D = d, M = m].$$

The strong trend assumption states that the difference in the mean outcome over time for treated and control units is the same, given a moderator M. This implies that, for the control group ($D_i = 0$), the average time trend of the outcome given $M = m$ would be identical to that of the treatment group ($D_i = 1$) had it not been treated. Likewise, for the treatment group, the average time trend on the outcome given $M = m$ is equal to that of all units. This assumption eliminates a particular type of selection bias and allows us to identify ATE_t in the same way that the conditional trend assumption **A.3** identifies ATT_t :[23]

Theorem T.2. Under **A.1**, **A.2**, and **A.4** $ATE_t(m)$ and ATE_t are identified by:

$$ATE_t(m) = E[\Delta Y_t|D = 1, M = m] - E[\Delta Y_t|D = 0, M = m], \tag{8}$$

$$\text{and } ATE_t = \int ATE_t(m) dF_M(m). \tag{9}$$

Proof. See Appendix C.2. ☐

The identification of the average effects ATT_t in **T.1** and ATE_t in **T.2** may give the false impression that they can be as easily obtained as the estimates of a common TWFE regression. This intuition is incorrect. To prove this point, let us examine the TWFE regression model that includes interactions between the posttreatment indicator, moderator, and treatment indicator, in addition to time and unit fixed effects:[24]

$$Y_{it} = \theta_t + \eta_i + \gamma \cdot (D_i \cdot Post_t) + \kappa \cdot (M_i \cdot Post_t) + \beta_{DiD} \cdot (W_i \cdot Post_t) + v_{it}, \tag{10}$$

The following theorem examines the causal content of the β_{DiD} estimator of the regression above:

[22] We present these nonidentification analyses in Appendix B.2.

[23] Mathematically, the strong trend assumption **A.4** is not strictly stronger than the conditional trend assumption **A.3**. Indeed, **A.4** does not imply **A.3** or vice versa. Despite this fact, the strong trend assumption imposes an empirical constraint that is far more difficult to satisfy than the conditional trend assumption.

[24] The β_{DiD} estimator of the TWFE regression is numerically equivalent to the estimator obtained by the regression of the outcome time-difference ΔY_t on a constant term α, the treatment indicator D, the moderator M, and their interaction $W = D \cdot M$, that is $\Delta Y_{it} = \alpha + \gamma \cdot D_i + \kappa \cdot M_i + \beta_{DiD} \cdot W_i + v_i$.

Theorem T.3. Under standard OLS assumptions, the expected value of the estimator for β_{DiD} in (10) with uniform sampling weights is:

$$\beta_{DiD} = \frac{\text{Cov}(\Delta Y_t, M|D=1)}{\text{Var}(M|D=1)} - \frac{\text{Cov}(\Delta Y_t, M|D=0)}{\text{Var}(M|D=0)} \tag{11}$$

Consider the regression weights that assign the probability $P(M = m|D = 1)$ for each unit i such that $M_i = m$. Under Assumptions **A.1**, **A.2**, and Conditional Trends **A.3**, the expected value of the OLS estimator is:

$$\beta_{DiD} = \int MATT_t(m) \frac{E(M - E(M|D=1)|M > m, D=1)\left(1 - F_{M|D=1}(m)\right)}{\text{Var}(M|D=1)} dm. \tag{12}$$

Consider the regression weights that balance the distribution of the moderator variable between the treated and untreated groups of the sample. Under Assumptions **A.1**, **A.2**, and Strong Trends **A.4**, the expected value of the OLS estimator for β_{DiD} in (10) is:

$$\beta_{DiD} = \int MATE_t(m) \frac{E(M - E(M)|M > m)\,(1 - F_M(m))}{\text{Var}(M)} dm, \tag{13}$$

where $F_{M|D=1}(m) = P(M \leq m|D = 1)$ and $F_M(m) = P(M \leq m)$.

Proof. See Appendix C.3. □

Theorem **T.3** states that the ordinary least squares (OLS) estimator for β_{DiD} in (10) does not evaluate the average effects ATT_t or ATE_t. Instead, the estimator has a causal interpretation of a weighted average of marginal effects. Under the conditional parallel trend assumption, the β_{DiD} estimator evaluates a weighted average of the marginal effect $MATT_t$,[25] while under the strong parallel trend assumption, we obtain a weighted average of the $MATE_t$.[26]

The TWFE regression (10) discussed in **T.3** is different from the typical TWFE regression (2) used in MDiD designs. The main difference is that the TWFE in (10) includes all variable interactions, while the typical TWFE in (2) does not. Appendix B.3 shows that including these interactions is necessary for expressing the DiD estimator as a weighted

[25] By setting the weights to $P(M = m|D = 1)$, we are effectively modifying the distribution of the moderator of the control group to be equal to the distribution of the treatment group.

[26] The last weighting scheme in **T.3** balances the probability distribution of the moderator between treated and control groups. It sets the conditional probability distribution of the treated $P(M|D = 1)$ and the control $P(M|D = 0)$ groups to the unconditional distribution $P(M = m)$. In practice, this can be obtained by estimating the DiD parameter using an inverse probability weighting scheme where each data entry $(\Delta Y_{it}, D_i, M_i)$ is weighted by the inverse of the probability $P(M = m|D = d)$ such that $D_i = d \in \{0, 1\}$ and $M_i = m \in supp(M)$. See Appendix C.3 for further interpretation of these weighted effects.

average of marginal effects. Unfortunately, empirical restrictions often make it impossible to include all interactions. It is often the case that the untreated group is unavailable. An example of this empirical restriction is found in Card (1992), who examines the impact of the federal minimum wage increase on labor market outcomes. Technically, all US states belonged to the treatment group since the intervention was a federal policy. Card (1992) uses the fraction of workers initially earning less than the new minimum wage as a moderator for the policy change. Without a control group, our leading TWFE regression in (2) can be expressed as:

$$Y_{it} = \theta_t + \eta_i + \beta_{DiD} \cdot (M_i \cdot Post_t) + \epsilon_{it}. \tag{14}$$

Alternatively, the OLS estimator of β_{DiD} in (14) can be obtained by regressing the outcome time-difference on the moderator M :

$$\Delta Y_{it} = \alpha + \beta_{DiD} \cdot M_i + (\epsilon_{it} - \epsilon_{it-1}). \tag{15}$$

It is possible to obtain a causal interpretation of the linear regression above by invoking strong functional form assumptions. For instance, suppose that the counterfactual outcomes are determined by a simple linear model:[27]

$$Y_{it}(d) = \kappa_i + \tau_t + \beta_d \cdot M_i + \upsilon_{it} \text{ for } d \in \{0,1\}, \tag{16}$$

where υ_{it} denotes a mean-zero exogenous error term that is statistically independent of the moderator M. In this case, the causal effect of the treatment on the outcome for each agent i is given by $Y_{it}(1) - Y_{it}(0) = (\beta_1 - \beta_0) \cdot M_i$. The average effect for a given value $m \in \mathcal{M}$ of the moderator is $ATE_t(m) = ATT_t(m) = (\beta_1 - \beta_0) \cdot m$. In this case, the marginal effects are constant and given by $MATT_t = MATE_t = (\beta_1 - \beta_0)$. Moreover, we can express the outcome time-difference as follows:

$$\Delta Y_{it} = \underbrace{(\tau_t - \tau_{t-1})}_{\alpha} + \underbrace{(\beta_1 - \beta_0)}_{\beta_{DiD}} \cdot M_i + (\upsilon_{it} - \upsilon_{it-1}). \tag{17}$$

Comparing the OLS regression in Eq. (15) with the counterfactual difference in Eq. (17), we conclude that the β_{DiD} estimator evaluates the causal parameter $\beta_1 - \beta_0$, while the intercept α evaluates the time trend $\tau_t - \tau_{t-1}$. Most importantly, if we standardize the moderator M to have a mean of zero and a standard deviation of one, then the β_{DiD} estimator in Eq. (14) evaluates the treatment effect of the intervention on the outcome generated by an

[27]This linear specification rules out treatment heterogeneity or selection bias.

increase of one standard deviation in the moderator M. The following theorem explores trend assumptions to weaken the linearity requirement of Eq. (16).

Theorem T.4. Let Assumptions **A.1**-**A.2** hold and the counterfactual outcome for the untreated units be $Y_{it}(0) = \kappa_i + \tau_t + f_0(M_i) + \upsilon_{it}$, where $f_0(\cdot)$ denotes an unknown function and υ_{it} is a mean-zero exogenous error term statistically independent of the moderator M and the periods. Under the Conditional Parallel Trend **A.3**, the expected value of the OLS estimator for β_{DiD} in (14) is:

$$\beta_{DiD} = \int MATT_t(m) \frac{E(M - E(M)|M > m, D = 1)\left(1 - F_{M|D=1}(m)\right)}{\text{Var}(M|D = 1)(m)} dm.$$

If we assume that the unconditional distribution of the mediator is observed, then, under the Strong Parallel Trend **A.4**, the expected value of the estimator is given by:

$$\beta_{DiD} = \int MATE_t(m) \frac{E(M - E(M)|M > m)\,(1 - F_M(m))}{\text{Var}(M)} dm.$$

Proof. See Appendix C.4. □

Theorem **T.4** weakens the linearity requirement for counterfactual outcomes of untreated units and does not impose any restriction on the counterfactual outcomes of the treated group. The theorem states that, given these assumptions, the β_{DiD} estimator from OLS regression (14) evaluates a weighted average of marginal effects. Under conditional parallel trends, this estimator evaluates a weighted average of $MATT_t$, and under strong parallel trends, it evaluates a weighted average of marginal $MATE_t$.

The comparison between Theorems **T.4** and **T.3** reveals a trade-off between model assumptions and data availability. Theorem **T.4** invokes a functional form assumption for the untreated outcomes, while Theorem **T.3** makes use of regression (10), which includes all indicator interactions. These features are interchangeable since either set of assumptions yields a DiD estimator that is a weighted average of marginal effects.

As mentioned, our empirical study employs regression (14), which is widely used in the literature on DiD designs with a moderator variable. The next section discusses the implementation of this method.

4 Empirical Strategy

Our empirical strategy extends the MDiD model in Eq. (14) to account for preintervention variables that we wish to control for. We estimate the following model specification:[28]

$$Y_{ipt} = \theta_t + \eta_p + \delta_p t + \beta_{DiD}(M_p \cdot Post_t) + \boldsymbol{Z}'_{pt}\lambda + \boldsymbol{X}'_{ipt}\gamma + v_{ipt}, \tag{18}$$

where the dependent variable Y_{ipt} is the mental health outcome of individual i born in prefecture p and in birth year t. This variable can be a mental health outcome, such as the incidence of severe depression, or other individual outcomes. The moderator M_p is the $NTR\,Gap_p$ in Eq. (1). The post-PNTR dummy, $Post_t$, indicates if the individual experienced the PNTR intervention during infancy. It takes a value of one if an individual i was born in or after 2002. The DiD coefficient, β_{DiD}, captures the impact of exposure to PNTR in the year of birth on mental health outcomes during adolescence.

The specification (18) controls for several baseline characteristics and other trade policies interacted with the post-PNTR indicator. The variable $\boldsymbol{Z}_{pt}$ stands for the characteristics of the prefecture of birth and exposure to other trade policies p interacted with the post-PNTR indicator $Post_t$. These prefecture characteristics include the log of GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate for women aged 20–45 observed in 1990. The other trade policies include output tariffs, input tariffs, external tariffs, export licensing, barriers to investment in China, MFA quotas, and NTR rates observed in the preliberalization period. Measures on prefecture characteristics and other trade policies are described in detail in Appendix A.1 and A.2.

The specification also includes year of birth fixed effects, θ_t, accounting for aggregate shocks that affected all prefectures in a given birth year, and prefecture of birth fixed effects, η_p, which net out characteristics of birth prefectures that are time invariant. Moreover, it includes prefecture-of-birth-specific linear time trends, $\delta_p t$, which account for changes in time trends specific to each prefecture of birth across years. Finally, variable $\boldsymbol{X}_{ipt}$ denotes individual-level controls, including age, gender, father's and mother's age and indicator variables for whether the mother and father completed middle school. 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

[28]Note that the equation includes an additional prefecture index p relative to the MDiD model in Eq. (14). We could suppress the index since each individual i is associated with a single prefecture p. Nevertheless, the inclusion of index p facilitates comprehension of the equation.

gradually.

