The Economic Impact of Depression Treatment in India: Evidence from Community-Based Provision of Pharmacotherapy

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Abstract

This study evaluates the impact of depression treatment on economic behavior in Karnataka, India. We cross-randomize pharmacotherapy and livelihoods assistance among 1000 depressed adults and evaluate impacts on depression severity, socioeconomic outcomes, and several potential pathways. When combined, the interventions reduce depression severity, with benefits that persist after treatment concludes. Pharmacotherapy alone has a weaker effect that is only marginally significant and dissipates sooner. Depression treatment does not significantly increase earnings, consumption, or human capital investment in children.

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Depression is a pervasive and costly illness with a lifetime prevalence of 15-20 percent (Moussavi et al. 2007, Ferrari et al. 2013, Hasin et al. 2018). It is the fourth largest contributor to the global burden of disease and the top contributor to global disability (WHO 2017). Depression symptoms include anhedonia (the inability to feel pleasure), pessimism, and disrupted sleep and nutrition. These symptoms may lower productivity (Beck et al. 2011), reduce the willingness or ability to invest in child human capital (Cummings and Davies 1994), and affect participation in household decisions (Baranov et al. 2020), thereby impacting socioeconomic outcomes throughout the household. By addressing these symptoms, depression treatment may have health benefits and improve socioeconomic outcomes.

For developing countries, it is particularly important to understand the economic impact of depression and find effective and scalable treatments. Depression is more prevalent among the poor and may contribute to poverty and poverty traps (Ridley et al. 2020, Kessler and Bromet 2013, Haushofer and Fehr 2014). Despite a high need for treatment, the supply of mental health care is constrained by a shortage of providers in low-income countries (Saxena et al. 2007). Stigma and the lack of awareness of mental illness also limit the demand for treatment.

Pharmacotherapy may be a useful tool to treat depression in developing countries. Clinical studies demonstrate the effectiveness of this approach in industrialized countries (Gartlehner et al. 2017). However, we lack evidence of the feasibility and effectiveness of community-based pharmacotherapy in developing countries, as well as evidence of the long-term effects of a single course of pharmacotherapy in general. It is also unclear how mental health care may affect outcomes such as time use, earnings, and investment, and the pathways through which these effects may occur.

This paper studies the effects of pharmacotherapy on depression, socioeconomic outcomes, and possible pathways that may link mental health and economic behavior. We implemented a community-based cluster-randomized trial that cross-randomized Psychiatric Care (PC) and Livelihoods Assistance (LA) among 1000 adults (86 percent of whom were female) with symptoms of mild or moderate depression in a peri-urban region near Bangalore, India. PC and LA are two commonly available services to treat depression both in this setting and elsewhere. Some mental health care providers believe that livelihoods assistance may increase the effectiveness of pharmacotherapy.

The PC intervention provided eight months of personalized pharmacotherapy with the diagnosis and oversight of a psychiatrist from a local research hospital. The LA intervention consisted of two group meetings to address work-related challenges, followed by personalized support to help participants find employment or other income-generating opportunities. By partnering with a local NGO that offers these programs to people with mental illness, we were able to provide both interventions using the existing local infrastructure. Use of local resources is an important factor that facilitates scale-up (Zamboni et al. 2019).

We measured impacts on the mental health, time use, and earnings of participants, human capital investment in children, and consumption, durable goods ownership, and hygiene/sanitation of households, as well as several potential pathways that could link depression to these outcomes. Forty-five percent of participants complied with PC and sixty-eight percent complied with LA. This level of participation suggests that it is possible to surmount barriers to mental health treatment such as stigma and a lack of awareness. We assessed impacts while the PC intervention was ongoing (our "during" period) and 16-26 months after it began (our "after" period). The follow-up data allow us to measure the longer-term effects of a single course of pharmacotherapy on mental health and other outcomes. At present, these effects are largely unknown.

We find that offering pharmacotherapy reduces depression severity when paired with livelihoods, and that this mental health impact persists after treatment concludes. The effect of PC/LA is -0.26 SD (95% CI: -0.41 to -0.10) during the PC intervention and -0.24 SD (95% CI: -0.41 to -0.07) afterward. These effects correspond to a 15.0 percentage point (95% CI: 7.4 to 22.5) decrease in the frequency of moderate or severe depression during the PC intervention and a 7.8 percentage point (95% CI: -0.002 to 15.8) decrease in this frequency afterward. When pharmacotherapy is offered without livelihoods, the effect on depression symptoms is weaker and dissipates sooner. The effect of PC alone is -0.14 SD (95% CI: -0.30 to 0.03) while PC is ongoing, which is only significant at the 10 percent level, and -0.04 SD (95% CI: -0.19 to 0.12) after PC concludes. These effects correspond to a 7.1 percentage point (95% CI: -0.006 to 14.7) decrease in the frequency of moderate or severe depression during the PC intervention and 0.3 percentage point (95% CI: -6.9 to

7.5) decrease in this frequency afterward.[1,2] Bundling LA with PC is a cost-effective way to reduce depression symptoms because adding LA to PC only increases the intervention cost by 5 percent.

Pharmacotherapy does not increase work time or earnings. In fact, PC *reduces* work time by 5.4 hours per week (95% CI: 2.1 to 8.7) during the PC intervention, but this effect dissipates afterward. By contrast, PC/LA does not reduce work time during the PC intervention.[3] Household consumption follows a similar pattern: PC significantly reduces consumption during the intervention but PC/LA does not. The differences between these effects of PC and PC/LA are statistically significant during the PC intervention but not afterward. Therefore, bundling LA with PC has the additional benefit of protecting against some temporary negative effects of PC. None of the interventions has a statistically significant effect on earnings, hygiene/sanitation, or durable goods ownership.

In contrast to Baranov et al. (2020), depression treatment does not have a statistically significant impact on child human capital investment overall. School holidays and the timing of decisions about enrollment may limit the scope for adjustments to human capital investment in the "during" period. However, overall effects are also statistically insignificant after the PC intervention: the effect of PC/LA is 0.12 SD (95%CI: -0.13 to 0.37) and the effect of PC is 0.18 SD (95% CI: -0.01 to 0.38). Despite the lack of an effect overall, we find some evidence of positive effects on human capital investment among older children. Among children who are older than 12 (the age of transition to secondary school), PC increases investment by 0.44 SD (95% CI: 0.12 to 0.75) and PC/LA increases investment by 0.40 SD (95% CI: -0.06 to 0.86), which is significant at the 10 percent level. The effects in the two arms are not statistically different from each other, although only one is statistically significant at conventional levels. Effect sizes among older children are comparable to the impact of conditional

[1] We report average intent to treat effects and provide lower and upper bounds based on 95 percent confidence intervals in parentheses throughout the paper.

[2] The impact of LA on depression severity is -0.08 SD (95% CI: -0.25 to 0.09) during the intervention and 0.01 SD (95% CI: -0.15 to 0.16) afterward. The "during" period estimate corresponds to a 6.9 percentage point (95% CI: -0.01 to 14.8) decline in the probability of moderate or severe depression, which is significant at the 10% level.

[3] Despite the intention behind the program, LA alone does not have impacts on productive time. The lack of an impact suggests that the LA intervention alone is not sufficient to overcome the barriers to work among study participants.

cash transfers (Baird et al. 2014), as well as other initiatives to increase student enrollment and attendance (Evans and Yuan 2020). While this evidence is not strong, this comparison suggests that living with a depressed adult may create an important demand-side barrier to child human capital accumulation.[4]

Next, we consider several possible pathways through which depression treatment may affect behavior. We do not find evidence that depression treatment improves subjective wellbeing, cognition, or participation in household decisions. We find suggestive evidence that pharmacotherapy decreases risk tolerance, which is consistent with a "preferences" pathway and might contribute to an effect of depression on investment, as we discuss later.[5]

Our interpretation of these findings is that the combination of community-based pharmacotherapy and other light-touch interventions may help alleviate depression symptoms in low-income countries. This approach may be especially useful in settings where psychologists and counselors are scarce. An important next step is to understand why livelihoods assistance (which does not directly improve labor market outcomes) enhances the effectiveness of pharmacotherapy. The findings also indicate that pharmacotherapy *per se* does not lead to short-term poverty reduction. However, at the same time, there is suggestive evidence of a causal link between depression treatment and older children's human capital investment. Since fostering human capital accumulation is an important avenue to increase the future wellbeing of children and limit the intergenerational transmission of poverty, further research should continue to investigate this link.

This paper advances several areas of research. We contribute to research on the effectiveness of pharmacotherapy in three ways. First, we establish that a community-based pharmacotherapy intervention in a developing country is feasible and effective at reducing symptoms of depression. Therefore, pharmacotherapy may be an additional tool to address

[4]LA also increases child human capital investment for older children in the "after" period. This effect could arise through a small effect of LA on mental health or through an effect of the interventions on human capital investment via other channels.

[5]We create a risk intolerance index using items from the DOSPERT scale (Blais and Weber 2006), a generalized risk self-assessment (Dohmen et al. 2011), and an incentivized lottery game (Eckel and Grossman 2008). We also compute a negative shocks scale from socioeconomic shocks experienced in the previous four months (Holmes and Rahe 1967). After the PC intervention, PC/LA increases risk intolerance by 0.18 SD (95% CI: -0.05 to 0.41) and reduces the incidence of negative shocks by 0.14 SD (95% CI: -0.31 to 0.02), while PC increases risk intolerance by 0.24 SD (95% CI: 0.01 to 0.47) and reduces the incidence of negative shocks by 0.11 SD (95% CI: -0.27 to 0.04).

the unmet mental health care needs of the global poor. Secondly, we study the longer-term effects of a single course of pharmacotherapy on mental health and socioeconomic outcomes. Most medical studies examine either the contemporaneous effects on mental health or the side effects of long-term, uninterrupted treatment. Thirdly, we show that adding LA to PC enhances the effect on mental health and protects against temporary negative impacts, suggesting that pairing pharmacotherapy with additional light-touch programs may be cost-effective (Wiles et al. 2016).

We also contribute to the literature on child development, which correlates parental depression with impaired development (Cummings and Davies 1994) and lower human capital investment (Claessens et al. 2015, Dahlen 2016, Shen et al. 2016).

Finally, we contribute to the understanding of poverty traps and the psychology of poverty by exploring the link between mental health and poverty (Mani et al. 2013, Mullainathan and Shafir 2013, Haushofer and Shapiro 2016). We do not find evidence that depression treatment improves labor market outcomes or increases consumption. This pattern is not consistent with a poverty trap due to the contemporaneous feedback between depression and low productivity. These results align with Baranov et al. (2020) and Bhat et al. (2022), who also focus on women in South Asia. At the same time, evidence suggests that it may be worth investigating whether and to what extent depression may prevent investment in older children's human capital, which may contribute to the intergenerational transmission of poverty.

I Setting and Interventions

We conducted this study in a peri-urban region northwest of Bangalore, Karnataka. Our study area comprises 506 villages and wards (urban jurisdictions) with at least 40 households within the catchment area of our partner NGO in the Doddaballapur, Korategere, and Gauribidanur districts.[6] To measure the prevalence and correlates of depression in this area, we concurrently surveyed a representative sample of adults in an adjacent non-study district. In this setting, 24 percent of adults aged 18 to 50 have some depression symptoms

[6] Hereafter we refer to villages and wards as "localities."

and 9 percent have symptoms of at least moderate depression.[7] Symptoms are more severe for women, older people, and people with low socioeconomic status, as studies document elsewhere (Gilman et al. 2002). We elaborate on these patterns in Appendix B.1.