The CFPS dataset contains information on the mental health of respondents aged 10 and older. Hence, we include observations with nonmissing responses for mental health questions in the 2016 and 2018 rounds of the CFPS and who were at or below the age of 20 at the time of PNTR introduction in our baseline analysis.

Additionally, we estimate the following event study specification to explore the relationship between the timing at initial exposure to PNTR and later depression outcomes to test whether preexisting trends in the outcomes of interest drive our results. Specifically, we allow the impact of exposure to PNTR to vary with the age group at PNTR introduction in the prefecture of birth by replacing the post-PNTR indicator with a set of age group dummies at PNTR introduction, i.e., G1: newborn in 2006, G2: newborn in 2004–05, G3: newborn in 2002–03, G4: toddler (ages 1–2), G5: preschooler (age 3–5), G6: primary-school aged (ages 6–11), G7: middle-school aged (ages 12–14), G8: high-school aged (ages 15–17) and G9: early adulthood (ages 18–20). Individuals from groups G1–G3 were exposed to PNTR in the year of birth, groups G4 and G5 were exposed in early life, and the other groups were exposed at later ages. We omit those not exposed to the PNTR until early adulthood (G9), so the estimates are relative to the outcomes for that age group.

$$Y_{ipt} = \theta_t + \eta_p + \sum_{t \in G1}^{G8} \beta_t \cdot (1\{T_i = t\} \times M_p) + \boldsymbol{Z}'_{pt}\lambda + \boldsymbol{X}'_{ipt}\gamma + \epsilon_{ipt} \tag{19}$$

where T_i denotes the birth year of individual i, M_p stands for the time-invariant prefecture-level NTR gap in Eq. (1), and the control variables are the same as those in Eq. (18). The summation on the right-hand side contains the interaction of age group dummies (excluding G9) with the moderator M_p. This specification is commonly used to test the parallel trend assumption. Indeed, if the mediator were a binary variable $M_p \in \{0, 1\}$, and the intervention had no impact on prefectures p such that $M_p = 0$, then β_t would evaluate the regression-adjusted difference in means of the outcome between groups with high and low values of the moderator M_p for each period t. If the common trend assumption holds, this difference should be statistically insignificant for the periods before the intervention and significant for the periods after the intervention. In our empirical setting, the estimates for β_t evaluate the linear relationship between the mental health outcome and the NTR gap for each period t. A similar rationale applies. 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 while being statistically significant for the periods after the intervention.

5 Results

5.1 Mental Health Outcomes

We report our primary estimates of Eq. (18) in Table 2. The coefficient estimates in Panel A are all very close to zero and insignificant, implying no evidence of a significant impact of PNTR on the probability of displaying symptoms of any depression. 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 significant declines in their incidence of severe depression. As discussed in Section 4, the most rigorous specifications including individual controls and the post-PNTR indicator 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 a one standard deviation increase in exposure to PNTR is associated with a decline in the probability of experiencing severe depression by 4.7 percentage points, or 52 percent of the pre-PNTR outcome mean, in cohorts born in more PNTR-exposed prefectures relative to those born in less affected regions in China. These results contribute to a substantial body of evidence documenting the impact of early life conditions on mental health outcomes during young adulthood.[29]

We also test whether the estimated effects for severe depression vary by length of exposure to PNTR. The event study estimates presented in Figure 1 allow us to see the relationship between birth year and the timing of the trade liberalization in greater detail. The horizontal axis represents the age in 2002, the year of the policy change. As we move from left to right, we have individuals who lived a larger share of their childhood after the implementation of PNTR in 2002. Although the coefficient estimates on the interaction of the year of birth with the NTR gap are indistinguishable from zero for observations born prior to 2002 (i.e., for those who spent only part of their childhood benefiting from the policy change) in Figure 1, the negative slope here indicates that as the age at initial exposure decreases, the impacts on severe depression become larger. For adolescents born after the implementation of PNTR in 2002, the coefficient estimates shift down and become significantly different from zero at the 5 percent significance level. Hence, Panel B in Figure 1 suggests that the largest effects are observed for adolescents who lived all their early childhood (i.e., from age 0 on) enjoying the advantages of the new trade policy

[29]For examples of works in this literature, see: Walker et al. (2022); Gertler et al. (2014); Hertzman (1999); Campbell et al. (2014) .

in prefectures more exposed to PNTR. In line with our results in Table 2, Panel A in Figure 1 shows no evidence of a significant impact on the probability of having any depression.

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 (Panel B) value on the NTR gap, to winsorizing the NTR gap at the 5^{th} and 95^{th} 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). 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 (Panel A) and to using year-of-birth fixed effects interacted with the control variables instead of the post-PNTR indicator interacted with the control variables (Panel B).

Finally, we examine whether the policy change led to any significantly heterogeneous treatment effects by gender, mother's education, parental absence during early childhood, and initial share of the rural population at the prefecture level in 1990. The results reported in Appendix Table A4 indicate no evidence of heterogeneity in the treatment effects on these dimensions, including whether the adolescent is female (Panel A), whether the mother completed middle school (Panel B), whether the parents were absent for at least one week when their child was between 0 and 3 years old (Panel C), or whether the initial share of the rural population is above the median, representing more rural locations (Panel D).

5.2 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 three subsections by focusing on the effects of the trade policy reform on the following outcomes: (a) parental income, (b) early life investments, and (c) migration and fertility.

5.2.1 Parental Income

One potential mechanism through which early exposure to PNTR might have led to decreased severe 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 rise in the income of households working in nontradable sectors through positive local labor demand effects. Such positive income effects during infancy could have large developmental effects that persist over time, leading to better mental health outcomes in adolescence.

Using the CHNS data, Table 3 reports the results. The coefficient estimates are positive and statistically significant, indicating that households in prefectures more exposed to PNTR experienced an increase in their household income level compared to households in less exposed prefectures. These results are consistent with the finding in Erten and Leight (2021) that regions with greater exposure to PNTR exhibited higher GDP per capita after the change in China's PNTR status.

5.2.2 Early Life Investments

Extensive literature demonstrates that early childhood investments yield long-lasting benefits that extend well into adulthood (Heckman et al. 2013; Campbell et al. 2014; Carneiro and Ginja 2014; Gertler et al. 2014; Heckman et al. 2010; Conti et al. 2016). This body of literature has established that augmenting investments in children during their early years leads to lifetime gains in various domains, such as education, earnings, behavior, and health (Kautz et al. 2014; Heckman 2006b, 2007b; García et al. 2018; García et al. 2019, 2021; Elango et al. 2016).

Our research aligns with the early childhood literature since the positive income shocks induced by the trade policy lead to greater parental investments in early childhood development. Specifically, we find that the policy change induced a significant increase in the frequency of prenatal visits, a greater likelihood of breastfeeding, and a higher number of vaccinations. Furthermore, an increase in parental income can enhance the nutrition intake of children by easing parents' budget constraints. This increase in resources may allow parents to purchase more expensive food items, which can lead to improved nutrition for their children. Such improvements in early life investments contribute to explaining the positive impact on mental health outcomes.

Table 4 reports the results for changes in early life investments in response to the trade policy change. The estimates in columns (1) and (2) are positive and significant, indicating a relative increase in prenatal visits of pregnant mothers and a relative increase in the likelihood of the child being breastfed in response to the policy change in more affected regions.[30] The estimate in column (3) shows that infants born after the policy change in

[30]To estimate the impact of contemporary exposure to the trade reform on the probability of prenatal

more affected prefectures received more vaccines than infants in other prefectures.[31] The estimates for individual vaccines reported in columns (4)–(9) indicate that the probability of receiving all vaccines significantly increased, with the exception of encephalitis B and measles, for which the estimates are 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 5, we present estimates for whether the policy change affected the nutrition intake and development of children using CHNS data. The column (3) estimates indicate that children in more exposed prefectures experienced a relative increase in their total caloric intake, driven mostly by a significantly higher intake of carbohydrates.[32] In Panels E and F, we also observe a positive impact of the reform on the current height and 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 Table 6, we test whether the reform affected childcare arrangements and early childcare education using CHNS data. Indeed, the column (1) estimate in Panel A shows that compared to children in less exposed prefectures, children born in prefectures more affected by the policy change were more likely to be cared for by people outside the household over the past week. Columns (2) and (3) show that treated children also experienced a significant relative increase in the hours of care per day and days of care per week that they received from people outside of the household. Moreover, the column (5) estimate indicates a significant relative increase in the probability of attending kindergarten for children born in more affected regions after the policy change. On the other hand, the estimates in Panel B indicate no evidence of a significant change in the probability of the caregiver being a nanny, relative, neighbor, or grandparent. Overall, these results support

visits and breastfeeding, we estimate the following specification using data from the 1993–2015 CHNS:

$$Y_{ipt} = \theta_t + \eta_p + \delta_p t + \beta(Post_t \times M_p) + \boldsymbol{Z}'_{pt}\lambda + \boldsymbol{X}'_{ipt}\gamma + v_{ipt} \tag{20}$$

where the dependent variable Y_{ipt} is the prenatal care or breastfeeding outcome for woman i living in prefecture p in year t. The moderator M_p is the $NTR\,Gap_p$ in Eq. (1). Policy exposure is captured by the interaction of the moderator and a post-PNTR dummy, $Post_t$, equal to one for the period after 2001. The terms θ_t, η_p, and $\delta_p t$ are year fixed effects, prefecture fixed effects, and prefecture-specific linear time trends, respectively. The set of individual controls $\boldsymbol{X}'_{ipt}$ includes the woman's age and middle school completion status. Additional controls at the prefecture-year level $\boldsymbol{Z}'_{pt}$ include the post-PNTR indicator interacted with other trade policies and prefecture initial characteristics as described in Section 4. We cluster standard errors at the prefecture level.

[31] We estimate Eq. (18) 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.

[32] We also observe an imprecisely estimated relative increase in protein and fat intake for exposed children after the reform.

the view that positive income shocks induced by the reform allowed parents to substitute care-giving at home with care provision in formal early childhood education centers. Since parents in China have to pay to send their children to these centers (Gong et al. 2015; Feng 2017; Wang and Gong 2019), a relaxation of income constraints have allowed them to afford these paid early child education services.

We also explore whether the policy change affected physical health, cognitive function, or school dropout rates during adolescence.[33] The coefficient estimates in Appendix Table A1 show no evidence of a significant change in these outcomes 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 and cognition of adolescents, they may nevertheless have lasting effects in reducing the risk of severe depression among adolescents.

5.2.3 Migration and Fertility

Finally, we test whether the policy change affected migration and fertility patterns in more affected regions, resulting in a potentially selected sample. If the increase in export-oriented jobs attracted higher-quality workers in more exposed regions, the children of these parents might be positively selected. To test for this possibility, we use the CFPS data focusing on the observations for whom we have mental health outcomes and construct three indicator variables: (i) an indicator variable equal to 1 if the respondent migrated to a different prefecture from the prefecture of birth, (ii) an indicator variable equal to 1 if the respondent migrated to a different prefecture from the prefecture of residence after the age of 12, and (iii) an indicator variable equal to 1 if the respondent changed his or her residence permit from rural to urban to migrate from a rural to an urban area after the age of 12. Appendix Table A5 reports the results from testing whether the PNTR had a significant impact on the migration outcomes of adolescents in our sample. The coefficient estimates are null, implying no evidence of a significant impact of early exposure to PNTR on the migration outcomes of adolescents.

Moreover, we use population census data from 2000 to 2015 and create prefecture-level migration measures following Imbert et al. (2022).[34] The results presented in Appendix Table A6 show no evidence of a significant change in the immigration rate in the destination prefecture (Panel A) or the emigration rate from the origin prefecture (Panel B) for the

[33]While dropping out of school is an extreme outcome to proxy school performance, we do not observe any other appropriate performance indicators in the CFPS dataset.

[34]We use the sample of migrants of prime reproductive ages 20–45 who moved across prefectures for new employment opportunities since this age group is more representative of parents with children in their earlier years of life. The results are very similar for the prime working-age population.

total population, men, or women in response to the policy change.