We study the effects of community-based provision of pharmacotherapy among adults who screen positive for depression. We collaborated with Grameena Abudaya Seva Samsthe (GASS), a local social service organization that has worked with people with physical and mental disabilities since 2001. GASS aims to improve mental health and patient wellbeing by facilitating psychiatric care and providing livelihoods assistance. To support psychiatric care, GASS organizes walk-in clinics, sets up appointments, and helps transport people to health centers. It provides livelihoods assistance by counseling patients about employment and other earnings opportunities and by helping patients obtain training and small loans as appropriate.

The PC intervention provided eight months of free psychiatric care through the Shridevi Institute of Medical Sciences and Research Hospital. Shridevi is an accredited private hospital in Tumkur, Karnataka, near the study area. The facility has 750 beds, 80 percent of which are allocated for pro bono care of disadvantaged patients. The hospital sometimes receives patients from GASS. The initial visit included a diagnosis, an explanation of the significance of mental illness, and an individualized course of medical treatment. Patients returned for monthly follow-up visits. The most commonly prescribed anti-depressants were Selective Serotonin Reuptake Inhibitors (SSRIs). These drugs are generally not under patent and are available inexpensively in India. They are widely used and have relatively few well-tolerated side effects (Ferguson 2001, Cascade et al. 2009).[8] In addition to treating depression, the PC intervention may raise awareness and increase the salience of depression in the household, which could lead to additional effects. Appendix B.2 discusses ethical considerations.

SSRIs are often used as first-line pharmacotherapy for unipolar depression. They are usually taken daily, and a course of treatment lasts 4 to 12 months, although the optimal

[7] The prevalence of depression symptoms in our sample exceeds Sagar et al.'s (2020) estimate of the nationwide prevalence of 3-4 percent. This pattern may arise because our sample is relatively old and poor. Both age and poverty are positively associated with depression. This discrepancy may also reflect the difference between having depression symptoms and being depressed.

[8] GASS organized all visits, transported participants to their appointments, and monitored patient welfare via home visits throughout the intervention.

treatment duration is unclear (Kovich and DeJong 2015). Meta-analyses have found SSRIs to be effective for treatment of unipolar adult major depression disorder (Gartlehner et al. 2017, Cipriani et al. 2018), but have not identified consistent sources of heterogeneity by patient characteristics or depression severity.[9] Around 20 percent of patients who abruptly discontinue SSRIs experience antidepressant discontinuation syndrome. Symptoms such as dizziness, fatigue, nausea, and irritability may last for 1-2 weeks (Fava et al. 2015, Gabriel and Sharma 2017), although evidence regarding this phenomenon continues to evolve (Davies and Read 2019). Discontinuation symptoms are milder and occur less frequently for patients who receive shorter courses of treatment (Warner et al. 2006, Eveleigh et al. 2018). The long-term impacts of a single course of SSRIs are not well studied. The literature on the long-term effects of SSRIs is primarily qualitative and focuses on long-term side effects and the effects of discontinuation (e.g. Cartwright et al. 2016).

The LA intervention provided two group meetings and personalized livelihoods assistance. The meetings, which lasted three hours each, discussed ways to earn income and deal with on-the-job challenges. Each meeting had about 30 participants. In the first meeting, participants had group discussions of their experiences working and earning income, as well as the challenges they perceived in the labor market. In the second group meeting, facilitators sought to identify suitable livelihoods activities for participants. In subsequent weeks, staff provided one-on-one assistance to help participants pursue income-generating activities through job placements, small loans, or training, according to participants' individual needs and circumstances. This intervention took place during the first two months of the study. Although the program was intended to facilitate economic opportunities, the group meetings may have fostered informal support by bringing participants together (Pfeiffer et al. 2011).

[9] A meta-analysis by Arroll et al. (2005) shows that treatment with SSRIs is more effective than a placebo in primary care, where the characteristics of patients and the manifestations of depression often differ from inpatient psychiatric settings. A meta-analysis by DeMaat et al. (2006) shows that pharmacotherapy and psychotherapy are similarly effective on average, and that pharmacotherapy is effective for treatment of both mild and moderate depression. It is unclear whether the effectiveness of SSRIs varies by baseline severity of symptoms (Maslej et al. 2021). For example, Fournier et al. (2010) suggest that SSRIs may be most effective for people with severe depression. However, Kirsch et al. (2008) argue that this is because people with severe depression respond less to the placebo.

II Design, Sampling, and Recruitment

The study design and analysis follow the analysis plan that we pre-specified and registered before collecting follow-up data. Table B1 itemizes and explains the minor deviations from the analysis plan.[10] We used a cluster-randomized design to cross-randomize psychiatric care (PC) and livelihoods assistance (LA) by locality. Figure 1 provides a CONSORT chart for this study. Before starting recruitment, we stratified the randomization by district and terciles of a locality socioeconomic index based on the 2011 Census of India, for a total of nine strata.[11] We screened about forty households per locality, with the target of selecting 1-2 participants per locality. The modal and median number of participants per locality is 2. This design minimized spillovers and cross-arm contamination. Treating few people per locality limited information leakages, protecting patient confidentiality.

Our partner NGO could offer the interventions only to a limited number of people. To increase statistical power given this constraint, we allocated twice as many participants to the control arm as to each of the other intervention arms. We ultimately enrolled 395 participants (from 204 localities) in the control arm, 207 participants (from 99 localities) in the PC arm, 205 participants (from 102 localities) in the LA arm, and 195 participants (from 101 localities) in the PC/LA arm. With these sample sizes, the minimum detectable effect (MDE) for the comparison of any of the intervention arms with the control arm (e.g. PC/LA vs. control) is 0.16 SD in either the "during" or "after" periods. This calculation is based on the assumptions of 80 percent power and 95 percent confidence. For a comparison of two interventions (e.g. PC/LA vs. PC), the MDE is 0.19 SD. For a test of the complementarity between the interventions (i.e., whether the effect of PC/LA exceeds the sum of the effects of PC and LA), the MDE is 0.28 SD.[12] Appendix B.3 discusses these calculations further.

We began recruitment in December 2016. We sampled participants through a door-skip pattern in which the skips were proportional to locality size. Once at the household, surveyors

[10] The main deviations include the addition of extra survey rounds and the inclusion of LASSO estimates as a robustness test. We also omit a few outcomes that were not collected reliably or that are analyzed in a separate paper, as detailed in Table B1.

[11] Socioeconomic index components include locality averages of house quality, electrification, latrine use, and durable good ownership.

[12] The difference in sample size across intervention arms and time periods is small enough that it has negligible effect on the MDE.

randomly chose an available adult to screen for eligibility. We screened people for depression symptoms with the PHQ-9 depression severity scale (Kroenke et al. 2001). This nine-item scale ranges from 0 to 27 and higher values indicate more severe symptoms. The PHQ-9 is widely validated to screen for depression and measure the response to treatment in India and throughout the world (e.g. Patel et al. 2008, Manea et al. 2012, Indu et al. 2018). To obtain a sample of mildly or moderately depressed people, we recruited subjects with PHQ-9 scores of 9-20.[13] In total, surveyors screened 6446 people in order to enroll a study sample of 1000 participants across 506 localities. We did not stratify by gender during recruitment, and 86 percent of participants are female. This gender ratio is common in other depression studies (e.g. Patel et al. 2017) and reflects the higher prevalence of depression among women.

III Data and Measurement

We surveyed respondents five times over 26 months. Round 1 took place at recruitment, before the start of the interventions. Round 2 occurred four months after recruitment, midway through the PC intervention and at the end of the LA intervention, and Round 3 occurred eight months after recruitment, around the end of the PC intervention. Round 4 occurred 16 months after recruitment and Round 5 occurred 26 months after recruitment. We refer to Rounds 2 and 3 as "during the PC intervention" and Round 4 and 5 as "after the PC intervention" in our analysis below. Figure B2 illustrates the study timeline.

We study four categories of outcomes: (1) participants' depression severity, work hours, and earnings; (2) child human capital investment; (3) household consumption, wealth, and hygiene/sanitation; (4) and potential pathways that link depression to the other outcomes. We winsorize monetary values at 5 percent and deflate to 2017 values using the Indian consumer price index.[14]

[13] We initially used a minimum PHQ-9 threshold of 7 before revising the threshold to 9 based on our success with recruitment. As a result, 8 percent of participants have baseline PHQ-9 scores of 7 or 8. Following our IRB protocol, we referred people with PHQ-9 scores of 21 or more (indicating severe depression) for immediate treatment and did not enroll them in the study. To select the people most likely to benefit from the livelihoods intervention, we did not recruit people who had disabilities that prevented them from working, who were currently earning more than Rs. 6000 per month, or whose child care duties required them to remain at home throughout the day. We also excluded pregnant women due the additional risks of pharmacotherapy during pregnancy.

[14] Results are similar if we do not winsorize earnings and consumption.

We measure depression severity using the PHQ-9 scale. Although the PHQ-9 is not a diagnostic tool, scores of 5-9 roughly correspond to mild depression and scores of 10-20 roughly correspond to moderate or moderately-severe depression, with 88 percent sensitivity and specificity (Kroenke et al. 2001). We examine impacts on standardized PHQ-9 scores.

We measure work time – the time spent on productive activities – from a 24-hour time diary and then convert responses into weekly values. Productive activities include primary and secondary jobs, agricultural work, child care, cooking, cleaning, doing laundry, and fetching water.[15] We measure weekly earnings from primary and secondary jobs. As in other informal economies, most people who work in our setting do not receive wages or salaries. Only 28 percent of people report receiving wages from their primary occupation, which suggests that many people are self-employed or work in the informal sector.

We measure child human capital investment for all children within the household aged 5-18. Outcomes include current school enrollment, days of attendance, hours of homework, and whether the child currently works for pay. We do not observe any of these variables in Round 5. We use child-level data for these regressions but we weight by the inverse number of children per household so that estimates are comparable to other results in the paper.[16]

Per-capita consumption is the sum of household food consumption in the past week (across 23 food groups that are common locally) and expenditures on 13 non-durable non-food commodities (converted into weekly values from 1 or 2 month recalls) divided by household size.[17] We measure durable goods ownership according to indicators for household ownership of nine goods.[18] We measure hygiene and sanitation by observing whether there is open defecation or visible garbage at the respondent's home, whether the cooking area is clean, and whether the respondent has visibly dirty hands and fingernails.

To identify several potential pathways for the socioeconomic impacts of depression treat-

[15] In addition, we elicit the time devoted to primary and secondary jobs and domestic work in the past seven days. Estimates using this definition of work time yield similar results. We prefer the time diary approach because it includes time spent on productive tasks that the respondent may not define as work.

[16] Estimates based on household averages yield similar results. 54 percent of study participants live with school-aged children and treatment effects on depression are similar regardless of whether school-aged children are present.

[17] We include foods that were purchased, produced at home, or received from others. To compute the value of non-purchased food, we multiply the quantity consumed by median unit values.

[18] These goods are a chair, a bed, a table, an electric fan, a television, a refrigerator, a bicycle, a motorcycle or scooter, and a car.

ment, we measure cognitive performance, risk intolerance, subjective wellbeing, and participation in household decisions. We assess cognitive performance through three incentivized tests: Raven's Progressive Matrices, which estimates fluid intelligence, and forward and backward digit spans, which measure verbal short-term and working memory. We elicit risk intolerance through items from the Blais and Weber (2006) DOSPERT scale, a generalized risk self-assessment (Dohmen et al. 2011), and the Eckel and Grossman (2008) incentivized lottery game.[19] We use the five-item Satisfaction with Life Scale to measure subjective wellbeing (Kobau et al. 2010). As a measure of participation in household decisions, respondents indicate whether they make household financial and employment decisions alone, with other household members, or not at all. The negative shock index follows the Holmes and Rahe (1967) scale and includes indicators for whether the household has experienced the following shocks in the past four months: an illness lasting at least one month, a death, an unemployment spell, a natural disaster, incarceration, divorce, or another serious loss.

Since each family of outcomes has multiple variables, we create family-specific indices by computing the first principal component of the outcomes within each family.[20] This approach accounts for multiple inference within families. We define the sign of the components within each group so that larger values have a common interpretation. We also standardize these indices to ease interpretation. As exceptions to this approach, total consumption is defined as the sum of food and non-food consumption; for participation in household decisions, we count the number of decisions (across financial and employment decisions) that the respondent participates in.

[19] We measure these variables in Rounds 1-4 only. For the DOSPERT scale items, participants indicate their willingness to ride a motorbike without a helmet, leave their children unattended for 30 minutes, lend money to a neighbor, invest 10 percent of annual income in a new business venture, eat spoiled food, and delay a child's health care. The first four items are from the original DOSPERT scale and the last two items are customized to our setting. The incentivized lottery exercise asks participants to choose from a menu of binary lotteries with payoffs that differ in variance and expected value.

[20] The Anderson (2008) Summary Index is an alternative way to create indices. Figure B18 shows impacts on all index outcomes calculated using the Anderson (2008) approach. Results are similar.

IV Treatment Compliance

Across the three arms that received either PC or LA, 65 percent of participants had at least one psychiatric meeting (for PC) or livelihoods-related interaction (for LA). Similar proportions of PC and PC/LA participants (46 and 43 percent) attended at least one psychiatric visit ($p = 0.51$ for this comparison) according to psychiatrist records. Participation in livelihoods assistance meetings was somewhat higher (68 versus 58 percent) in LA than in PC/LA ($p = 0.10$ for this comparison). Within PC/LA, 30 percent of participants took up both interventions. Figures B3 and B4 further illustrate intervention compliance.

91 percent of people who met with a psychiatrist were diagnosed with depression. Patients who were diagnosed with depression received SSRIs for a median of four months. When asked in Round 4 to recall drug usage during the PC intervention, 91 percent of participants report that they took medications either "every day" or "almost every day" and 13 percent of patients continued to take SSRIs after the PC intervention ended. Medication adherence was 8 percentage points higher in the PC/LA arm ($p = 0.07$). This difference suggests that the LA treatment may have enabled participants to plan or follow through.[21] Among LA compliers, 81 percent attended at least one livelihoods workshop and 47 percent received personalized livelihoods assistance.[22] Appendix B.4 considers the correlates of intervention compliance. PC and PC/LA compliers are more likely to be men than non-compliers, while LA compliers are more likely to have better mental health than non-compliers. However, these differences are small and compliers and non-compliers do not differ along most dimensions, including SES and household economic circumstances. Moreover, aside from better mental health in LA, complier characteristics do not differ across arms. Because the compliance rate and the characteristics of compliers are similar in PC and PC/LA, differential impacts of PC/LA relative to PC are unlikely to arise because of differences in intervention participation.

[21] 55 percent of patients were diagnosed with anxiety, which is a common depression comorbidity (Hirschfeld 2001), and 14 percent were diagnosed with other conditions (e.g. chronic pain, anemia).

[22] Nobody in the control group sought treatment through GASS. It is possible but unlikely that control participants sought treatment elsewhere; most people with mental disorders go untreated in this setting.

V Identification and Estimation

We estimate the parameters of the following equation for respondent i in locality j and in round t:

$$\begin{aligned} Y_{ijt} = \beta_1[PC_j \cdot D_t] \; + \; \beta_2[LA_j \cdot D_t] \; + \; \beta_3[PC/LA_j \cdot D_t] \; + \\ \beta_4[PC_j \cdot A_t] \; + \; \beta_5[LA_j \cdot A_t] \; + \; \beta_6[PC/LA_j \cdot A_t] \; + \\ X'_{ij}\beta_7 \; + \; \varepsilon_{ijt} \end{aligned} \tag{1}$$

The variables PC, LA, and PC/LA are indicators for the arms that receive PC only, LA only, or both PC and LA. D ("during") is an indicator for Rounds 2 and 3 (while PC was ongoing or had just concluded) and A ("after") is an indicator for Rounds 4 and 5 (up to 26 months after the start of the PC intervention). X is a vector of predetermined covariates. The parameters β_1 to β_6 identify the Average Intent to Treat (AIT) effects of each intervention arm under the assumptions that potential outcomes of each treated person are unaffected by the treatment status of other people and that treatment assignment is independent of potential outcomes. Assigning treatment by locality minimizes instances of violations of the first assumption through spillovers such as social interactions, while treating 1-2 people per locality minimizes locality-specific equilibrium effects. Random assignment should ensure that the second assumption holds.

We test whether PC and PC/LA have the same effects ($\beta_1 = \beta_3$ and $\beta_4 = \beta_6$) and whether there are no complementarities between PC and LA ($\beta_3 - \beta_1 - \beta_2 = 0$ and $\beta_6 - \beta_4 - \beta_5 = 0$). We also test whether the treatment effects differ by arm ($\beta_1 = \beta_2 = \beta_3$ and $\beta_4 = \beta_5 = \beta_6$) and whether other pairwise effects are identical (e.g. $\beta_1 = \beta_2$, and $\beta_3 = \beta_2$). We use OLS and cluster standard errors by locality.

We estimate ANCOVA and LASSO versions of this specification for all outcomes. Under ANCOVA, X includes the baseline dependent variable and strata and time dummies.[23] The

[23] Our analysis plan prescribes using an ANCOVA specification for outcomes with low serial correlation and a difference-in-difference specification for outcomes with high serial correlation (McKenzie 2012). In practice, all outcomes have serial correlations below 0.3, except for the durable goods index, which has serial correlation of 0.63. Therefore, we use ANCOVA to streamline the analysis. Difference-in-difference estimates closely resemble ANCOVA estimates and are available from the authors.

LASSO approach uses the post-double-selection method of Belloni et al. (2014) to choose covariates. When these approaches yield similar estimates (the majority of cases), the text describes the ANCOVA estimates. Otherwise, we note the discrepancy between the two estimates.[24]

Table 1 shows baseline summary statistics of key outcome variables and covariates by intervention arm. Column 2 shows the control mean of each variable. Columns 2-7 show the mean difference between each intervention arm and the control arm, along with p-values (based on locality-clustered standard errors) that indicate the statistical significance of these differences. Finally, Column 8 provides the p-value for the joint test of significance of the three intervention arms relative to control. Most outcomes are balanced across intervention arms in Round 1, and we cannot reject that the variables in the table are jointly balanced ($p = 0.21$). However, the table shows that PHQ-9 scores are imbalanced, which could contribute to follow-up differences in this or other outcomes. To address this concern, we also estimate a version of all regressions that uses entropy weights to impose balance across arms in the first three moments of the PHQ-9 distribution (Hainmueller 2012, Hainmueller and Xu 2013). Estimates are robust to weighting, and weighted estimates (available from the authors) are generally similar to unweighted estimates.

The bottom of Table 1 shows that, overall, attrition does not vary systematically by arm except in Round 5, in which attrition is higher in the PC arm. Appendix B.5 shows that differential attrition does not confound the results we present below.[25]

[24] For the lasso regression, we allow the estimator to select from the following list of baseline covariates: strata indicators, round indicators, gender, marital status, education, scheduled caste/tribe, literacy, household size, PHQ-9 score and components, PHQ-9 < 10 indicator, PHQ-9 < 5 indicator, GAD-7 (anxiety) score and components, activities of daily living index and components, time use (all work, paid work, unpaid work, sleep, leisure, and job search hours), per capita household non-durable consumption and expenditures (total, food, non-food, clothes for children, medical), sanitation/hygiene index and components, older child human capital index and components, young child health index and components, per capita net savings and components, durable goods index and components, risk intolerance index and components, negative shock index and components, cognition index and components, subjective wellbeing index and components, participation in household decision and components. This list includes the baseline values of all outcomes in our analysis. Child human capital regressions also include child-level covariates: an indicator that the individual is the child of the study participant, the baseline human capital index and components, and age and gender dummies. To avoid dropping observations, we include indicators for missing values of all covariates and then set missing values to zero. The algorithm chooses the baseline dependent variable or some of its components in 76 percent of cases. All specifications choose at least some time dummies and 36 percent of specifications select at least some strata dummies. The algorithm selects a median of nine covariates.

[25] As a separate inquiry, we provided free hand sanitizer and explained its uses to 80 percent of participants

VI Impacts on Participants

VI.A Depression Symptoms

Table 2 shows treatment effects on depression symptoms. The impact of PC/LA is larger and more durable: PC/LA reduces the PHQ-9 score by 0.26 SD (95% CI: -0.41 to -0.10) during the PC intervention and by 0.24 SD (95% CI: -0.41 to -0.07) after, while PC alone reduces the PHQ-9 score by 0.14 SD (95% CI: -0.30 to 0.03) during the PC intervention, which is significant at the 10% level, and by a statistically insignificant 0.04 SD (95% CI: -0.19 to 0.12) afterward.[26]

The impact of PC/LA is significantly larger than the impact of PC in the "after" period ($0.04 \leq p \leq 0.06$) and it is significantly larger than the impact of LA in both periods ($0.01 \leq p \leq 0.07$). We also fail to reject that PC and LA have the same effects. The impact of PC/LA is generally larger than the sum of the impacts of PC and LA, consistent with a complementarity between these interventions. However, this difference is generally statistically insignificant ($0.10 \leq p < 0.28$). LA has a statistically insignificant effect on the PHQ-9 score: -0.08 SD (95% CI: -0.25 to 0.09) during the PC intervention and 0.01 SD (95% CI: -0.15 to 0.16) afterward.

To quantify the differential impact of PC/LA relative to PC, we compute the total reduction in PHQ-9 $\times$ months over the study period for each arm.[27] Under this metric, PC/LA is 3.5 times more effective than PC. Since PC/LA costs just 5 percent more than PC alone ($232 versus $221 per study participant), bundling PC and LA improves cost effectiveness in terms of reducing depression symptoms. Appendix B.7 describes this exercise in more detail.

Figure 2 plots the PHQ-9 densities by arm during and after the PC intervention. De-

who were present in the study by Round 2. Angelucci and Bennett (2022) explain further. Figure B19 shows that adding an indicator for this intervention to Equation (1) does not affect our estimates.

[26] Using entropy weights to correct for the baseline imbalance in PHQ-9 scores leads to estimates that are comparable and not statistically distinguishable from our main estimates. The most notable difference is that the ANCOVA estimate of the impact of PC in the "during" period shrinks from -0.14 SD to -0.13 SD and is no longer statistically significant (95% CI: -0.30 to 0.04). The comparable LASSO estimate is -0.13 SD (95% CI: -0.29 to 0.02), which is significant at the 10% level. These effect sizes are consistent with the literature, as we discuss in Appendix B.6.