An additional possibility is that the policy change may have led to parental absence in more affected regions if some parents migrated to other regions for better employment opportunities. Indeed, the restrictive household registration (*hukou*) system renders it difficult for migrant parents to bring their children with them, creating a large pool of "left behind" children in rural areas (Heckman and Yi 2012; Tong et al. 2019). We test whether this particular trade policy change may have contributed to parental absence in more affected regions by using three indicators: (i) an indicator variable that takes a value of 1 if the parents were absent for at least one week when their child was between 0 and 3 years old based on the CFPS data, (ii) an indicator variable that takes a value of 1 if the mother was not living in the household at the time of the interview and was seeking employment elsewhere based on CHNS data, and (iii) an indicator variable that takes a value of 1 if the father was not living in the household at the time of interview and was seeking employment elsewhere based on the CHNS data. The coefficient estimates presented in Appendix Table A7 indicate no evidence of a significant impact of the policy change on these parental absence outcomes.

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. Appendix Table A8 provides estimates for three 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, (ii) the number of children for women living in a prefecture, and (iii) the percent of women who have had a child. 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.

6 Conclusion

This study uses the 2002 US bill that granted China PNTR status 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 moderated DiD design to evaluate how the impacts on early life circumstances induced by the trade liberalization affected adolescent mental health in China.

We evaluate an MDiD model in which the impact of the intervention is moderated by the trade exposure of each Chinese prefecture. We investigate the requirements for causal inference through MDiD designs and present a menu of assumptions that allow a causal interpretation of the MDiD estimators. Our theoretical contributions align with a recent DiD literature that demonstrates that causal inference through DiD designs that depart from the canonical case is not trivial.

Using a nationally representative sample of households in China, we find that adolescents born in prefectures more exposed to a plausibly exogenous change in international trade policy experienced a significant and economically meaningful decline in the incidence of severe depression relative to that of the same birth cohort born in other regions. A one standard deviation change in prefectures' exposure to PNTR is associated with a decline in the probability of experiencing severe depression of 4.7 percentage points, which is slightly more than half the mean.

Exploring potential channels, we find that compared to households in less affected prefectures, households in more affected prefectures experienced an increase in their household income after the reform. These positive income shocks increased early life investments in children. Specifically, these children were more likely to be breastfed and they experienced higher caloric intake, increased prenatal visits and vaccinations. They were more likely to attend early childhood education centers, as parents could afford to pay for these educational services. Finally, our findings are not explained by factors such as selective migration, parental absence, or fertility declines in response to the policy change.

Recent evidence from adolescent mental health studies indicates that mental health treatments through cognitive behavioral therapy (CBT) or a combination of CBT and antidepressant medication can reduce depressive symptoms by 43 to 70 percent after 12 weeks (Lewandowski et al. 2013; Kennard et al. 2009; March et al. 2004). We show that improving access to advanced country markets by one standard deviation at the local labor market level can reduce the prevalence of severe depression by approximately 50 percent. Since the recent WHO estimates for the cost-effectiveness ratio of mental health treatments for adolescents in upper middle-income settings range from US$1,000–$5,000 per healthy life year gained (World Health Organization 2021),[35] trade policies that improve employment and earnings opportunities for households as a means of preventing depression later in life might be more cost-effective. The case for such policies is even

[35] This cost-effectiveness ratio is based on the WHO recommendation of implementing universal, school-based social-emotional learning programs to improve mental health and prevent suicide in adolescents. The targeted interventions at the school level for such programs are estimated to have a higher cost-effectiveness ratio of US$10,000–$50,000 per health life year gained. Note also that China is an upper middle-income country by the WHO's definition.

stronger when the total economic costs of depression in China, which are estimated to reach US$6,264 million annually (Hu et al. 2007) (84 percent of which are productivity losses due to illness), are taken into account.

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. 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 of the welfare importance of these reforms, while already large, are underestimated to the extent that they do not account for mental health effects.

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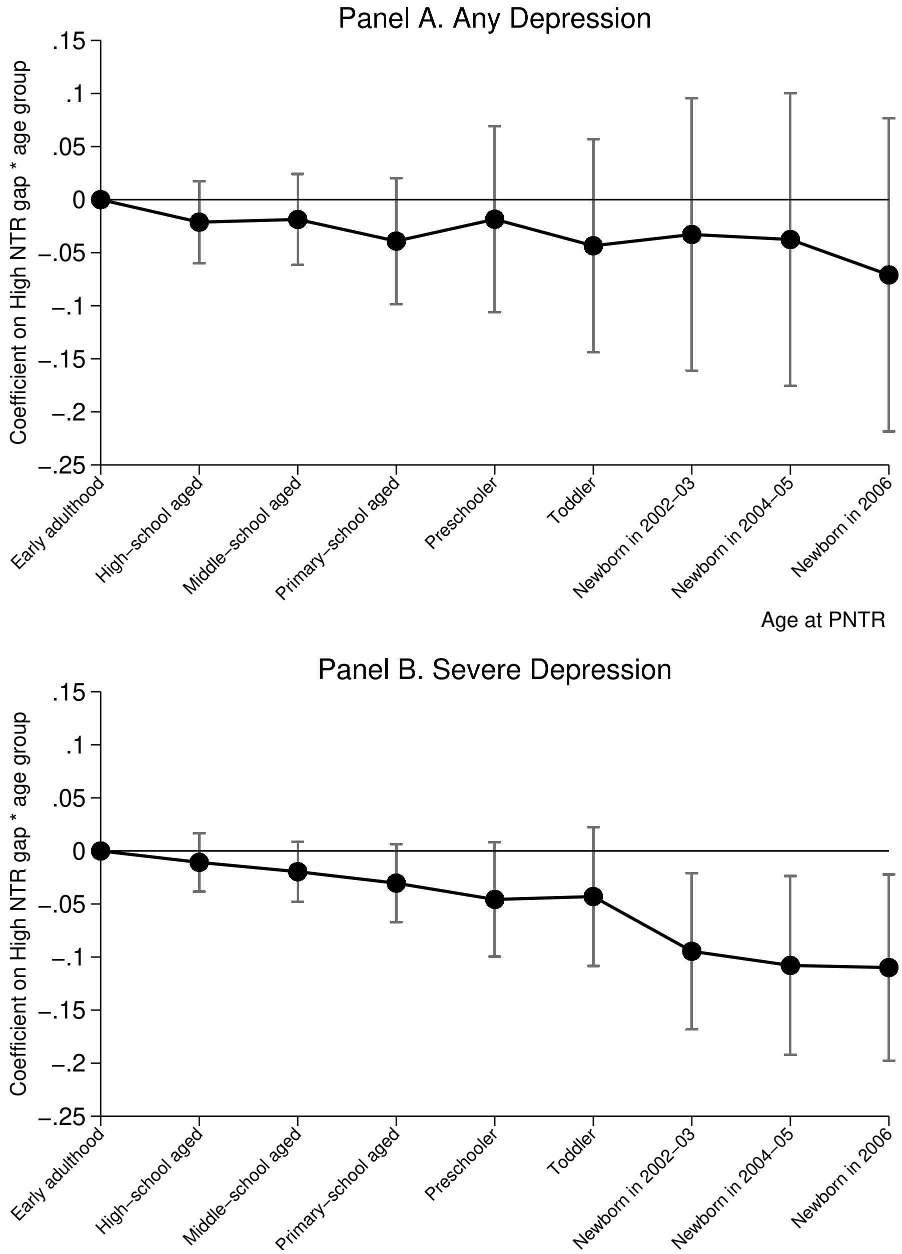
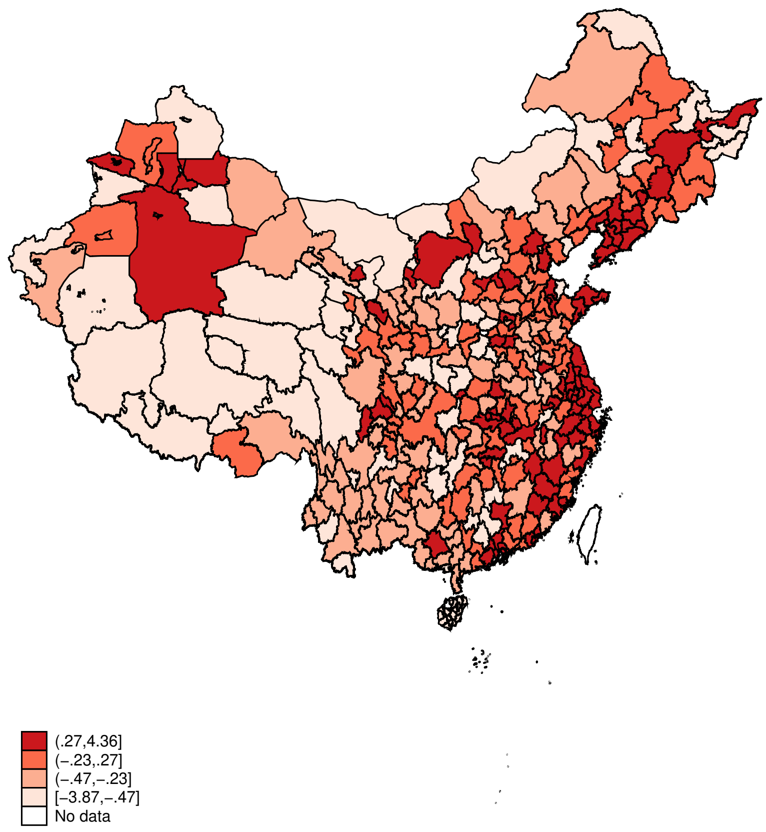
Figure 1: Event Study: Exposure to PNTR and Severe Depression Incidence



Note: This figure plots the coefficients and 95% confidence intervals from an event-study regression that compares the incidence of any depression (Panel A) and severe depression (Panel B) in prefectures that are more exposed to the PNTR shock to those that are less exposed for each cohort born before and after the policy change. Event time is defined as age group when PNTR was implemented, and groups include G1: newborn in 2006, G2: newborn in 2004-05, G3: newborn in 2002-03, G4: toddler (age 1-2), G5: preschooler (age 3-5), G6: primary-school aged (age 6-11), G7: middle-school aged (age 12-14), G8: high-school aged (age 15-17) and G9: early adulthood (age 18-20). Age group 18-20 is the omitted cohort so estimates are relative to that point. Data are from the 2016–2018 CFPS.

Figure 2: 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).

Table 1A: Summary Statistics

	Obs (1)	Mean (2)	S.D. (3)	Min (4)	Max (5)
Panel A. CFPS 2016-2018: cohorts born between 1982 and 2006					
Health and cognition outcomes					
Any depression	14,521	0.258	0.438	0.000	1.000
Severe depression	14,521	0.082	0.274	0.000	1.000
Physical health index	14,519	0.003	0.997	-4.362	0.478
Cognitive function index	10,220	0.021	0.966	-5.536	3.885
Demographic characteristics					
Age	14,521	22.696	7.243	10.000	36.000
Male	14,521	0.549	0.498	0.000	1.000
Father's age	14,521	50.201	8.340	28.000	89.000
Mother's age	14,521	48.340	8.087	27.000	148.000
Completed middle school - Father	14,521	0.525	0.499	0.000	1.000
Completed middle school - Mother	14,521	0.363	0.481	0.000	1.000
Panel B. CHNS 1993-2015					
Early life investment					
Ever married women during the sample period of survey					
Most recent pregnancy: any prenatal checkups	2,525	0.833	0.373	0.000	1.000
Women pregnant during the sample period of survey					
Ever breastfed child	1,804	0.930	0.255	0.000	1.000
Vaccinations: cohorts born between 1982 and 2006					
No. of vaccinations	1,429	1.753	2.215	0.000	9.000
BCG vaccination	1,429	0.213	0.410	0.000	1.000
Hepatitis B vaccination	1,429	0.185	0.388	0.000	1.000
DPT vaccination	1,429	0.272	0.445	0.000	1.000
Encephalities B vaccination	1,429	0.164	0.371	0.000	1.000
Measles vaccination	1,429	0.233	0.423	0.000	1.000
Polio vaccination	1,429	0.338	0.473	0.000	1.000
Nutrition intake over the past 3 days: children aged 0-6					
Calories	3,446	5.081	0.484	0.978	6.603
Protein	3,446	7.008	0.435	4.359	8.967
Carbohydrate	3,446	3.398	0.740	-1.427	6.664
Fat	3,446	3.532	0.467	0.798	4.818
Child development: cohorts born between 1982 and 2006					
Current height in cm	14,554	138.987	26.845	10.600	193.000
Current weight in kg	14,554	37.944	19.501	3.000	159.000

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 1993, 1997, 2000, 2004, 2006, 2009, 2011 and 2015 CHNS samples, respectively. All variables are summarized at the individual level.