[27] We multiply the "during" period estimates in Column 1 of Table 2 by eight months and the "after" period estimates in Column 1 by eighteen months.

pression symptoms decrease throughout the support both during and (to a lesser extent) after the intervention. As noted, impacts are largest for PC/LA participants. Appendix A provides estimates for indicators of "no moderate or severe depression" (PHQ-9 $<$ 10) and "no depression" (PHQ-9 $<$ 5). These estimates are helpful for comparative purposes, since they are commonly reported in the literature. In Table A1, PC/LA reduces the frequency of moderate or severe depression by 15.0 percentage points (95% CI: 7.4 to 22.5), PC reduces this frequency by 7.1 percentage points (95% CI: -0.006 to 14.7), and LA reduces this frequency by 6.9 percentage points (95% CI: -0.01 to 14.8) in the "during" period. In the "after" period, PC/LA reduces the frequency of moderate or severe depression by 7.8 percentage points (95% CI: -0.002 to 15.8), while PC and LA have smaller and statistically insignificant effects. Figure A1 shows means of these outcomes by intervention arm and round. Means for the "no depression" indicator are zero for all arms in Round 1 because we only recruited people with PHQ-9 scores of at least 7. As a result, this outcome is balanced at baseline by construction. The figure shows the largest impacts on "no moderate/severe depression" and "no depression" for the PC/LA arm in all follow-up rounds.

Figure B5 in Appendix B.8 estimates heterogeneity in the impacts on mental health by baseline characteristics, including gender, age, socioeconomic status, PHQ-9 score, physical health, cognition, and exposure to negative shocks during childhood. Both PC/LA and PC have larger effects for people with worse physical health. PC/LA is more effective for people with many childhood shocks. We do not find significant heterogeneity in the impact of LA or in the impacts of any of the interventions along other dimensions.

Figure B6 in Appendix B.9 estimates impacts on the GAD-7 anxiety score and an index of activities of daily living (ADL). The PC/LA intervention significantly reduces anxiety while the other interventions do not have statistically significant effects. The impact on activities of daily living varies by arm: PC/LA increases the ADL index, PC decreases it during the intervention, and LA does not have statistically significant effects.

VI.B Work Time and Earnings

Table 3 shows that PC/LA and PC have different treatment effects on work time and earnings while the PC intervention is ongoing. PC reduces weekly work time by 5.4 hours (95% CI:

-8.7 to -2.1) and weekly earnings by 65 rupees (95% CI: -172 to 41), a 10 percent decrease in both outcomes, although the effect on earnings is not statistically significant. Figure B7 shows that there is a concurrent increase in sleep and leisure time in the PC arm, as we discuss in Appendix B.10. This pattern suggests that PC may reduce work time by increasing the marginal utility of leisure or self-care. Alternatively, mental health stigma might reduce either labor supply or demand (Corrigan et al. 2001, Bharadwaj et al. 2017). By contrast, PC/LA increases weekly work time by 1.1 hours (95% CI: -2.2 to 4.3) and weekly earnings by 38 rupees (95% CI: -83 to 158), but neither effect is statistically significant. The difference between the effects of PC/LA and PC in the "during" period is significant for work time ($p = 0.001$) but not for earnings ($p = 0.12$). None of the impacts of PC and PC/LA are statistically different from zero or from each other at the 5 percent level in the "after" period.

In sum, our results suggest that pharmacotherapy does not increase the time spent on productive activities in our sample. 86 percent of our study participants are female, and low female labor force participation in India may weaken the labor market impacts in this setting. Baranov et al. (2020) and Bhat et al. (2022) also find no long-term effects of depression treatment on labor market outcomes among all or mostly female samples in South Asia. By contrast, Patel and Kleinman (2003) and Patel et al. (2017) find that mental health care reduces self-reported work absenteeism and Lund et al. (2022) find that various mental health interventions have positive effects on employment.

The LA intervention has no statistically or economically significant effects on work time or earnings. In the "during" period, the effect of LA on work time is -1.0 hours per week (95% CI: -4.3 to 2.3) and the effect on earnings is -33 rupees per week (95% CI: -154 to 89). In the "after" period, the effect of LA on work time is -1.5 hours per week (95% CI: -5.4 to 2.3) and the effect on earnings is 48 rupees per week (95% CI: -74 to 170). There appear to be multiple barriers to increasing work time and earnings for our sample. Our findings suggest that neither mental health care nor livelihoods assistance are sufficient to overcome these barriers.

VII Impacts on Children and the Household

Impacts on child human capital investment appear in Table 4. Estimates are not statistically significant in the pooled data in Columns 1-2. In the "during" period, PC/LA has an effect of -0.14 SD (95%CI: -0.32 to 0.04) and PC has an effect of 0.11 SD (95% CI: -0.04 to 0.25). In the "after" period, PC/LA has an effect of 0.12 SD (95% CI: -0.13 to 0.37) and PC has an effect of 0.18 SD (95% CI: -0.01 to 0.38). Next, we examine heterogeneity by the median age of 12, which corresponds to the transition to secondary school. Estimates are small and statistically insignificant for younger children in Columns 3-4. For older children, PC/LA has an impact of 0.40 SD (95% CI: -0.06 to 0.86) and PC has an impact of 0.44 SD (95% CI: 0.12 to 0.75) in the "after" period in Columns 5-6. All older child estimates are statistically significant at the 10 percent level in the "after" period, with slightly larger estimates under LASSO.[28]

The effects differ by child age for PC/LA ($p = 0.09$) and for PC ($p = 0.02$). This pattern may reflect a ceiling on the potential impact for younger children. For example, among children who are 12 or younger in the control group, 94 percent are enrolled and 0.5 percent work for pay across Rounds 1-4. By comparison, 85 percent of children over 12 are enrolled and 11 percent work for pay. To benchmark these impacts, we compare our estimates for enrollment with the impacts of educational interventions on enrollment from Evans and Yuan (2020) and conditional and unconditional cash transfers from the meta-analysis by Baird et al. (2014). We find that our estimates are within the range of impacts from both studies, suggesting that these effects are economically relevant and that adult depression may cause substantial demand-side barriers to older children's human capital accumulation.

The effects of LA are also insignificant for the pooled sample and for younger children. However, there is an impact of 0.32 SD (95% CI: -0.05 to 0.70) for older children in the "after" period. Finding that the LA intervention, which had minimal mental health improvements,

[28]The impact on child human capital investment may happen with a lag because enrollment typically occurs at the beginning of the academic year. In addition, school attendance and homework time are likely to be inelastic among non-enrolled students and among all students during periods when school is not in session. Enrollment, attendance, and homework may be unresponsive in Round 2 because it occurred during the same academic year as Round 1. Moreover, attendance and homework may be unresponsive in Round 3 because school was not in session for many students at that time. Appendix B.11 shows impacts on the components of the child human capital investment index.

also increases human capital investment for older children suggests that the treatments could affect human capital investment through channels other than improved mental health. For example, the interventions may raise awareness of mental health within the household or lead the household to reconsider important economic choices.

Figure 3 shows treatment effect heterogeneity in the "after" period by several additional child and respondent characteristics. We show differential effects by the gender, relation to the study participant, and baseline human capital of children, as well as by the baseline depression severity, gender, age, socioeconomic status, physical health, cognition, and exposure to childhood shocks of the associated study participants. PC/LA has a significantly larger effect for boys and for children with high baseline human capital. LA has a significantly larger effect for children living with study participants who are relatively old. Other differential effects are not statistically significant. In principle, an adult mental health improvement might increase child human capital investment by lessening the burden of home production that falls on school-aged children. Since girls play a larger role in home production than boys, the lack of a differential effect for girls suggests that this explanation is unlikely.

Figure 4 shows that the interventions have no statistically significant impacts on hygiene/sanitation, durable good ownership, or household consumption. An exception to this pattern is that PC significantly reduces per capita household consumption in the "during" period. A concurrent decline in per capita household income for the PC arm may be responsible for this effect.[29] Appendix B.12 shows impacts on the components of these indices.

VIII Potential Pathways

This section considers four pathways that may link depression and depression treatment to socioeconomic outcomes: risk intolerance, subjective wellbeing, cognitive performance, and participation in household decisions. Figure 5 shows treatment effects on these outcomes. While there is not strong evidence of improvements on any of these outcomes, a few interesting patterns emerge, as we describe below.

[29] PC reduces per capita household income by Rs. 44 (95% CI: -3 to 90), which is significant at the 10% threshold, and reduces per capita household consumption by Rs. 59 (95% CI: 13 to 104), in the "during" period. No other arms have statistically significant effects on this outcome either during or after the PC intervention.

Although the results are only suggestive, pharmacotherapy may increase risk intolerance in the "after" period, with effects of 0.18 SD (95% CI: -0.05 to 0.41) for PC/LA and 0.24 SD (95% CI: 0.01 to 0.47) for PC.[30] An effect of depression on the marginal utility of consumption could jointly explain this result and the impact on child human capital investment above. The reasoning is as follows. Anhedonia, a common depression symptom, suppresses the ability to derive happiness from pleasant activities. By reducing the marginal utility of consumption, this symptom may narrow the utility gap between good and bad outcomes. Consequently, someone with depression may be less interested in achieving the consumption gains of human capital investment as well as avoiding the consumption losses associated with negative shocks. Since the willingness to avoid negative shocks is a key aspect of risk intolerance, anhedonia may also lead someone to take additional risks.[31] Consistent with this interpretation, we find suggestive evidence that the pharmacotherapy interventions reduce the incidence of negative shocks in the "after" period: Figure B15 shows that the effect of PC/LA on the incidence of negative shocks is -0.14 SD (95% CI: -0.31 to 0.02), which is significant at the 10 percent level, and the effect of PC is -0.11 SD (95% CI: -0.27 to 0.04).[32] Appendices B.13 and B.14 provide further details on these outcomes.

Next, we consider estimates for subjective wellbeing. Effect sizes vary by arm. While most estimates are statistically insignificant at conventional levels, LA significantly decreases subjective wellbeing during the PC intervention (-0.18 SD; 95% CI: -0.02 to -0.33). LA participants may have been disappointed if they expected benefits of the intervention that did not occur. Such an effect would echo results by Adhvaryu et al. (2020), who found that people whose material conditions improved by less than expected displayed lower subjective wellbeing.

Depression could also change behavior by affecting cognition. Figure 5 shows an impact of PC/LA on cognitive performance of -0.16 SD (95% CI: -0.33 to 0.01), which is significant at the 10% level, and an impact of PC on cognitive performance of -0.19 SD (95% CI: -0.35 to -0.04) in the "after" period. The lack of a positive effect rules out improved cognition

[30] The effect of LA in the "after" period is -0.03 SD (95% CI: -0.25 to 0.20), although it is bigger in the "during" period.

[31] This logic is commonly applied to the measurement of the value of statistical life (e.g. León and Miguel 2017), in which the willingness to pay to avoid mortality risk indicates the value of life over death.

[32] Estimates for individual shocks are not statistically significant after adjusting for multiple inference.

as a pathway linking better mental health to socioeconomic improvements in our sample. Appendix B.15 discusses possible explanations for this finding and argues that low effort on the cognitive assessments and antidepressant discontinuation syndrome are both unlikely explanations for this effect.

Finally, we consider impacts on participation in household decisions. We do not find evidence for this channel: most estimates are small and statistically insignificant both during and after the PC intervention. An examination of the components of this index in Figure B17 suggests a shift toward joint rather than individual decision-making under PC.

IX Joint Significance and Treatment Complementarities

This section tests whether the interventions have effects that are jointly significant across the eleven main outcomes of our analysis.[33] We use the "omnibus" test proposed by Young (2019) to examine the joint significance of the interventions. We reject the null hypothesis that the three interventions are jointly insignificant ($p < 0.001$). Implementing this test separately by intervention arm, we reject the hypothesis of no effect of PC/LA ($p = 0.001$) and of PC ($p = 0.001$), but fail to reject the hypothesis of no effect of LA ($p = 0.22$). Therefore, we conclude that both pharmacotherapy interventions have significant effects.

Next, we re-estimate the effects on these eleven outcomes as a system of seemingly unrelated regressions (SUR) to test the additional hypotheses described in Section V jointly across outcomes. When comparing the effects of PC and PC/LA ($H_0 : PC = PC/LA$), we reject equivalence in the "during" period ($p = 0.001$) but not in the "after" period ($p = 0.37$). We also reject equivalence if we pool time periods ($p = 0.001$). When testing for "no complementarity" in the effects of PC and LA ($H_0 : PC/LA = PC + LA$), we do not find evidence of a complementarity in either the "during" period ($p = 0.76$) or the "after" period ($p = 0.78$). However we reject the hypothesis of "no complementarity" if we pool time periods ($p = 0.02$).[34] We reject the hypothesis that the three interventions have equal

[33] These outcomes are the PHQ-9 score, weekly work time, weekly earnings, child human capital investment, hygiene/sanitation, durable goods ownership, per-capita consumption, risk intolerance, subjective wellbeing cognitive performance, and participation in household decisions.

[34] The SUR approach allows us to test the remaining Section V hypotheses jointly. We do not find strong evidence that the effects of PC and LA are different ($p = 0.10$ overall, $p = 0.29$ in the "during" period,

effects overall ($p < 0.001$) and in the "during" period ($p < 0.001$) but not in the "after" period ($p = 0.18$).

X Discussion

There is an urgent need for mental health care in India and other developing countries. For example, in a representative survey we conducted adjacent to the study area, 24 percent of adults had at least mild depression symptoms and depression was strongly correlated with low socioeconomic status. Although the Mental Health Care Act of 2017 creates a legally-binding right to mental health care in India (Duffy and Kelly 2019), only 15 percent of people with depression in India receive care (Gautham et al. 2020).

Evidence regarding the effectiveness of depression treatment in low-income settings is limited (Patel et al. 2007). The impact of treatment may differ across developed and developing countries due to disparities in health care access and quality, the severity of depression, the prevalence of different types of depression (Harald and Gordon 2012), awareness of mental illness, stigma, and treatment compliance.

Psychotherapy and pharmacotherapy are the leading approaches to depression treatment. While studies have shown the utility of psychotherapy as a way to provide depression care to poor people in developing countries (e.g., Baranov et al. 2020, Haushofer et al. 2020, Patel et al. 2017, Barker et al. 2021), research has not explored the effectiveness of community-based pharmacotherapy. Since it requires fewer personnel than psychotherapy, pharmacotherapy may be a valuable tool to treat depression in low and middle income countries, where mental health specialists are scarce (Saxena et al. 2007).

In our trial, we find effects on depression symptoms that align with the clinical literature (Gartlehner et al. 2017). There is also suggestive evidence that treatment increases human capital investment for older children. However, pharmacotherapy has some transitory negative effects (e.g., on productive time and consumption) and reduces cognitive performance. Pairing livelihoods assistance with pharmacotherapy increases the size and duration of the mental health benefit, preserves the positive effects on older children's human capital in-

$p = 0.24$ in the "after" period). For the hypothesis that the effects of PC/LA and PC are equal, the p-value is 0.00 overall, 0.00 in the "during" period, and 0.37 in the "after" period.

vestment (although the effect becomes insignificant at the 95% level), and safeguards people against several of these negative effects. Adding livelihoods assistance is cost-effective, since it increases the intervention benefits while raising costs by only 5 percent.

Future research should investigate the complementarity between pharmacotherapy and livelihoods assistance and whether other inexpensive light-touch interventions enhance the benefit of mental health care in a similar way. Since LA does not directly increase work time or earnings, features other than job-related benefits of LA may impact mental health. The group and individual social interactions that occurred under LA may have enabled participants to receive emotional support from like-minded peers. Higher medication adherence among the PC/LA participants also suggests that LA may have improved the ability of participants to plan or follow through. Moreover, LA may have helped participants overcome the stigma of receiving mental health care by creating a "reason" for participating without admitting to mental illness.

Our findings suggest that there may be a negative effect of depression on investment. Anhedonia, a core feature of depression, could explain this pattern. Anhedonia lowers the marginal utility of consumption, which may lead people with depression to appear risk tolerant and unwilling to make investments. By alleviating this symptom, pharmacotherapy may increase human capital investment, as well as reduce risk tolerance and the incidence of negative shocks. We acknowledge that this evidence is tenuous and that this interpretation is speculative.[35]

[35] This effect is consistent with findings by Finkelstein et al. (2013) and Schofield and Venkataramani (2021).

Table 1: Baseline Summary Statistics

	Control Mean	PC/LA Diff.	PC/LA P-Value	PC Diff.	PC P-Value	LA Diff.	LA P-Value	Joint P-Value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Respondent, Child, and Household Characteristics								
Age	35.6	-0.32	0.65	-0.53	0.46	-0.69	0.32	0.76
Female	0.90	-0.07	0.04	-0.06	0.07	-0.05	0.09	0.07
Married	0.78	0.00	0.94	0.00	0.96	-0.03	0.38	0.82
Schooling (years)	4.95	0.32	0.39	-0.22	0.57	0.08	0.83	0.65
Scheduled Caste/Tribe	0.50	0.02	0.76	0.06	0.25	0.05	0.31	0.61
Literacy (1-3)	1.9	0.05	0.55	-0.05	0.54	-0.01	0.94	0.78
Household size	4.17	0.07	0.63	0.01	0.94	-0.19	0.18	0.38
Exposure to early-life shocks	92.5	-1.8	0.83	7.5	0.39	-7.0	0.43	0.57
Housing quality index	0	0.02	0.88	0.06	0.57	0.01	0.94	0.95
B: Primary Outcomes								
PHQ-9 depression scale (0-27)	14.4	-0.83	0.03	-0.5	0.21	-0.8	0.03	0.08
Weekly paid and unpaid work hours	57.0	-1.75	0.51	-2.0	0.35	-0.19	0.94	0.76
Weekly earnings (Rs.)	308	103	0.14	57	0.37	7	0.9	0.44
C: Household Socioeconomic Outcomes								
Hygiene and sanitation	0	-0.13	0.23	0.00	0.98	0.09	0.37	0.36
Durable goods	0	0.03	0.78	0.06	0.51	-0.08	0.40	0.62
Total per capita consumption (Rs.)	463	-18	0.46	21	0.36	18	0.53	0.46
D: Child Outcomes								
Human Capital Investment Index	0	-0.11	0.43	-0.05	0.71	0.11	0.29	0.40
E: Potential Mechanisms								
Risk intolerance	0	-0.26	0.02	-0.05	0.63	-0.21	0.05	0.04
Cognitive performance	0	0.05	0.62	-0.12	0.22	-0.06	0.47	0.43
Subjective wellbeing	0	-0.01	0.91	0.11	0.25	0.08	0.43	0.52
Participation in household decisions	1.24	0.12	0.16	0.05	0.57	0.17	0.04	0.16
Present in Round 2	0.95	-0.01	0.66	-0.04	0.07	0.01	0.43	0.13
Present in Round 3	0.89	0.01	0.71	-0.02	0.56	0.01	0.69	0.80
Present in Round 4	0.83	-0.03	0.39	-0.01	0.74	0.03	0.47	0.57
Present in Round 5	0.85	-0.02	0.51	-0.13	0.00	0.02	0.50	0.01

Note: PC = psychiatric care, LA = livelihoods assistance, C = control. All statistics are computed at baseline. Column 1 shows the mean in the control arm. Columns 2, 4, and 6 show the difference between the PC/LA, PC, and LA arms and the control arm. Columns 3, 5, and 7 show the p-values for these differences based on regressions with locality-clustered standard errors. Column 8 reports the p-value for the joint significance of the parameters in Columns 2, 4, and 6. The housing quality index is the first principal component of four measures of the quality of the flooring, roof, and walls of the dwelling as observed by the surveyor. Exposure to early life shocks is based on the Holmes and Rahe (1967) scale. All other variables are self explanatory or are described in the text. $N = 743$ for the housing quality index and $N = 906$ for child human capital. N ranges from 971 to 1000 for all other outcomes.

Table 2: Impact on Depression Severity

	PHQ-9 (std.)	
	(1)	(2)
A: During the PC Intervention		
PC/LA	-0.26	-0.26
	(0.081)	(0.080)
PC	-0.14	-0.15
	(0.083)	(0.079)
LA	-0.079	-0.063
	(0.087)	(0.079)
H_0: $PC/LA = PC$	0.21	0.23
H_0: $PC/LA = PC + LA$	0.76	0.70
H_0: $PC = LA$	0.55	0.36
H_0: $PC/LA = LA$	0.07	0.04
H_0: $PC/LA = PC = LA$	0.17	0.11
Control mean of outcome	0	0
B: After the PC Intervention		
PC/LA	-0.24	-0.24
	(0.086)	(0.087)
PC	-0.039	-0.067
	(0.077)	(0.075)
LA	0.0058	0.016
	(0.081)	(0.079)
H_0: $PC/LA = PC$	0.04	0.06
H_0: $PC/LA = PC + LA$	0.10	0.12
H_0: $PC = LA$	0.62	0.35
H_0: $PC/LA = LA$	0.01	0.01
H_0: $PC/LA = PC = LA$	0.03	0.03
Control mean of outcome	0	0
Baseline outcome coefficient	0.151	0.095
	(0.026)	(0.023)
Specification	ANCOVA	LASSO
Observations	3476	3476

Note: The table reports AIT effects following Equation (1). Column 1 uses an ANCOVA specification that controls for time indicators, strata indicators, and the baseline dependent variable. Column 2 uses the post-double-selection LASSO method to choose covariates (Belloni et al. 2014). Footnote 24 explains this approach in more detail. Locality-clustered standard errors appear in parentheses. "During" and "after" estimates are based on a common regression. The outcome is the standardized PHQ-9 depression severity score.