Table 1B: Summary Statistics

	Obs (1)	Mean (2)	S.D. (3)	Min (4)	Max (5)
Panel B. CHNS 1993-2015: cohorts born between 1982 and 2006 (Cont')					
Childcare from people outside the household: children aged 0-6					
Whether cared for by people outside household	2,754	0.350	0.477	0.000	1.000
Hours per day cared for by people outside household	2,624	2.293	3.958	0.000	24.000
Days per week cared for by people outside household	2,667	1.619	2.522	0.000	7.000
Whether cared for in nursery school	2,638	0.054	0.226	0.000	1.000
Whether cared for in kindergarten	2,654	0.179	0.383	0.000	1.000
Whether cared for by the nanny	2,644	0.126	0.332	0.000	1.000
Whether cared for by relatives	2,621	0.011	0.105	0.000	1.000
Whether cared for by the neighbor	2,622	0.015	0.121	0.000	1.000
Whether cared for by grandparents	2,655	0.113	0.317	0.000	1.000
Demographic characteristics: children aged 0-12					
Age	3,446	4.034	1.561	0.000	6.000
Male	3,446	0.547	0.498	0.000	1.000
Father's age	3,446	32.405	5.340	21.000	70.000
Mother's age	3,446	30.856	4.933	18.000	61.000
Completed middle school - Father	3,446	0.671	0.470	0.000	1.000
Completed middle school - Mother	3,446	0.545	0.498	0.000	1.000
Panel C. CHNS 1993-2015					
Household income per capita (in 1,000 *yuan*)	30,643	11.458	12.770	0.000	68.764
Household head's age	30,643	53.009	13.277	16.000	98.000
Household head is male	30,643	0.836	0.371	0.000	1.000
Household head completed middle school	30,643	0.507	0.500	0.000	1.000
Panel D. Census 1990-2015					
Migration: population aged 20-45					
Immigration rate					
Total population	1,312	0.064	0.180	0.000	3.237
Male	1,312	0.030	0.047	0.000	0.388
Female	1,312	0.018	0.035	0.000	0.376
Emigration rate					
Total population	1,312	0.078	0.079	0.000	0.569
Male	1,312	0.046	0.046	0.000	0.347
Female	1,312	0.032	0.034	0.000	0.230
Fertility: females aged 20-45					
Number of births per 1,000 women	1,655	25.668	39.062	0.000	179.763
Total number of children	1,655	4011.897	4475.676	95.000	55557.000
Share of women who had children	1,655	0.802	0.064	0.436	0.945

Notes: Panel C presents the summary statistics of household income per capita (in 1,000 *yuan*) and household head's demographic characteristics from the 1993-2015 CHNS households. Panel D presents the summary statistics of migration (immigration and emigration) separately for the total, male and female population, and fertility for women aged 20 to 45 years. Migration data are from the 2000 (the earliest census year where migration data are available), 2005, 2010 and 2015 population census samples. Migration is defined as migrants aged 20-45 who moved across prefectures to seek jobs. Immigration rate is measured as the ratio of migrants who arrived in a given destination prefecture to the total number of non-migrant residents in that prefecture. Emigration rate is measured as the share of migrants who left a given prefecture to the total number of residents in that prefecture. Fertility data are 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. Any depression			
Post × NTR gap	-0.003	0.002	0.003
	(0.030)	(0.032)	(0.033)
Observations	14521	14521	14521
Control mean	0.28	0.28	0.28
Panel B. Severe depression			
Post × NTR gap	-0.035**	-0.046**	-0.047***
	(0.017)	(0.018)	(0.018)
Observations	14521	14521	14521
Control mean	0.09	0.09	0.09
Prefecture-of-birth fixed effects	Yes	Yes	Yes
Year-of-birth fixed effects	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes
Post × 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 mental health outcomes on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions in column 1 control for prefecture of birth fixed effects, year of birth fix effects, prefecture-specific linear trend in year of birth, and the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity. Regressions in column 2 further control for the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate. Regressions in column 3 further control for individual characteristics including age, gender, father's and mother's age, and indicator variables for whether the mother and father completed middle school. Standard errors are clustered at the prefecture of birth level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table 3: Impact of PNTR on Household Income

	(1)	(2)	(3)
Post × NTR gap	1.641*	3.300**	3.508***
	(0.967)	(1.329)	(1.275)
Observations	30643	30643	30643
Control mean	4.81	4.81	4.81
Prefecture fixed effects	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes
Post × Initial prefecture characteristics		Yes	Yes
Household head's characteristics			Yes

Notes: Data are from the 1993–2015 CHNS. All columns report the DiD regressions of household income per capita (in 1,000 RMB) on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regression in column 1 controls for prefecture fixed effects, survey year fixed effects, prefecture-specific linear time trend, and the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity. Regression in column 2 further controls for the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate. Regression in column 3 further controls for household head's age, household head's gender, indicator for whether the household head completed middle school. 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 Early Life Investments

	Prenatal visits (1)	Breast-feeding (2)	Number of vaccinations (3)	BCG vaccination (4)	Hepatitis B vaccination (5)
Post × NTR gap	0.137**	0.121*	0.500**	0.137**	0.101*
	(0.055)	(0.065)	(0.187)	(0.053)	(0.055)
Observations	2525	1804	1426	1426	1426
Control mean	0.79	0.94	1.75	0.20	0.14
Prefecture fixed effects	Yes	Yes	Yes	Yes	Yes
Survey/Birth year fixed effects	Yes	Yes	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes	Yes	Yes
Post × 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	0.174***	0.053	0.064	0.113**
	(0.058)	(0.055)	(0.046)	(0.048)
Observations	1426	1426	1426	1426
Control mean	0.28	0.16	0.24	0.36
Prefecture fixed effects	Yes	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes	Yes
Post × Initial prefecture characteristics	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes

Notes: Prenatal visit and breastfeeding information come from data of pregnant women for the sample period of 1993-2015 CHNS. The regressions in columns (1)-(2) control for prefecture fixed effects, survey year fixed effects, prefecture-specific linear time trend, and the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity, the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate, and individual characteristics including mother's age and whether completed middle school. Information on vaccinations reported in columns (3)–(9) come from data on children born from 1982 to 2006 included in the 1993-2015 CHNS. Regressions control for the child's prefecture fixed effects, year of birth fixed effects, prefecture-specific linear time trend, and the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity, the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate, and individual characteristics including the child's age, gender, father's and mother's age, and indicator variables for whether the mother and father completed middle school. Standard errors are clustered at the prefecture level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table 5: Impact of PNTR on Nutrition Intake Outcomes

	(1)	(2)	(3)
Panel A. Total Calories			
Post × NTR gap	0.297**	0.398***	0.241**
	(0.123)	(0.122)	(0.105)
Observations	3446	3446	3446
Control mean	7.07	7.07	7.07
Panel B. Protein			
Post × NTR gap	0.264**	0.173*	0.041
	(0.104)	(0.091)	(0.086)
Observations	3446	3446	3446
Control mean	3.57	3.57	3.57
Panel C. Carbohydrate			
Post × NTR gap	0.361**	0.487***	0.318**
	(0.136)	(0.134)	(0.119)
Observations	3446	3446	3446
Control mean	5.19	5.19	5.19
Panel D. Fat			
Post × NTR gap	0.203	0.274	0.121
	(0.160)	(0.177)	(0.158)
Observations	3446	3446	3446
Control mean	3.35	3.35	3.35
Panel E. Height			
Post × NTR gap	5.205	6.519**	4.878**
	(3.662)	(2.987)	(1.854)
Observations	14554	14554	14554
Control mean	141.56	141.56	141.56
Panel F. Weight			
Post × NTR gap	2.301	3.455*	2.161*
	(2.068)	(1.725)	(1.282)
Observations	14554	14554	14554
Control mean	39.50	39.50	39.50
Prefecture fixed effects	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes
Post × Initial prefecture characteristics		Yes	Yes
Individual characteristics			Yes

Notes: Data are from the 1993–2015 CHNS. This table reports results of the DiD regressions of nutrition intakes on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions in column 1 control for prefecture fixed effects, survey year fixed effects, prefecture-specific linear time trend, and the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity. Regressions in column 2 further control for the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate. Regressions in column 3 further control for individual characteristics including child age, gender, father's and mother's age and indicator variables for whether the mother and father completed middle school. Standard errors are clustered at the prefecture level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table 6: Impact of PNTR on Childcare and Early Childhood Education

Panel A. Cared for by people outside the household over the past week

	Yes/ No (1)	Hours per day in a typical day (2)	Days per week in a typical week (3)	Cared for in nursery school (4)	Cared for in kindergarten (5)
Post × NTR gap	0.365**	2.901**	1.756**	0.025	0.280*
	(0.157)	(1.223)	(0.840)	(0.067)	(0.145)
Observations	2754	2624	2667	2638	2654
Control mean	0.33	2.15	1.59	0.05	0.14
Prefecture fixed effects	Yes	Yes	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes	Yes	Yes
Post × Initial prefecture characteristics	Yes	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes	Yes

Panel B. Was care in ...'s home/facility over the past week

	Cared for by nanny (6)	Cared for by relatives (7)	Cared for by neighbor (8)	Cared for by grandparents (9)	Cared for at other facility (10)
Post × NTR gap	0.131	-0.012	0.012	0.073	0.122
	(0.116)	(0.022)	(0.041)	(0.114)	(0.164)
Observations	2644	2621	2622	2655	2643
Control mean	0.14	0.01	0.02	0.12	0.09
Prefecture fixed effects	Yes	Yes	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes	Yes	Yes
Post × Initial prefecture characteristics	Yes	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes	Yes

Notes: This table reports results of the DiD regressions of childcare and early childhood education on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Information of childcare provision and early childhood education come from data of children aged 0-6 from the 1993-2015 CHNS. The regressions control for prefecture fixed effects, survey year fixed effects, prefecture-specific linear time trend, and the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity, the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate, and individual characteristics including the child's age, gender, father's and mother's age, and indicator variables for whether the mother and father completed middle school. Standard errors are clustered at the prefecture level. ***, **, 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.

- Years of education: Prefecture's average years of education of population aged above six.
- Population age: Prefecture's average population age.
- Fertility rate: Prefecture's average births to women aged 20 to 45 years old.
- Child population: Prefecture's total population under age 18.

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.

- 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.

Appendix B Causal Analyses

B.1 Relationships between Average, Conditional, and Marginal Effects

Section 3 of the main paper defines three causal effects of interest. The average effects are ATT_t, ATE_t; the conditional effects are $ATT_t(m)$ in (3) and $ATE_t(m)$ in (4); and the marginal effects are $MATT_t(m)$ and $MATE_t(m)$ in (5).

The relationship between average and conditional effect is simple. We can obtain the average effects ATT_t and ATE_t by integrating the conditional effects $ATT_t(m)$ in (3) and $ATE_t(m)$ in (4) over the associated probability distribution of M. For instance, the average causal effect is given by:

$$ATE_t \equiv E(Y_t(1) - Y_t(0)) = \int E(Y_t(1,m) - Y_t(0,m)|M = m)dF_M(m) = \int ATE_t(m)dF_M(m),$$

while the average treatment effect on the treated is:

$$\begin{aligned} ATT_t \equiv E(Y_t(1) - Y_t(0)|D = 1) &= \int E(Y_t(1,m) - Y_t(0,m)|M = m, D = 1)dF_{M|D=1}(m), \\ &= \int ATT_t(m)dF_{M|D=1}(m), \end{aligned}$$

where $F_M(m) = P(M \leq m)$ denotes the cumulative distribution function of the moderator and $F_{M|D=1(m)} = P(M \leq m|D = 1)$ is the conditional cumulative distribution.