Table 3: Impact on Weekly Work Time and Earnings

	Hours		Earnings	
	(1)	(2)	(3)	(4)
A: During the PC Intervention				
PC/LA	1.07	1.48	37.9	22.4
	(1.66)	(1.60)	(61.3)	(57.7)
PC	-5.40	-4.92	-65.4	-82.9
	(1.70)	(1.64)	(54.2)	(53.1)
LA	-1.02	-0.50	-32.8	-38.0
	(1.68)	(1.61)	(61.8)	(58.1)
H_0: $PC/LA = PC$	0.00	0.00	0.12	0.10
H_0: $PC/LA = PC + LA$	0.00	0.01	0.14	0.10
H_0: $PC = LA$	0.03	0.02	0.63	0.48
H_0: $PC/LA = LA$	0.29	0.30	0.33	0.38
H_0: $PC/LA = PC = LA$	0.00	0.00	0.30	0.26
Control mean of outcome	58.7	58.7	577.1	577.1
B: After the PC Intervention				
PC/LA	-3.31	-2.84	38.7	20.8
	(1.77)	(1.74)	(67.3)	(65.9)
PC	-1.18	-0.84	-52.8	-63.6
	(1.98)	(1.89)	(61.0)	(57.5)
LA	-1.52	-1.04	47.9	45.1
	(1.95)	(1.93)	(62.2)	(60.0)
H_0: $PC/LA = PC$	0.34	0.35	0.22	0.24
H_0: $PC/LA = PC + LA$	0.84	0.74	0.65	0.68
H_0: $PC = LA$	0.89	0.93	0.15	0.10
H_0: $PC/LA = LA$	0.42	0.41	0.90	0.74
H_0: $PC/LA = PC = LA$	0.58	0.58	0.29	0.23
Control mean of outcome	60.4	60.4	639.2	639.2
Baseline outcome coefficient	0.208	0.131	0.188	0.095
	(0.023)	(0.020)	(0.028)	(0.028)
Specification	ANCOVA	LASSO	ANCOVA	LASSO
Observations	3476	3476	3476	3476

Note: The table reports AIT effects following Equation (1). Columns 1 and 3 use an ANCOVA specification that controls for time indicators, strata indicators, and the baseline dependent variable. Columns 2 and 4 use the post-double-selection LASSO method to choose covariates (Belloni et al. 2014). Footnote 24 explains this approach in more detail. Locality-clustered standard errors appear in parentheses. “During” and “after” estimates are based on a common regression. The outcome in Columns 1 and 2 is weekly productive time, which is the sum of time spent on primary and secondary jobs, agriculture, child care, cooking, cleaning, laundry, and fetching water. The outcome in Columns 3 and 4 is weekly earnings from primary and secondary jobs.

Table 4: Impact on Child Human Capital Investment

	Child Human Capital Investment Index					
	Full Sample		Child Age ≤ 12		Child Age > 12	
	(1)	(2)	(3)	(4)	(5)	(6)
A: During the PC Intervention						
PC/LA	-0.14	-0.12	-0.065	-0.065	-0.23	-0.22
	(0.090)	(0.090)	(0.059)	(0.060)	(0.17)	(0.17)
PC	0.11	0.13	0.00027	0.00065	0.17	0.19
	(0.073)	(0.077)	(0.056)	(0.059)	(0.12)	(0.13)
LA	0.036	0.042	-0.061	-0.061	0.12	0.12
	(0.065)	(0.067)	(0.059)	(0.059)	(0.10)	(0.12)
H_0: $PC/LA = PC$	0.01	0.01	0.35	0.35	0.03	0.03
H_0: $PC/LA = PC + LA$	0.02	0.04	0.96	0.96	0.01	0.02
H_0: $PC = LA$	0.34	0.23	0.38	0.36	0.67	0.57
H_0: $PC/LA = LA$	0.06	0.08	0.95	0.96	0.04	0.05
H_0: $PC/LA = PC = LA$	0.05	0.09	0.57	0.56	0.07	0.08
Control mean of outcome	0	0	0.22	0.22	-0.20	-0.20
B: After the PC Intervention						
PC/LA	0.12	0.12	-0.083	-0.073	0.40	0.30
	(0.13)	(0.13)	(0.13)	(0.13)	(0.24)	(0.27)
PC	0.18	0.21	-0.012	-0.0014	0.44	0.44
	(0.099)	(0.10)	(0.11)	(0.11)	(0.16)	(0.17)
LA	0.11	0.11	-0.025	-0.019	0.32	0.30
	(0.12)	(0.11)	(0.11)	(0.11)	(0.19)	(0.20)
H_0: $PC/LA = PC$	0.61	0.45	0.62	0.62	0.87	0.56
H_0: $PC/LA = PC + LA$	0.30	0.21	0.80	0.77	0.21	0.16
H_0: $PC = LA$	0.53	0.33	0.92	0.89	0.53	0.40
H_0: $PC/LA = LA$	0.97	0.92	0.69	0.71	0.76	0.99
H_0: $PC/LA = PC = LA$	0.78	0.56	0.88	0.88	0.82	0.63
Control mean of outcome	0	0	0.21	0.21	-0.27	-0.27
Baseline outcome coefficient	0.40	0.32	0.15	–	0.47	0.49
	(0.045)	(0.056)	(0.045)		(0.049)	(0.054)
Specification	ANCOVA	LASSO	ANCOVA	LASSO	ANCOVA	LASSO
Observations	2232	2232	1244	1244	988	988

Note: The table reports AIT effects following Equation (1). Locality-clustered standard errors appear in parentheses. The "during" period in Panel A includes Rounds 2-3 and the "after" period in Panel B includes Round 4 because child human capital data are not available in Round 5. "During" and "after" estimates are based on a common regression. All estimates are weighted by the inverse number of school-aged children in the household. We test whether treatment effects are equal for younger and older children in the "after" period and report the following p-values: Columns 3 and 5: $p = 0.09$ for PC/LA, $p = 0.02$ for PC, and $p = 0.19$ for LA; Columns 4 and 6: $p = 0.10$ for PC/LA, $p = 0.04$ for PC, and $p = 0.21$ for LA.

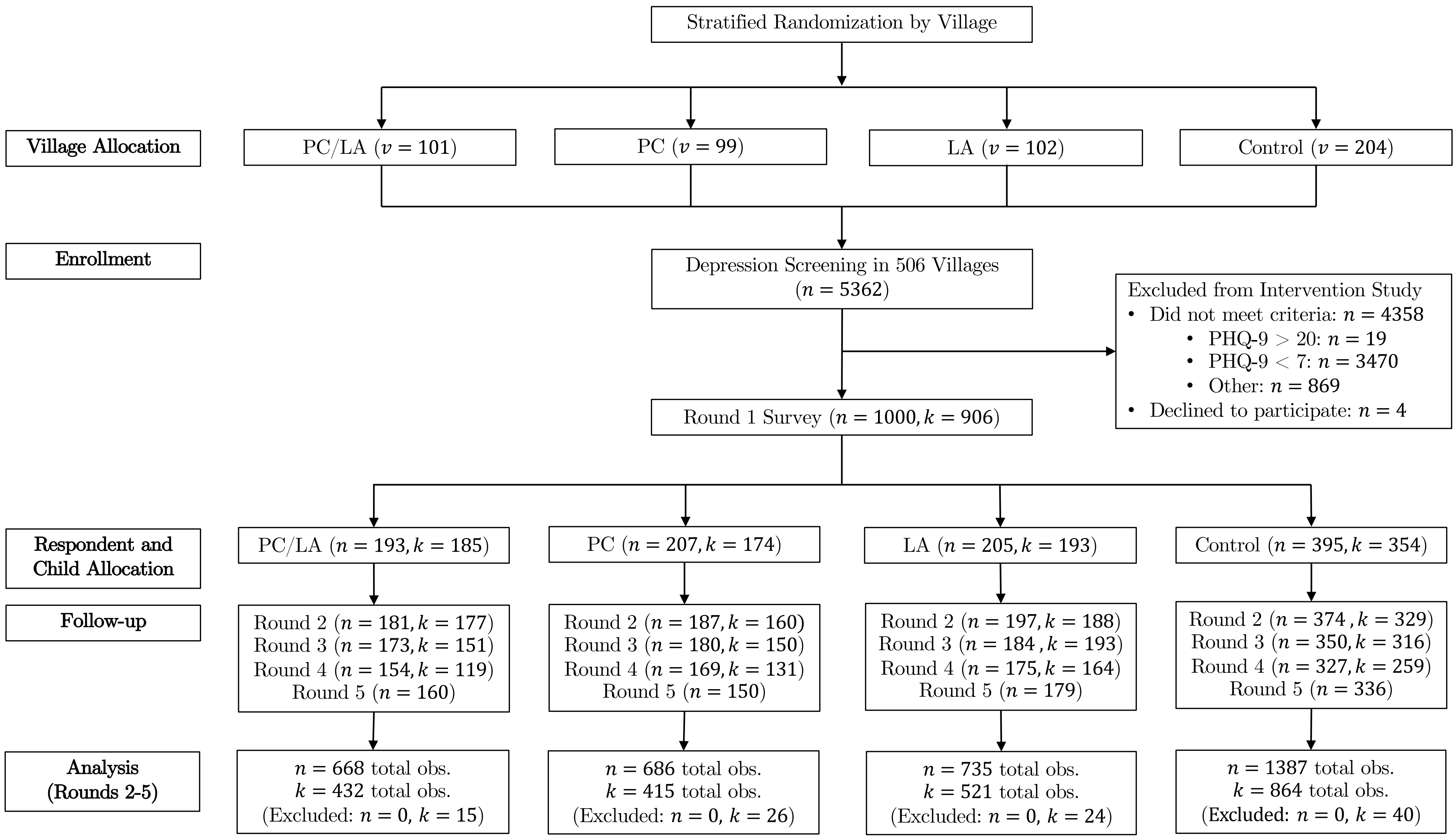


Figure 1: CONSORT Flow Diagram

Note: the chart illustrates the trial design and the allocation of localities, respondents, and household children to intervention arms. v indicates the number of localities, n the number of respondents, and k number of the school-aged children. We randomized localities across intervention arms and then recruited participants through depression screening within the community. Participants immediately completed the Round 1 ("Baseline") survey. Participants in the PC/LA and PC arms received the PC intervention. Participants in the PC/LA and LA arms received the LA intervention. In addition to the PHQ-9, participants could be excluded from the intervention study if they were pregnant, not interested in work, or had full-time child care duties. Some participants failed to qualify for multiple reasons. Some children were excluded from the estimation sample because surveyors did not obtain information about their ages. Figure B2 illustrates the study timeline in more detail.

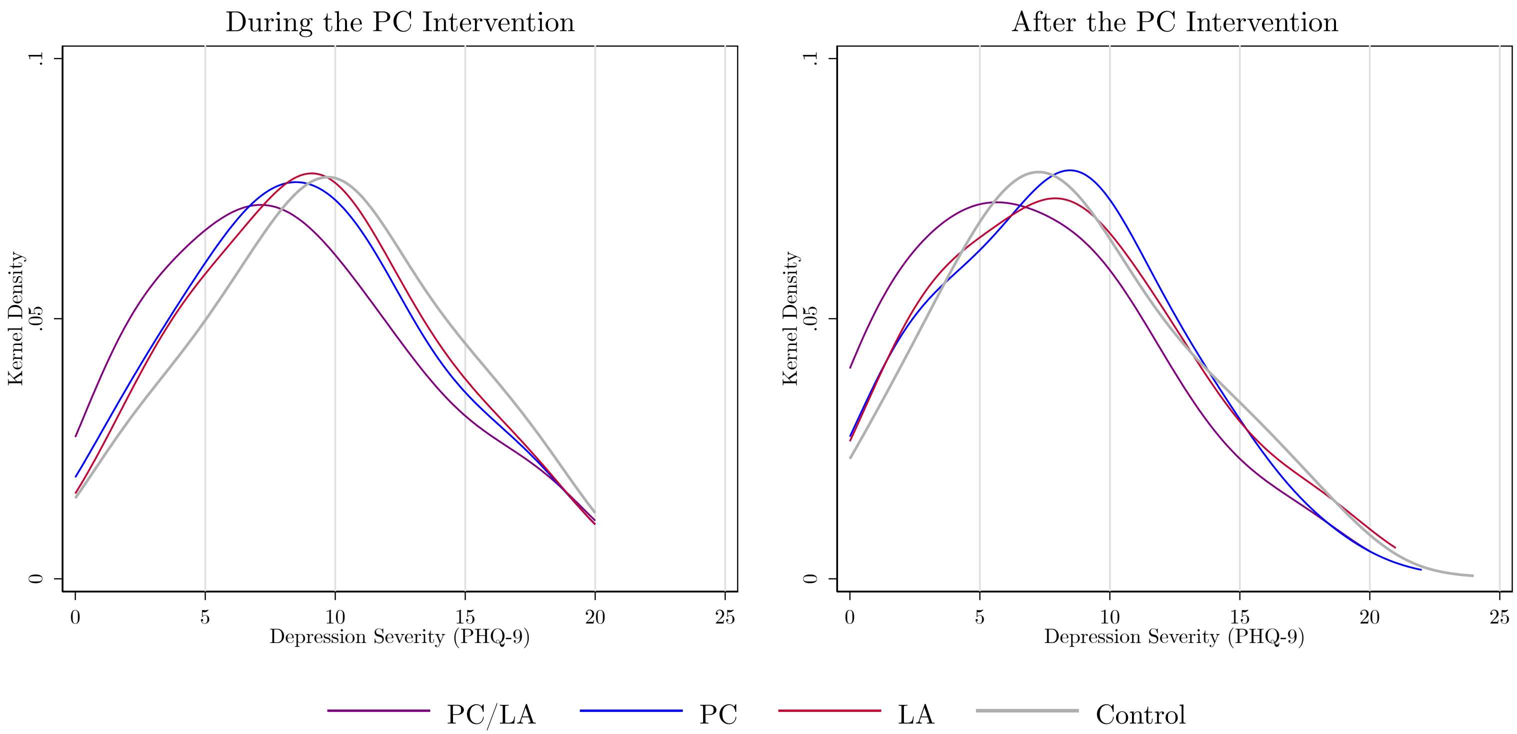


Figure 2: Density of PHQ-9 Scores by Arm

Note: The figure shows the density of PHQ-9 scores by intervention arm during the PC intervention (left panel) and after the PC intervention (right panel).

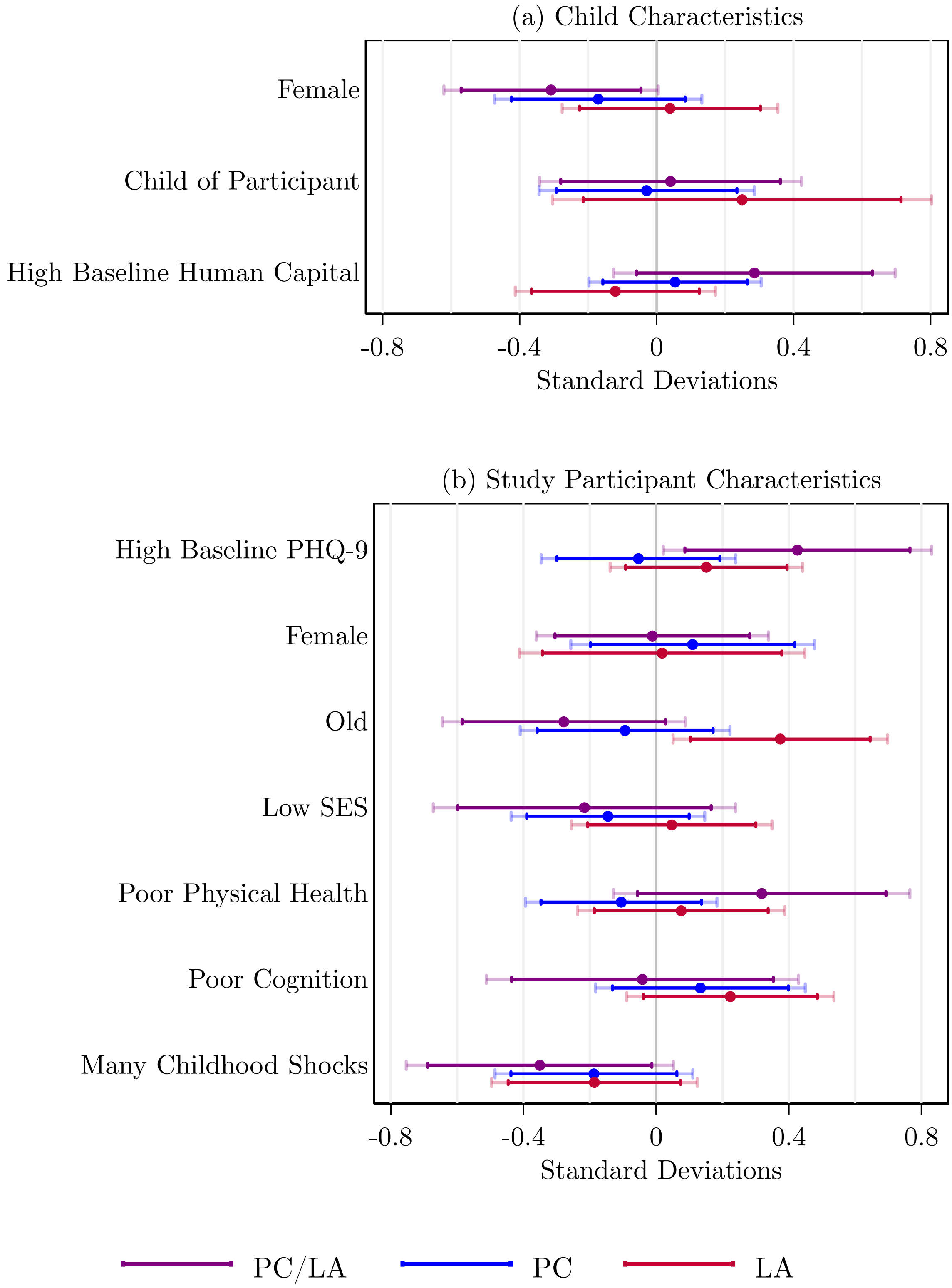


Figure 3: Differential Effects on Child Human Capital Investment by Child and Study Participant Characteristics in Round 4

Note: The figure shows differential impacts on the child human capital index in Round 4 for indicated subgroups. Light bars indicate 95 percent confidence intervals and dark bars indicate 90 percent confidence intervals based on locality-clustered standard errors. All estimates follow the ANCOVA specification of Equation (1), which controls for time indicators, strata indicators, and the baseline dependent variable. Results using the Belloni et al.'s (2014) post-double-selection LASSO method to choose covariates appear in Figure B22. All estimates are weighted by the inverse number of school-aged children in the household. Panel (a) shows differential effects according to child characteristics and Panel (b) shows differential effects according to study participant characteristics. Physical health is the first principal component of five activities of daily living and recent levels of pain. Cognition is the first principal component of scores for the Raven's Progressive Matrices and forward and backward digit spans. Childhood shocks follows the Holmes and Rahe (1967) index of childhood negative life events. Other variables are defined in the text. We divide at the median for baseline human capital investment (0.24 SD), PHQ-9 score (15), age (36), SES (-0.13 SD), physical health (-0.04 SD), cognition (-0.55 SD), and exposure to childhood shocks (65).

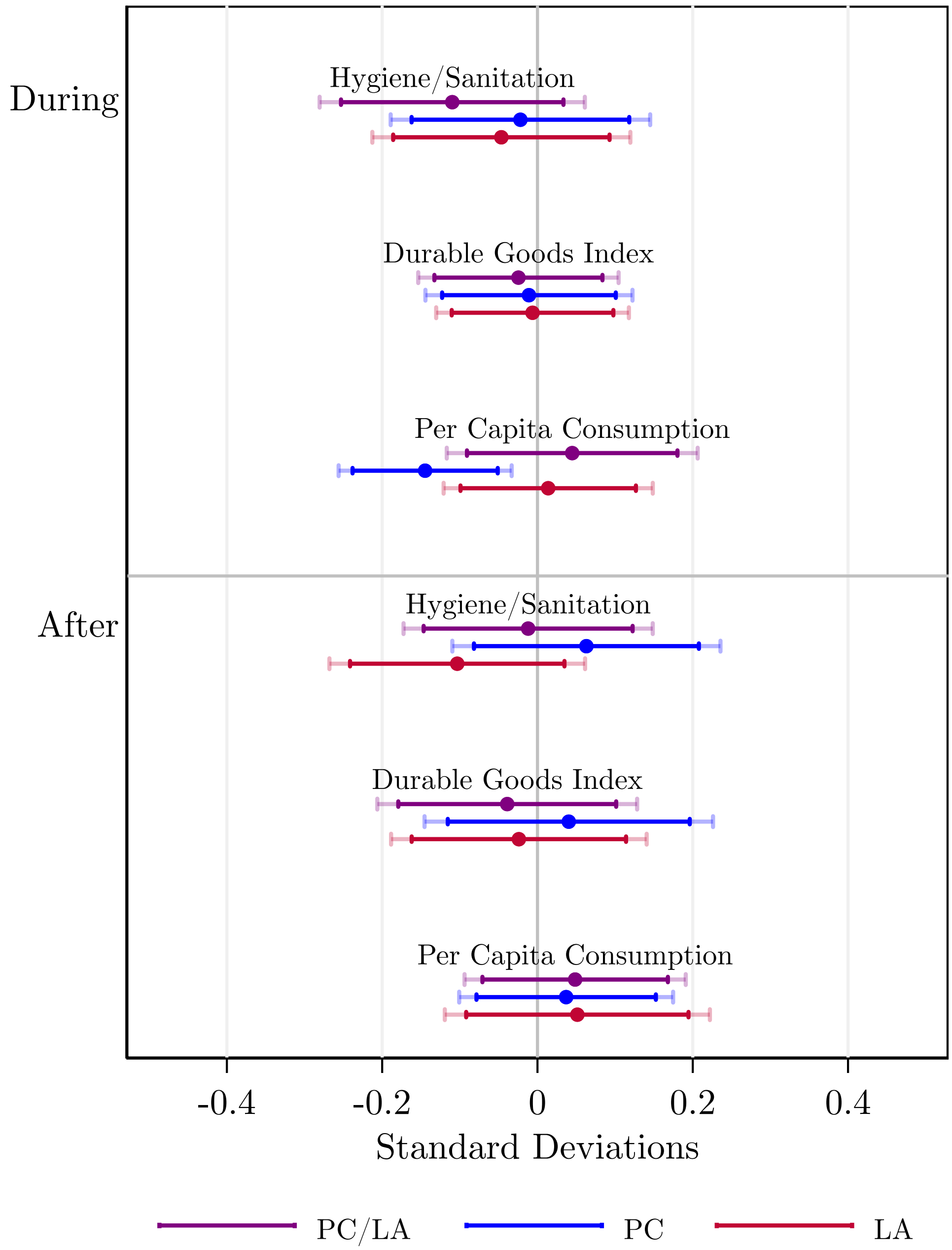


Figure 4: Standardized Impacts on Socioeconomic Outcomes

Note: The figure shows standardized impacts for socioeconomic outcomes, as explained in the text. Light bars indicate 95 percent confidence intervals and dark bars indicate 90 percent confidence intervals based on locality-clustered standard errors. All estimates follow the ANCOVA specification of Equation (1), which controls for time indicators, strata indicators, and the baseline dependent variable. Results using the Belloni et al.'s (2014) post-double-selection LASSO method to choose covariates appear in Figure B20. The top of the figure shows impacts during the PC intervention and the bottom of the figure shows impacts after the PC intervention.