The relationship between average and marginal effects is not as straightforward. Consider the case of a continuous moderator M whose support is given by the interval $\mathcal{M} = [\underline{m}, \overline{m}]$. Note that the integral of $MATT_t$ or $MATE_t$ in (5) over the support of the moderator M does not deliver ATT_t or ATE_t. In the case of $MATE_t$, we have that:

$$\int_{\underline{m}}^{\overline{m}} MATE_t(m)dm = ATE_t(\overline{m}, \overline{m}) - ATE_t(\underline{m}, \underline{m}). \tag{21}$$

The next proposition clarifies the relationship between the average, marginal and conditional effects:

Proposition P.1. Consider a DiD model where **A.1**–**A.2** holds, M is a continuous random variable in $[\underline{m}, \overline{m}]$, and $ATE_t(m)$ in (4) be a differentiable function. Then for any value $m^* \in [\underline{m}, \overline{m}]$ we have that:

$$ATE_t = \int_{\underline{m}}^{\overline{m}} MATE_t(m)\Big(\mathbf{1}[m > m^*]\,(1 - F_M(m)) - \mathbf{1}[m < m^*]F_M(m)\Big)dm + ATE_t(m^*|m^*). \tag{22}$$

If $ATT_t(m)$ in (3) is differentiable, then (22)–(24) also hold if we were to replace ATE_t, $MATE_t$, $ATE_t(m^*)$, $F_M(m)$ by ATT_t, $MATT_t$, $ATT_t(m^*)$, $F_{M|D=1}(m)$ respectively.

Proposition P.1 shows that is it possible to express the average treatment effect in terms of the marginal response $MATE_t$ and the conditional effect $ATE_t(m|m)$. The proposition states that for any value m^* of the moderator, the difference between ATE_t and the conditional effect $ATE_t(m^*)$ can be expressed as a weighted average of the marginal effect $MATE_t(m)$ over the moderator's probability distribution. Moreover, the weights for $MATE_t(m)$ such that $m > m^*$ are positive and given by $(1 - F_M(m))$, while the weights for $MATE_t(m)$ such that $m > m^*$ are the negative values of the CDF $F_M(m)$.

Now suppose there are values of the moderator that make the treatment ineffective. Notationally, this means that there is a set $\mathcal{M}_0 \subset \mathcal{M}$ such that for any value $m_0 \in \mathcal{M}_0$ we have the following:

$$Y_{it}(d, m_0) = Y_{it}(0, m_0); d \in \{0, 1\} \text{ for all units } i \in \mathcal{I}.$$

This means that [36]

In our empirical setting, M moderates the impact of trade tariffs on the economy of the Chinese prefecture and D is the policy of tariff changes. The economic impact of the policy depends on the share of the prefecture's industries targeted by the tariff change. The policy has limited effect on prefectures with closed economies or those whose industries are not targeted by the policy. In this case $M_i = 0 \equiv m_0$.

A consequence of Proposition P.1 is that for any value $m_0 \in \mathcal{M}_0 \subset [\underline{m}, \overline{m}]$ we have that:

$$ATE_t = \int_{\underline{m}}^{\overline{m}} MATE_t(m)\Big(\mathbf{1}[m > m_0]\,(1 - F_M(m_0)) - \mathbf{1}[m < m_0]F_M(m)\Big)dm. \tag{23}$$

$$\text{Moreover, if } \underline{m} \in \mathcal{M}_0, \text{ then } ATE_t = \int_{\underline{m}}^{\overline{m}} MATE_t(m)\,(1 - F_M(m))\,dm. \tag{24}$$

The equations above mean that if we set m^* to a value m_0 where no treatment moderation occurs, then ATE can be expressed as a function of its marginal effect as in equation (23). The weights of this equation can be further simplified into equation (24) if the lowest value of the moderator $\underline{m}$ renders the treatment ineffective.

B.2 Conditional Parallel Trend does not Identify Average Effects

A common goal of the empirical evaluation is to examine how the moderator affects the effect of the treatment on the outcomes. The natural procedure to assess the impact of the moderator is to compare the treatment effects across the values of the moderator M. The following notation is useful to investigate this comparison:

$$ATT_t(m|m') = E(Y_t(1, m) - Y_t(0, m)|D = 1, M = m'),$$

[36] If we set the moderator value m_0 to zero, than we have $Y_{it}(d, 0) = Y_{it}(0, 0); d \in \{0, 1\}$ for all units $i \in \mathcal{I}$.

which denotes the treatment on the treated when we fix the moderator at the value m conditioning on the units i that share the moderator value of $M_i = m'$. According to the notation of the main paper, we have that $ATT_t(m|m) = ATT_t(m)$.

The Conditional Parallel Trend Assumption A.3 enable us to decompose the difference between the treated on the treated as:

$$
\begin{aligned}
& ATT_t(m|m) - ATT_t(m'|m') && (25)\\
&= E(Y_t(1,m) - Y_t(0,m)|D=1, M=m) - E(Y_t(1,m') - Y_t(0,m')|D=1, M=m') && (26)\\
&= \underbrace{E(Y_t(1,m) - Y_t(0,m)|D=1, M=m) - E(Y_t(1,m') - Y_t(0,m')|D=1, M=m)}_{ATT_t(m|m) - ATT_t(m'|m)} && (27)\\
&+ \underbrace{E(Y_t(1,m') - Y_t(0,m')|D=1, M=m) - E(Y_t(1,m') - Y_t(0,m')|D=1, M=m')}_{ATT_t(m'|m) - ATT_t(m'|m')}. && (28)
\end{aligned}
$$

In summary, we can express the difference of conditional ATT parameters as:

$$
ATT_t(m|m) - ATT_t(m'|m') = \underbrace{ATT_t(m|m) - ATT_t(m'|m)}_{\text{Effect Difference}} + \underbrace{ATT_t(m'|m) - ATT_t(m'|m')}_{\text{Selection Bias on the Moderator}}. \quad (29)
$$

The decomposition above shows that the difference between the treated on the treated effects comprises two terms. The first term is the difference in the treatment effect when we fix the moderator at different levels for the same units i such that $M_i = m$. It accounts for the change in the treatment-on-the-treated effect due to a shift in the moderator.

The second term in (29) is due to selection bias. It accounts is the change in the treatment on the treated effect between two sets of units. The parameter $ATT_t(m'|m)$ denotes the treatment effect when we fix the mediator M to the value $m' \in \mathcal{M}$ for units i that share the moderator value m. The parameter $ATT_t(m'|m)$ also fixes the mediator M to the value $m' \in \mathcal{M}$, however this effect is evaluated for a different set of units i that share the moderator value m'.

The main conclusion of the decomposition (29) is that the Conditional Parallel Trends Assumption A.3 is not sufficiently strong to render a clear causal interpretation of the differences between the treatment on the treated effects.

The Conditional Parallel Trend Assumption A.3 is not sufficient to identify ATE_t or $ATE_t(m)$ in (4) either. To understand this limitation, it is useful to rewrite the conditional average treatment effect $ATE_t(m)$ in terms of the conditional treatment on the treated $ATT_t(m)$:

$$
\begin{aligned}
ATE_t(m|m) &= E(Y_t(1,m) - Y_t(0,m)|M=m) && (30)\\
&= E(Y_t(1,m) - Y_t(0,m)|M=m, D=1)P(D=1|M=m)\\
&\quad + E(Y_t(1,m) - Y_t(0,m)|M=m, D=0)P(D=0|M=m) && (31)\\
&= ATT_t(m|m)P(D=1|M=m)\\
&\quad + E(Y_t(1,m) - Y_t(0,m)|M=m, D=0)P(D=0|M=m). && (32)
\end{aligned}
$$

The Conditional Parallel Trends Assumption A.3 enable us to identify $ATT_t(m|m)$, but not $E(Y_t(1,m) - Y_t(0,m)|M = m, D = 0)$, which is the causal effect the treatment for the control group. Note that the control group never experience the treatment itself. Its identification requires an assumption that enable us to use the treatment group to evaluate the causal effect for the control group.

The lack of identification of average effects presented here is inline with the results in Callaway, Goodman-Bacon, and Sant'Anna (2021), who shows that the common trend assumption is not sufficient to identify causal effects of interest in the DiD design with a continuous treatment.

B.3 Common DiD Regression under Parallel Trend Assumptions

The TWFE regression in (2) is numerically equivalent to the following regression:[37]

$$\Delta Y_{it} = \alpha + \beta_{DiD} \cdot W_i + \epsilon_i, \text{ where } W_i = D_i \cdot M_i. \tag{33}$$

We seek to examines the causal content of the expected value of the OLS estimator for the parameter β_{DiD} in the regression above. To do so, consider the following notation. Let $\overline{M}_d = E(M|D = d); d \in \{1,0\}$ be expected value of the moderator condition on the treatment group. Let $\overline{\Delta Y_t}_d = E(Y_t - Y_{t-1}|D = d); d \in \{1,0\}$ denotes the expected value of the outcome time difference condition on the treatment groups. Finally, let $P_d = P(D = d); d \in \{0,1\}$ denotes the probability of each treatment group. Under this notation, we can state the following theorem:

Theorem T.5. Under standard OLS assumptions, the expected value of the OLS estimator for moderator DiD parameter β_{DiD} in (33) is given by the following:

$$\beta_{DiD} = \frac{\text{Cov}(\Delta Y_t, M|D = 1) + \left(\overline{\Delta Y}_{t1} - \overline{\Delta Y}_{t0}\right) \cdot \overline{M}_1 \cdot P_0}{\text{Var}(M|D = 1) + \overline{M}_1^2 \cdot P_0} \tag{34}$$

Moreover, consider replacing the moderator M by a linear transformation $M^* = M - \overline{M}_1$. Then, under Assumptions A.1 and A.3, the expected value of the OLS estimator for β_{DiD} in (33) is given by the following:

$$\beta^*_{DiD} = \int \frac{\partial E(Y_t(1) - Y_{t-1}(0)|D = 1, M^* = m)}{\partial m} \, \omega(m) \, dm \tag{35}$$

$$\text{where } \omega(m) = \frac{E(M^*|M^* > m, D = 1)\left(1 - F_{M^*|D=1}(m)\right)}{\text{Var}(M|D = 1)}. \tag{36}$$

Proof. See Appendix C.5. □

[37]This is the linear regression of the outcome time-difference ΔY_t on a constant term α and the interaction between the treatment indicator D and the moderator variable M,; that is, $W = D \cdot M$. Standard OLS assumptions are that the observed data $(Y_{it}, Y_{it-1}, D_i, M_i)$ denote random variables that are independent and identically distributed (i.i.d.) across i and ϵ_i is an i.i.d. unobserved mean-zero exogenous error term that is statistically independent of D_i, M_i.

Equation (34) is a statistical result arising from applying the Frisch-Waugh-Lovell Theorem (1933; 1963).[38] Under a linear transformation of the moderator, β_{DiD} in (35) is a weighted average of the time difference of the counterfactual outcomes for the treatment group (33). [39]

The main assessment of Theorem **T.5** is that the estimate for the parameter β_{DiD} in the regressions (2) or (33) cannot be easily described in terms of the marginal effects in (5).

Appendix C Mathematical Proofs

C.0 Proof of Proposition P.1

The proposition requires multiple applications of the formula for the integration by parts. Namely, let $h(x) : \mathbb{R} \to \mathbb{R}$ be an integrable function and $g(x) : \mathbb{R} \to \mathbb{R}$ be an a differentiable function, then the following equation holds:

$$\int_a^b \frac{\partial g(x)}{\partial x} h(x) dx = \Big[g(x)h(x) \Big]_a^b - \int_a^b g(x) \frac{\partial h(x)}{\partial x} dx \tag{37}$$

Note that the moderator M is a continuous random variable in $[\underline{m}, \overline{m}]$, and $ATE_t(m)$ is differentiable. Thereby $ATE_t(m) = E(Y_t(1,m) - Y_t(0,m)|M = m)$ is continuous in $[\underline{m}, \overline{m}]$. Moreover, we have that

$$ATE_t = \int_{\underline{m}}^{\overline{m}} ATE_t(m) f_M(m) dm,$$

where $f_M(m) > 0$ is the probability density of M and $F_M(m) = \int_{\underline{m}}^{m} f_M(m)dm = P(M \leq m)$ denotes the cumulative probability function of M such that $F_M(\underline{m}) = 0$ and $F_M(\overline{m}) = 1$.