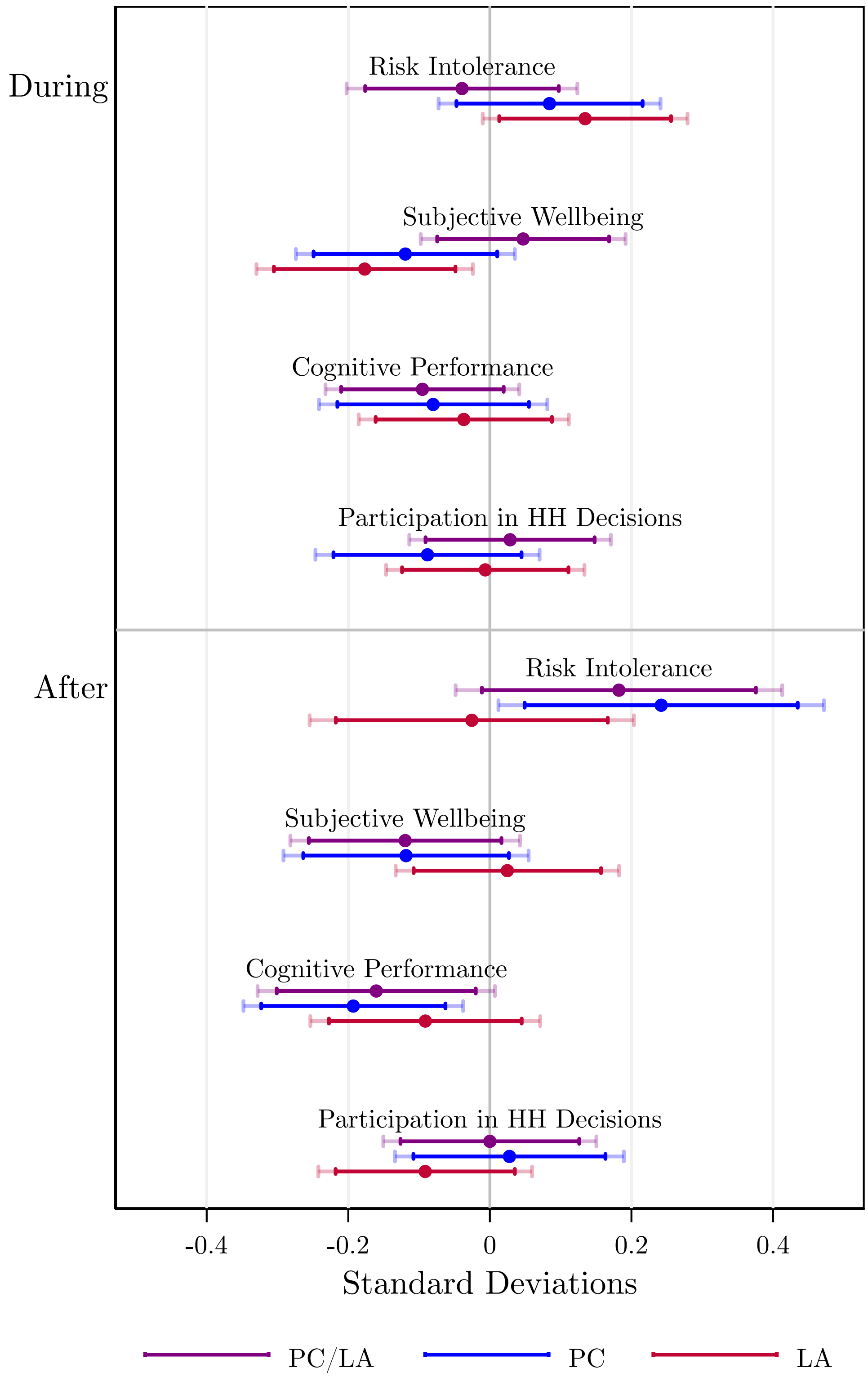


Figure 5: Standardized Impacts on Possible Pathways

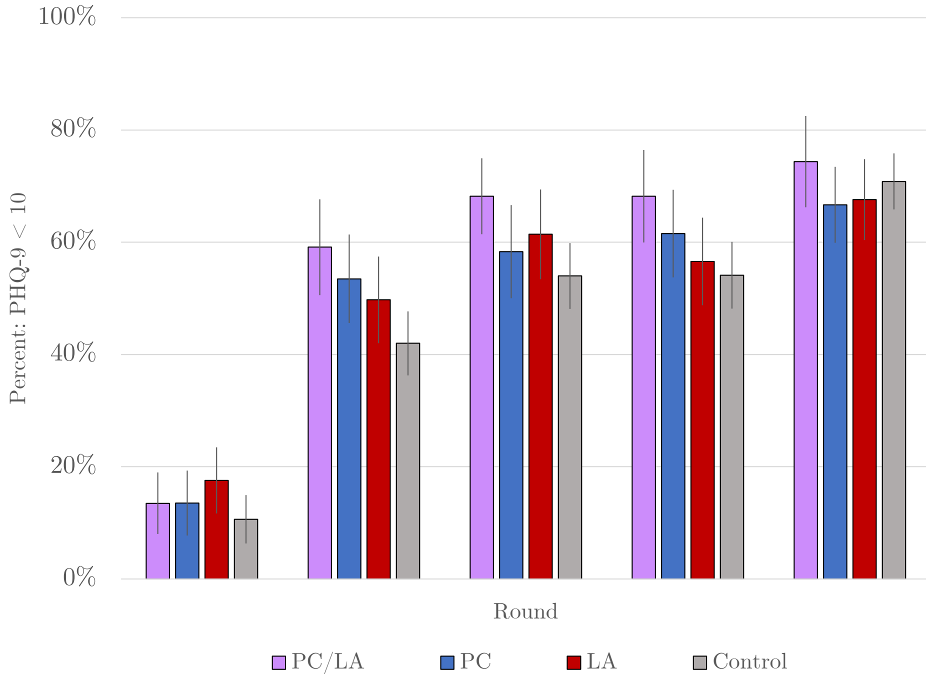
Note: The figure shows standardized impacts for possible pathways through which depression treatment may improve socioeconomic outcomes, as explained in the text. Light bars indicate 95 percent confidence intervals and dark bars indicate 90 percent confidence intervals based on locality-clustered standard errors. All estimates follow the ANCOVA specification of Equation (1), which controls for time indicators, strata indicators, and the baseline dependent variable. Results using the Belloni et al.'s (2014) post-double-selection LASSO method to choose covariates appear in Figure B21. The top of the figure shows impacts during the PC intervention and the bottom of the figure shows impacts after the PC intervention.

A Print Appendix

Table A1: Impact on Depression Indicators

	No Moderate/Severe Depression		No Depression	
	(1)	(2)	(3)	(4)
A: During the PC Intervention				
PC/LA	0.15	0.14	0.095	0.085
	(0.039)	(0.038)	(0.028)	(0.028)
PC	0.071	0.067	0.043	0.041
	(0.039)	(0.037)	(0.028)	(0.026)
LA	0.069	0.052	0.028	0.019
	(0.040)	(0.037)	(0.030)	(0.028)
H_0: $PC/LA = PC$	0.07	0.10	0.10	0.17
H_0: $PC/LA = PC + LA$	0.87	0.73	0.59	0.56
H_0: $PC = LA$	0.97	0.73	0.67	0.48
H_0: $PC/LA = LA$	0.08	0.04	0.05	0.04
H_0: $PC/LA = PC = LA$	0.12	0.11	0.11	0.12
Control mean of outcome	0.48	0.48	0.15	0.15
B: After the PC Intervention				
PC/LA	0.078	0.069	0.12	0.11
	(0.041)	(0.040)	(0.035)	(0.036)
PC	0.0030	0.0085	0.027	0.034
	(0.037)	(0.036)	(0.030)	(0.029)
LA	-0.012	-0.026	0.031	0.024
	(0.037)	(0.036)	(0.031)	(0.029)
H_0: $PC/LA = PC$	0.10	0.17	0.02	0.05
H_0: $PC/LA = PC + LA$	0.14	0.13	0.24	0.28
H_0: $PC = LA$	0.72	0.38	0.91	0.78
H_0: $PC/LA = LA$	0.05	0.03	0.03	0.03
H_0: $PC/LA = PC = LA$	0.12	0.09	0.05	0.07
Control mean of outcome	0.63	0.63	0.22	0.22
Baseline outcome coefficient	0.106	–	–	–
	(0.029)			
Specification	ANCOVA	LASSO	ANCOVA	LASSO
Observations	3476	3476	3476	3476

Note: The table reports AIT effects following Equation (1). Columns 1 and 3 use an ANCOVA specification that controls for time indicators, strata indicators, and the baseline dependent variable. Columns 2 and 4 use the post-double-selection LASSO method to choose covariates (Belloni et al. 2014). Footnote 24 explains this approach in more detail. Locality-clustered standard errors appear in parentheses. "During" and "after" estimates are based on a common regression. The outcome in Columns 1-2 is an indicator for PHQ-9 < 10, which is consistent with no moderate or severe depression. The outcome in Columns 3-4 is an indicator for PHQ-9 < 5, which is consistent with no depression.



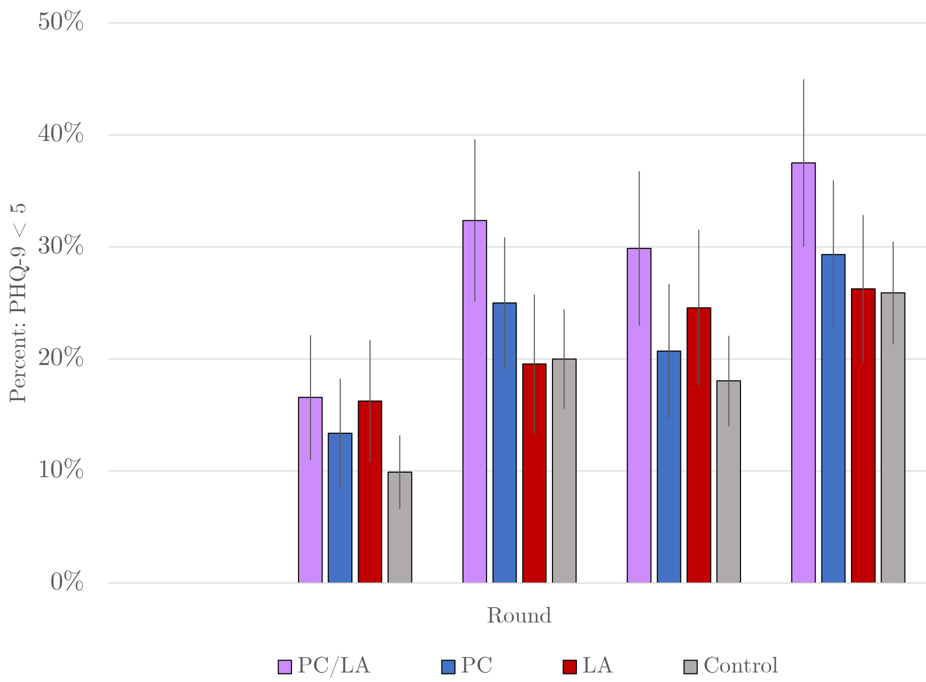


Figure A1: Depression Indicators by Round and Intervention Arm

Note: Panel A shows the percent of participants with PHQ-9 scores below 10, which is consistent with no moderate or severe depression, and Panel B shows the percent of participants with PHQ-9 scores below 5, which is consistent with no depression. All participants have PHQ-9 scores that are greater than 5 in Round 1 because people were required to have initial PHQ-9 scores above 7 to participate in the study. Error bars show 95 percent confidence intervals based on locality-clustered standard errors.

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