Let m^* be any value in $[\underline{m}, \overline{m}]$. We first apply (37) to the integral $\int_{m^*}^{\overline{m}} MATE_t(m) \left(1 - F_M(m)\right) dm$:

[38]This result can also be understood as an ANOVA decomposition that rewrites the OLS coefficient as a weighted average of the intra- and between-groups regression coefficients (see, for instance, Section 4.2 of Yitzhaki (2013)). If $M_i = 1$ for all $i \in \mathcal{I}$, then β_{DiD} in Eq. (34) yields the standard DiD estimator for the TWFE model, $\beta_{DiD} = \overline{\Delta Y_{t1}} - \overline{\Delta Y_{t0}}$.

[39]The weights $\omega(m)$ in (36) are a function of truncated expectation and the CDF of the moderator. These weights are always positive and sum to one.

$$
\begin{aligned}
&\int_{m^*}^{\overline{m}} MATE_t(m)\,(1-F_M(m))\,dm \\
&\quad = \int_{m^*}^{\overline{m}} \frac{\partial ATE_t(m)}{\partial m}\,(1-F_M(m))\,dm \\
&\quad = \Big[ATE_t(m)\,(1-F_M(m))\Big]_{m^*}^{\overline{m}} + \int_{m^*}^{\overline{m}} ATE_t(m) f_M(m) dm \\
&\quad = \Big(ATE_t(\overline{m}|\overline{m})\,(1-F_M(\overline{m}))\Big) - \Big(ATE_t(m^*)\,(1-F_M(m^*))\Big) + \int_{m^*}^{\overline{m}} ATE_t(m) f_M(m) dm, \\
&\quad = -\Big(ATE_t(m^*)\cdot(1-F_M(m^*))\Big) + \int_{m^*}^{\overline{m}} ATE_t(m) f_M(m) dm, \\
&\therefore \int_{m^*}^{\overline{m}} ATE_t(m) f_M(m) dm = \int_{m^*}^{\overline{m}} MATE_t(m)\,(1-F_M(m))\,dm + ATE_t(m^*)\cdot(1-F_M(m^*))\,,
\end{aligned} \tag{38}
$$

where the first equality comes from the definition of $MATE_t(m)$. The second equality applies the integration by parts. The fourth equality is due to the fact that $F_M(\overline{m}) = 1$. The last equality simply rearranges the terms.

Next, we apply (37) to the integral $\int_{\underline{m}}^{m^*} MATE_t(m)\big(-F_M(m)\big)dm$:

$$
\begin{aligned}
&\int_{\underline{m}}^{m^*} MATE_t(m)\big(-F_M(m)\big)dm \\
&\quad = \int_{\underline{m}}^{m^*} \frac{\partial ATE_t(m)}{\partial m}\big(-F_M(m)\big)dm \\
&\quad = \Big[ATE_t(m)\,(-F_M(m))\Big]_{\underline{m}}^{m^*} + \int_{\underline{m}}^{m^*} ATE_t(m) f_M(m) dm \\
&\quad = \Big(ATE_t(m^*)\,(-F_M(m^*))\Big) - \Big(ATE_t(\underline{m}|\underline{m})\,(-F_M(\underline{m}))\Big) + \int_{\underline{m}}^{m^*} ATE_t(m) f_M(m) dm, \\
&\quad = \Big(ATE_t\cdot F_M(m^*)\Big) + \int_{\underline{m}}^{m^*} ATE_t(m) f_M(m) dm, \\
&\therefore \int_{\underline{m}}^{m^*} ATE_t(m) f_M(m) dm = \int_{\underline{m}}^{m^*} MATE_t(m)\big(-F_M(m)\big)dm - ATE_t(m^*)\cdot F_M(m^*),
\end{aligned} \tag{39}
$$

where the first equality comes from the definition of $MATE_t(m)$. The second equality applies the integration by parts. The fourth equality is due to the fact that $F_M(\overline{m}) = 1$. The last equality simply rearranges the terms.

The final expression is obtained by summing equations (38) and (39). The sum of the left-hand side of these two equations give us the average treatment effect:

$$
\int_{\underline{m}}^{m^*} ATE_t(m) f_M(m) dm + \int_{m^*}^{\overline{m}} ATE_t(m) f_M(m) dm = \int_{\underline{m}}^{\overline{m}} ATE_t(m) f_M(m) dm = ATE_t. \tag{40}
$$

The sum of the right-hand side of equations (38) and (39) give us the following expression:

$$\left(\int_{\underline{m}}^{m^*} MATE_t(m)\big(-F_M(m)\big)dm - ATE_t(m^*)\cdot F_M(m^*)\right)$$

$$+\left(\int_{m^*}^{\overline{m}} MATE_t(m)\,(1-F_M(m))\,dm + ATE_t(m^*)\cdot(1-F_M(m^*))\right) \tag{41}$$

$$= \int_{\underline{m}}^{\overline{m}} MATE_t(m)\cdot\Big(\mathbf{1}[m \geq m^*](1-F_M(m)) - \mathbf{1}[m \leq m^*]\cdot F_M(m)\Big)dm + ATE_t(m^*). \tag{42}$$

We can equate the left-hand in (40) with the right-hand side in (42) to obtain the desired expression:

$$ATE_t = \int_{\underline{m}}^{\overline{m}} MATE_t(m)\cdot\Big(\mathbf{1}[m \geq m^*](1-F_M(m)) - \mathbf{1}[m \leq m^*]\cdot F_M(m)\Big)dm + ATE_t(m^*).$$

C.1 Proof of Theorem T.1

The identification of $ATT_t(m)$ in (6) is obtained by the following equations:

$$\begin{aligned}
ATT_t(m) &= E[Y_t(1,m) - Y_t(0,m)|D=1, M=m],\\
&= E[Y_t(1,m) - Y_{t-1}(0,m)|D=1, M=m] - E[Y_t(0,m) - Y_{t-1}(0,m)|D=1, M=m],\\
&= E[Y_t(1,m) - Y_{t-1}(0,m)|D=1, M=m] - E[Y_t(0,m) - Y_{t-1}(0,m)|D=0, M=m],\\
&= E[\Delta Y_t|D=1, M=m] - E[\Delta Y_t|D=0, M=m],
\end{aligned}$$

where the first equality is due to the definition of $ATT_t(m)$ in (3). The second equality adds and subtracts $E[Y_{t-1}(0,m)|D=1, M=m]$. The third equality invokes the Conditional Parallel Trends Assumption **A.3**. The last equality is due to **A.1**. Namely, the expected value of $Y_t(1,m)$ and $Y_{t-1}(0,m)$ are observed when conditioning on $(D=1, M=m)$, and the expected value of $Y_t(0,m)$ and $Y_{t-1}(0,m)$ are observed when conditioning on $D=0, M=m$.

The identification equation for ATT_t in (7) stems from the following equations:

$$\begin{aligned}
ATT_t &= E[Y_t(1) - Y_t(0)|D=1],\\
&= \int_m E[Y_t(1) - Y_t(0)|D=1, M=m]dF_{M|D=1}(m),\\
&= \int_m E[Y_t(1,m) - Y_t(0,m)|D=1, M=m]dF_{M|D=1}(m),\\
&= \int_m E[Y_t(1,m) - Y_t(0,m)|D=1, M=m]\frac{P(D=1|M)}{P(D=1)}dF_M(m),
\end{aligned}$$

where the second equation is due to the law of iterated expectations. The third equation is due to **A.1** and the fourth equation is due to the Bayes theorem.

C.2 Proof of Theorem T.2

The identification of $ATE_t(m)$ in (8) is obtained by the following equations:

$$\begin{aligned}
ATE_t(m) &= E[Y_t(1,m) - Y_t(0,m)|M = m],\\
&= E[Y_t(1,m) - Y_{t-1}(0,m)|M = m] - E[Y_t(0,m) - Y_{t-1}(0,m)|M = m],\\
&= E[Y_t(1,m) - Y_{t-1}(0,m)|D = 1, M = m] - E[Y_t(0,m) - Y_{t-1}(0,m)|D = 0, M = m],\\
&= E[\Delta Y_t|D = 1, M = m] - E[\Delta Y_t|D = 0, M = m],
\end{aligned}$$

where the first equality is due to the definition of $ATE_t(m)$ in (3). The second equality adds and subtracts $E[Y_{t-1}(0,m)|M = m]$. The third equality invokes the Strong Parallel Trends **A.4** and the Full Support **A.2** . The last equality is due to **A.1**. Namely, the expected value of $Y_t(1,m)$ and $Y_{t-1}(0,m)$ are observed when conditioning on $(D = 1, M = m)$, and the expected value of $Y_t(0,m)$ and $Y_{t-1}(0,m)$ are observed when conditioning on $D = 0, M = m$.

The identification equation for ATE_t in (9) stems from the following equations:

$$\begin{aligned}
ATE_t &= E[Y_t(1) - Y_t(0)],\\
&= \int_m E[Y_t(1) - Y_t(0)|M = m]dF_M(m),\\
&= \int_m E[Y_t(1,m) - Y_t(0,m)|D = 1, M = m]dF_M(m),
\end{aligned}$$

where the second equation is due to the law of iterated expectations and the third equation is due to **A.1**.

C.3 Proof of Theorem T.3

The DiD estimator for β_{DiD} in the TWFE regression (10) is numerically equivalent to the estimator obtained from the following regression:[40]

$$\Delta Y_{it} = \alpha + \gamma \cdot D_i + \kappa \cdot M_i + \beta_{DiD} \cdot W_i + \upsilon_i, \text{ such that } W_i = D_i \cdot M_i, \tag{43}$$

where $\Delta Y_{it} = Y_{it} - Y_{it-1}$ is the temporal outcome difference for unit i.

The first sampling weighting scheme of the theorem is uniform, which means that the regression employs the actual distribution of the data. Equation (11) is a standard result in the OLS literature. By using the full set of indicator interaction, the DiD estimator evaluates the difference of the OLS estimators if we were to regress two separate regressions, one for the control (untreated) group and another for the treatment group.

Equations (12)–(13) are based on the Yitzhaki's Weights (Yitzhaki 2013), which states that the covariance of any random variables Y, X such that $E(|Y|) < \infty$ and $E(|X|) = \mu_X < \infty$

[40] The following expression denotes the regression of the outcome time-difference ΔY_t on a constant term α, the treatment indicator D, the moderator M, and their interaction $W = D \cdot M$.

and $E(Y|X)$ is differentiable, can be expressed as:

$$\text{Cov}(Y, X) = \int_{-\infty}^{\infty} \frac{\partial E(Y|X = x)}{\partial x} \omega(x) dx,$$
$$\text{such that } \omega(x) = E(X - \mu_X | X > x)(1 - F_X(x)).$$

According to the equation above, we can express the covariances $\text{Cov}(\Delta Y_t, M|D = d); d \in \{0, 1\}$ by the following expression:

$$\text{Cov}(\Delta Y_t, M|D = d) = \int_{-\infty}^{\infty} \frac{\partial E(\Delta Y_t|D = d, M = m)}{\partial m} \omega(m) dm; \quad d \in \{0, 1\}, \tag{44}$$
$$\text{such that } \omega(m) = E(M - \overline{M}_d | M > m, D = d)(1 - F_{M|D=d}(m)),$$

where $\overline{M}_d \equiv E(M|D = d); d \in \{0, 1\}$.

The second weighting scheme sets the distribution of the moderator of the treatment and control group to the distribution of the treatment group. The DiD parameter of the regression still delivers the difference of two separate OLS regressions that evaluate the covariance between ΔY_t and M over the variance of M for each treatment group. The weighting scheme modifies the distribution of M. The first OLS parameter β_1 is associated with the treatment group ($D = 1$) and the asymptotic cumulative distribution of M is given by $F_{M|D=1}(m)$. The expected value of this OLS estimator is given by:

$$E(\beta_1) = \int_{-\infty}^{\infty} \frac{\partial E(\Delta Y_t|D = 1, M = m)}{\partial m} \omega_1(m) dm \tag{45}$$
$$\text{where } \omega_1(m) = \frac{E(M - E(M|D = 1)|M > m, D = 1)(1 - F_{M|D=1}(m))}{\text{Var}(M|D = 1)}. \tag{46}$$

The second OLS parameter β_0 is associated with the control group ($D = 0$) and the cumulative distribution of M is also given by $F_{M|D=1}(m)$. The expected value of this OLS estimator is given by:

$$E(\beta_0) = \int_{-\infty}^{\infty} \frac{\partial E(\Delta Y_t|D = 0, M = m)}{\partial m} \omega_1(m) dm, \tag{47}$$

where $\omega_1(m)$ is the same as in (46). The difference between the expected value of the OLS estimators in (53) and (47) is:

$$E(\beta_1) - E(\beta_0) = \int_{-\infty}^{\infty} \frac{\partial \left(E(\Delta Y_t|D = 1, M = m) - E(\Delta Y_t|D = 0, M = m)\right)}{\partial m} \omega_1(m) dm \tag{48}$$
$$= \int_{-\infty}^{\infty} MATT_t(m) \omega_1(m) dm, \tag{49}$$

where the second equality is due to **T.1**.

The last weighting scheme sets the conditional distribution of the moderator of the treatment and control groups to the unconditional distribution of the moderator. The DiD parameter of the regression also delivers the difference of two separate OLS regressions that evaluate the covariance between ΔY_t and M over the variance of M for each treatment group. However, the weighting scheme modifies the distribution of M. The first OLS parameter β_1^* is associated with the treatment group ($D = 1$) and the asymptotic cumulative distribution of M is given by $F_M(m)$. The expected value of this OLS estimator is given by:

$$E(\beta_1^*) = \int_{-\infty}^{\infty} \frac{\partial E(\Delta Y_t|D=1, M=m)}{\partial m} \omega^*(m) dm \tag{50}$$

$$\text{where } \omega^*(m) = \frac{E(M - E(M|D=1)|M > m, D=1)(1 - F_{M|D=1}(m))}{\text{Var}(M|D=1)}. \tag{51}$$

The second OLS parameter β_0^* is associated with the control group ($D = 0$) and the cumulative distribution of M is also given by $F_M(m)$. The expected value of this OLS estimator is given by:

$$E(\beta_0^*) = \int_{-\infty}^{\infty} \frac{\partial E(\Delta Y_t|D=0, M=m)}{\partial m} \omega^*(m) dm, \tag{52}$$

where $\omega^*(m)$ is the same as in (51). The difference between the expected value of the two OLS estimators in (50) and (52) is:

$$E(\beta_1^*) - E(\beta_0^*) = \int_{-\infty}^{\infty} \frac{\partial \left(E(\Delta Y_t|D=1, M=m) - E(\Delta Y_t|D=0, M=m)\right)}{\partial m} \omega^*(m) dm \tag{53}$$

$$= \int_{-\infty}^{\infty} MATE_t(m) \omega^*(m) dm, \tag{54}$$

where the second equality is due to **T.2**.

C.4 Proof of Theorem T.4

The OLS estimator of the linear regression in (14) is numerically equivalent to the estimator of the regression (15), that is,

$$\Delta Y_{it} = \alpha + \beta_{DiD} \cdot M_i + (\epsilon_{it} - \epsilon_{it-1}). \tag{55}$$

It is useful to rewrite the dependent variable ΔY_{it} in the following manner:

$$\begin{aligned}
\Delta Y_{it} &\equiv Y_{it} - Y_{it-1} && (56)\\
&= Y_{it}(1) - Y_{it-1}(0) && (57)\\
&= \big(Y_{it}(1) - Y_{it}(0)\big) + \big(Y_{it}(0) - Y_{it-1}(0)\big) && (58)\\
&= \big(Y_{it}(1) - Y_{it}(0)\big) + \big((\tau_t - \tau_{t-1}) + (v_{it} - v_{it-1})\big) && (59)
\end{aligned}$$

Equation (56) simply uses the definition that ΔY_{it} is the outcome time difference. Equation (57) uses Assumption **A.1** and the fact that all units are treated, $D = 1$, thus, in period $t-1$, none of the units are treated, while in period t, all units are treated. Equation (58) adds and subtracts the term $Y_{it}(0)$. Equation (59) uses the assumption that the counterfactual outcome for the untreated units is given by $Y_{it}(0) = \kappa_i + \tau_t + f_0(M_i) + v_{it}$ for t and $t-1$. Thus, $Y_{it}(0) - Y_{it-1}(0) = (\tau_t - \tau_{t-1}) + (v_{it} - v_{it-1})$ as stated in (59).

The expected value of β_{DiD}-estimator is given by:

$$\begin{aligned}
\beta_{DiD} &= \frac{Cov\Big(\Delta Y_t, M\Big)}{Var(M)}, && (60)\\
&= \frac{Cov\Big(\big(Y_{it}(1) - Y_{it}(0)\big) + \big((\tau_t - \tau_{t-1}) + (v_{it} - v_{it-1}), M\big)\Big)}{Var(M)}, && (61)\\
&= \frac{Cov\Big(Y_{it}(1) - Y_{it}(0), M\Big)}{Var(M)}. && (62)
\end{aligned}$$

Equation (60) is due to independence the independence between error terms ϵ and M. Equation (61) replaces ΔY_t by the expression in (59). Equation (62) is due to the independence of employs the $(v_t - v_{t-1})$ and M and because $(\tau_t - \tau_{t-1})$ is a constant term.

We can now apply Yitzhaki's Weights (Yitzhaki 2013), who shows that the covariance of any random variables Y, X such that $E(|Y|) < \infty$ and $E(|X|) = \mu_X < \infty$ and $E(Y|X)$ is differentiable, can be expressed as:

$$\frac{\text{Cov}(Y, X)}{\text{Var}(X)} = \int_{-\infty}^{\infty} \frac{\partial E(Y|X = x)}{\partial x} \omega(x) dx, \tag{63}$$

$$\text{such that } \omega(x) = \frac{E(X - E(X)|X > x)(1 - F_X(x))}{\text{Var}(X)} \tag{64}$$

where the weighting function $\omega(x)$ is positive and integrate to one. Under the Full Support Assumption **A.2**, we can apply equation (63)–(64) to equation (62) in order to obtain the following expression:

$$\beta_{DiD} = \int \frac{\partial E(Y_t(1) - Y_t(0)|M = m, D = 1)}{\partial m} \frac{E(M - E(M))|M > m, B = 1)\left(1 - F_{M|D=1}(m)\right)}{\text{Var}(M|D = 1)} dm \quad (65)$$

$$= \int MATT_t(m) \frac{E(M - E(M))|M > m, D = 1)\left(1 - F_{M|D=1}(m)\right)}{\text{Var}(M|D = 1)} dm. \quad (66)$$

Equation (65) simply applies the Yitzhaki's Weights, while (66) uses the definition of $MATT_t$. The specification is conditioned on $D = 1$ because all agents belong to the treated group. This proof did not explicitly invoke the Conditional Parallel Trends (**A.3**) since the condition is implied by the linear equation that defines the counterfactual outcomes for the untreated.

The second part of the theorem assumes that the observed distribution of the moderator, $P(M = m|D = 1)$, is equal to the unconditional distribution $P(M = m)$. Moreover, the Strong Parallel Trend enable us to equate $MATT_t(m) = MATE_t(m)$. These two features enable us to express β_{DiD} in (66) as:

$$\beta_{DiD} = \int MATT_t(m) \frac{E(M - E(M))|M > m, D = 1)\left(1 - F_{M|D=1}(m)\right)}{\text{Var}(M|D = 1)} dm, \quad (67)$$

$$= \int MATE_t(m) \frac{E(M - E(M))|M > m)(1 - F_M(m))}{\text{Var}(M)} dm. \quad (68)$$

C.5 Proof of Theorem T.5

This proof adopts a short-hand notation. Let $\overline{M}_d = E(M|D = d); d =\in \{1, 0\}$ denotes the expected value of the moderator condition on the treatment group; Let $\overline{\Delta Y}_d = E(Y_t - Y_{t-1}|D = d); d =\in \{1, 0\}$ denotes the expected value of the outcome time difference condition on the treatment groups; Let $P_d = P(D = d); d \in \{0, 1\}$ denotes the probability of each treatment group; and $\overline{\Delta Y} = E(\Delta Y) = \overline{\Delta Y}_1 P_1 + \overline{\Delta Y}_0 P_0$.

The expected value of the OLS estimator of the parameter β_{DiD} in (33) evaluates the following ratio:

$$\beta_{DiD} + \frac{\text{Cov}(\Delta Y_t, D \cdot M)}{\text{Var}(D \cdot M)} \tag{69}$$

We can express the numerator of (69) as:

$$\begin{aligned}
\text{Cov}(\Delta Y_t, D \cdot M) &= E((\Delta Y_t - \overline{\Delta Y}) \cdot M|D = 1)P_1 &(70)\\
&= E(\Delta Y_t \cdot M|D = 1)P_1 - \overline{\Delta Y} \cdot \overline{M}_1 P_1 &(71)\\
&= E(\Delta Y_t \cdot M|D = 1)P_1 - (\overline{\Delta Y}_1 P_1 + \overline{\Delta Y}_0 P_0)\overline{M}_1 P_1 &(72)\\
&= (E(\Delta Y_t \cdot M|D = 1) - \overline{\Delta Y}_1 P_1 \overline{M}_1 + \overline{\Delta Y}_0 P_0 \overline{M}_1)P_1 &(73)\\
&= (E(\Delta Y_t \cdot M|D = 1) - \overline{\Delta Y}_1 \overline{M}_1 + \overline{\Delta Y}_1 \overline{M}_1(1 - P_1) - \overline{\Delta Y}_0 P_0 \overline{M}_1)P_1 &(74)\\
&= (\text{Cov}(\Delta Y_t, M|D = 1) + \overline{\Delta Y}_1 \overline{M}_1 P_0 - \overline{\Delta Y}_0 P_0 \overline{M}_1)P_1 &(75)\\
&= \text{Cov}(\Delta Y_t, M|D = 1)P_1 + (\overline{\Delta Y}_1 - \overline{\Delta Y}_0)P_0 \overline{M}_1 P_1 &(76)\\
&= (\text{Cov}(\Delta Y_t, M|D = 1) + (\overline{\Delta Y}_1 - \overline{\Delta Y}_0)P_0 \overline{M}_1)P_1 &(77)
\end{aligned}$$

We can express the denominator of (69) as:

$$\begin{aligned}
\text{Var}(D \cdot M) &= E((M \cdot D - E(M \cdot D)) \cdot (M \cdot D)) &(78)\\
&= E((M \cdot D - \overline{M}_1 P_1) \cdot (M \cdot D)) &(79)\\
&= E((M - \overline{M}_1 P_1) \cdot M|D = 1)P_1 &(80)\\
&= (E(M^2|D = 1) - \overline{M}_1^2 P_1) \cdot P_1 &(81)\\
&= (E(M^2|D = 1) - \overline{M}_1^2 P_1 - \overline{M}_1^2 P_0 + \overline{M}_1^2 P_0) \cdot P_1 &(82)\\
&= ((E(M^2|D = 1) - \overline{M}_1^2) + \overline{M}_1^2 P_0) \cdot P_1 &(83)\\
&= (\text{Var}(M|D = 1) + \overline{M}_1^2 P_0) \cdot P_1 &(84)
\end{aligned}$$

The ratio of (77) and (84) generates the following equation:

$$\frac{\text{Cov}(\Delta Y_t, D \cdot M)}{\text{Var}(D \cdot M)} = \quad = \frac{\text{Cov}(\Delta Y_t, M|D = 1) + (\overline{\Delta Y}_1 - \overline{\Delta Y}_0)P_0 \overline{M}_1}{\text{Var}(M|D = 1) + \overline{M}_1^2 P_0} \tag{85}$$

If we set $\overline{M}_1 = 0$, then we have that:

$$\frac{\text{Cov}(\Delta Y_t, D \cdot M)}{\text{Var}(D \cdot M)} = \quad = \frac{\text{Cov}(\Delta Y_t, M|D = 1)}{\text{Var}(M|D = 1)} \tag{86}$$

The next part of the theorem employs the Yitzhaki's Weights (Yitzhaki 2013). Using integration by parts, it is easy to show that the covariance of any random variables Y, X

such that $E(|Y|) < \infty$ and $E(|X|) = \mu_X < \infty$ and $E(Y|X)$ is differentiable, can be expressed as:

$$\text{Cov}(Y, X) = \int_{-\infty}^{\infty} \frac{\partial E(Y|X = x)}{\partial x} E(X - \mu_X | X > x)(1 - F_X(x))dx, \tag{87}$$

Moreover, we can apply (87) to express the variance of a random variable X as:

$$\text{Var}(X) \equiv \text{Cov}(X, X) = \int_{-\infty}^{\infty} E(X - \mu_X | X > x)(1 - F_X(x))dx. \tag{88}$$

Setting $\overline{M}_1 \equiv E(M|D = 1) = 0$, and applying the formula (87) to the OLS estimator in (85), we obtain:

$$\begin{aligned}
&\frac{\text{Cov}(\Delta Y_t, D \cdot M)}{\text{Var}(D \cdot M)} = \\
&= \int \frac{\partial E(\Delta Y_t | D = 1, M = m)}{\partial m} \frac{E(M|M > m, D = 1)\left(1 - F_{M|D=1}(m)\right)}{\text{Var}(M|D = 1)} dm \\
&= \int \frac{\partial E(Y_t(1) - Y_{t-1}(0)|D = 1, M = m)}{\partial m} \frac{E(M|M > m, D = 1)\left(1 - F_{M|D=1}(m)\right)}{\text{Var}(M|D = 1)} dm
\end{aligned}$$

Equation (88) and the feature that $\overline{M}_1 = 0$ assures that the weights in the equation above are always positive and integrate to one.

Appendix D Additional Tables

Table A1: Impact of PNTR on Adolescent Physical Health, Cognition and School Dropout Rates

	(1)	(2)	(3)
Panel A. Physical health index			
Post × NTR gap	0.030	0.065	0.066
	(0.065)	(0.077)	(0.076)
Observations	5976	5976	5976
Control mean	-0.00	-0.00	-0.00
Panel B. Cognitive function index			
Post × NTR gap	0.023	0.077	0.072
	(0.065)	(0.062)	(0.057)
Observations	4892	4892	4892
Control mean	0.19	0.19	0.19
Panel C. School dropout rate			
Post × NTR gap	-0.026	-0.017	-0.015
	(0.030)	(0.033)	(0.033)
Observations	5977	5977	5977
Control mean	0.26	0.26	0.26
Prefecture-of-birth fixed effects	Yes	Yes	Yes
Year-of-birth fixed effects	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes
Post × 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 mental health outcomes on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions in column 1 control for prefecture of birth fixed effects, year of birth fix effects, prefecture-specific linear trend in year of birth, and the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity. Regressions in column 2 further control for the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate. Regressions in column 3 further control for individual characteristics including age, gender, father's and mother's age, and indicator variables for whether the mother and father completed middle school. Standard errors are clustered at the prefecture of birth level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A2: Robustness Checks: Alternative Measures of the NTR Gap

	Any depression (1)	Severe depression (2)
Panel A. NTR gap measured by excluding industries with the highest NTR gap		
Post × NTR gap	0.003	-0.043***
	(0.030)	(0.016)
Observations	14521	14521
Control mean	0.28	0.09
Panel B. NTR gap measured by excluding industries with the lowest NTR gap		
Post × NTR gap	0.002	-0.044**
	(0.037)	(0.021)
Observations	14521	14521
Control mean	0.28	0.09
Panel C. NTR gap winsorized at the 5/95 percentiles		
Post × NTR gap	0.002	-0.047**
	(0.035)	(0.019)
Observations	14521	14521
Control mean	0.28	0.09
Panel D. NTR gap measured by excluding nontradable industries		
Post × NTR gap	0.021	-0.096**
	(0.075)	(0.043)
Observations	14521	14521
Control mean	0.28	0.09
Prefecture-of-birth fixed effects	Yes	Yes
Year-of-birth fixed effects	Yes	Yes
Prefecture-specific linear trend	Yes	Yes
Post × Other trade policies	Yes	Yes
Post × Initial prefecture characteristics	Yes	Yes
Individual characteristics	Yes	Yes

Notes: Data are from the 2016–2018 CFPS. This table reports results of the DiD regressions of mental health outcomes on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions control for prefecture of birth fixed effects, year of birth fix effects, prefecture-specific linear trend in year of birth, and the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity, the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate, and individual characteristics including age, gender, father's and mother's age, and indicator variables for whether the mother and father completed middle school. The knitwear industry has the highest NTR gap value 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. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A3: Robustness Checks: Alternative Specifications

	Any depression	Severe depression
	(1)	(2)
Panel A. Regression weighted by the 1990 prefecture population		
Post × NTR gap	-0.020	-0.050**
	(0.031)	(0.020)
Observations	5978	5978
Control mean	0.17	0.07
Prefecture-of-birth fixed effects	Yes	Yes
Year-of-birth fixed effects	Yes	Yes
Prefecture-specific linear trend	Yes	Yes
Post × Other trade policies	Yes	Yes
Post × Initial prefecture characteristics	Yes	Yes
Individual characteristics	Yes	Yes
Panel B. Using year of birth fixed effects interacted with controls		
Post × NTR gap	-0.011	-0.056***
	(0.033)	(0.019)
Observations	14521	14521
Control mean	0.28	0.09
Prefecture-of-birth fixed effects	Yes	Yes
Year-of-birth fixed effects	Yes	Yes
Prefecture-specific linear trend	Yes	Yes
Year-of-birth fixed effects × Other trade policies	Yes	Yes
Year-of-birth fixed effects × Initial prefecture characteristics	Yes	Yes
Individual characteristics	Yes	Yes

Notes: Data are from the 2016–2018 CFPS. This table reports results of the DiD regressions of mental health outcomes on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions control for prefecture of birth fixed effects, year of birth fix effects, prefecture-specific linear trend in year of birth, and the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity, the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate, and individual characteristics including age, gender, father's and mother's age, and indicator variables for whether the mother and father completed middle school. Regressions in Panel A are weighted by the 1990 prefecture population. Regressions in Panel B use year of birth fixed effects interacted with other trade policies and initial prefecture characteristics. Standard errors are clustered at the prefecture of birth level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A4: Heterogeneous Effects of PNTR on Adolescent Mental Health Outcomes

	Any depression (1)	Severe depression (2)
Panel A. Interact with "Female"		
Post × NTR gap	0.032	-0.019
	(0.045)	(0.028)
Post × NTR gap × Interaction	-0.072	-0.053
	(0.049)	(0.040)
Observations	14521	14521
Control mean	0.28	0.09
Panel B. Interact with "Mother completed middle school"		
Post × NTR gap	0.062	-0.040*
	(0.055)	(0.022)
Post × NTR gap × Interaction	-0.070	-0.000
	(0.067)	(0.039)
Observations	14520	14520
Control mean	0.28	0.09
Panel C. Interact with "Parental absence for at least one week from ages 0-3"		
Post × NTR gap	0.004	-0.031
	(0.030)	(0.021)
Post × NTR gap × Interaction	-0.000	-0.098
	(0.182)	(0.103)
Observations	11245	11245
Control mean	0.27	0.09
Panel D: Interact with "Above the median initial share of the rural population"		
Post × NTR gap	0.036	-0.042**
	(0.042)	(0.020)
Post × NTR gap × Interaction	-0.028	0.065
	(0.163)	(0.110)
Observations	14521	14521
Control mean	0.28	0.09
Prefecture-of-birth fixed effects	Yes	Yes
Year-of-birth fixed effects	Yes	Yes
Prefecture-specific linear trend	Yes	Yes
Post × Other trade policies	Yes	Yes
Post × Initial prefecture characteristics	Yes	Yes
Individual characteristics	Yes	Yes

Notes: Data are from the 2016–2018 CFPS. This table reports results of the DiD regressions of mental health outcomes 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 in Panel D. All regressions control for prefecture of birth fixed effects, year of birth fixed effects, prefecture-specific linear trend in year of birth, and the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity, the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate, and individual characteristics including age, gender, father's and mother's age, and indicator variables for whether the mother and father completed middle school. The regressions also control for the triple interactions of those terms with a heterogeneous group indicator. Standard errors are clustered at the prefecture of birth level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A5: Impact of PNTR on Individual Migration Experience

	(1)	(2)	(3)
Panel A. Cross-prefecture migration since birth			
Post × NTR gap	0.006	0.005	0.005
	(0.011)	(0.011)	(0.011)
Observations	13100	13100	13100
Control mean	0.02	0.02	0.02
Panel B. Cross-prefecture migration since age 12			
Post × NTR gap	0.008	0.004	0.003
	(0.011)	(0.011)	(0.011)
Observations	9043	9043	9043
Control mean	0.02	0.02	0.02
Panel C. Rural-urban migration since age 12			
Post × NTR gap	0.021	0.022	0.024
	(0.025)	(0.026)	(0.026)
Observations	11697	11697	11697
Control mean	0.10	0.10	0.10
Prefecture-of-birth fixed effects	Yes	Yes	Yes
Year-of-birth fixed effects	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes
Post × 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 migration indicators on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Regressions in column 1 control for prefecture of birth fixed effects, year of birth fix effects, prefecture-specific linear trend in year of birth, and the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity. Regressions in column 2 further control for the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate. Regressions in column 3 further control for individual characteristics including age, gender, father's and mother's age, and indicator variables for whether the mother and father completed middle school. Standard errors are clustered at the prefecture of birth level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A6: Impact of PNTR on Immigration and Emigration Rate

	Total population (1)	Male (2)	Female (3)
Panel A. Immigration rate in destination prefecture			
Post × NTR gap	0.070	-0.001	0.004
	(0.044)	(0.007)	(0.003)
Observations	1312	1312	1312
Control mean	0.05	0.02	0.01
Panel B. Emigration rate from origin prefecture			
Post × NTR gap	0.023	0.014	0.009
	(0.019)	(0.011)	(0.008)
Observations	1312	1312	1312
Control mean	0.04	0.02	0.02
Prefecture fixed effects	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes
Post × Initial prefecture characteristics	Yes	Yes	Yes

Notes: Data are from the 2000 (the earliest census year where migration data are available), 2005, 2010, and 2015 population censuses in China. This table reports results of the DiD regressions of migration outcomes (immigration and emigration rates) on the interaction of the prefecture-level NTR gap and a post-PNTR indicator. Migration is defined as migrants aged 20-45 who moved across prefectures to seek jobs. The immigration rate is measured as the ratio of migrants who arrived in a given destination prefecture to the total number of non-migrant residents in that prefecture. The emigration rate is measured as the share of migrants who left a given prefecture to the total number of residents in that prefecture. All regressions control for prefecture fixed effects, survey year fixed effects, prefecture-specific linear trend, the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity, and the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate. Standard errors are clustered at the prefecture level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A7: 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	-0.024	-0.007	-0.006
	(0.029)	(0.033)	(0.032)
Observations	11253	11253	11253
Control mean	0.10	0.10	0.10
Prefecture-of-birth fixed effects	Yes	Yes	Yes
Year-of-birth fixed effects	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes
Post × 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	0.044	0.021	0.021
	(0.040)	(0.044)	(0.045)
Observations	3446	3446	3446
Control mean	0.01	0.01	0.01
Panel B2. Father			
Post × NTR gap	0.000	0.029	0.024
	(0.063)	(0.079)	(0.079)
Observations	3446	3446	3446
Control mean	0.03	0.03	0.03
Prefecture fixed effects	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes
Post × 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 1993-2015 CHNS. Regression in column 1 controls for prefecture of birth fixed effects (prefecture fixed effects in Panel B), year of birth fixed effects (survey year fixed effects in Panel B), prefecture-specific linear time trend, and the post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity. Regression in column 2 further controls for the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate. Regression in column 3 further controls for individual characteristics including age, gender, father's and mother's age, and indicator variables for whether the mother and father completed middle school. Standard errors are clustered at the prefecture of birth level in Panel A and are clustered at the prefecture level in Panel B. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A8: Impact of PNTR on Fertility Outcomes

	Births per 1,000 women (1)	Number of children (2)	Percent of women with children (3)
Post × NTR gap	2.761	-613.377	-0.011
	(2.054)	(381.649)	(0.010)
Observations	1640	1640	1640
Control mean	46.71	6799.82	0.82
Prefecture fixed effects	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes
Prefecture-specific linear trend	Yes	Yes	Yes
Post × Other trade policies	Yes	Yes	Yes
Post × Initial prefecture characteristics	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 fertility outcomes 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 post-PNTR indicator interacted with other trade policies including China's output, input and external tariffs, NTR rates, MFA quotas, and contract intensity. The regressions also control for the post-PNTR indicator interacted with initial prefecture characteristics including GDP per capita, average population age, average population years of schooling, total number of children, and fertility rate. Standard errors are clustered at the prefecture level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.