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ABSTRACT

The Empowerment Paradox? The Long-Run Impact of a Cycling Program for Girls in Zambia

This study examines the five-year impacts of a bicycle distribution program for adolescent girls in rural Zambia, implemented across 91 schools as part of a randomized controlled trial. While the program increased girls' self-reported empowerment and reduced experiences of domestic and intimate partner violence, it also led to higher rates of early marriage and teenage pregnancy—outcomes that run counter to the program's objectives. We explore mechanisms behind this paradox, including improved socioeconomic status and increased receipt of bride prices, which may reflect girls' higher perceived value in the marriage market. These findings suggest that girls may have exercised greater agency by making strategic decisions about marriage and childbearing. Our results underscore the complex interplay between empowerment, economic mobility, and local norms, and highlight the importance of accounting for potential unintended consequences when designing gender-focused development interventions.

JEL Classification: J12, J13, J16, O10, O15

Keywords: bicycles, female empowerment, marriage, fertility, domestic violence, intimate partner violence, Zambia, RCT

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"Yes, we felt uplifted when we received the bicycle, as though we are the only ones on earth." **Male parent from a treatment school**

1 Introduction

Efforts to empower adolescent girls and women in low- and middle-income countries (LMICs) have led to a range of innovative strategies aimed at improving educational attainment, delaying early marriage, enhancing agency, and promoting economic inclusion. Among these, bicycle distribution programs have emerged as a particularly promising and scalable intervention. By reducing travel time to school and increasing girls' mobility, bicycles address one of the most persistent barriers to girls' education: physical access. Recent evidence underscores their transformative potential. Studies by Fiala et al. (2022), Kjelsrud et al. (2024), and Muralidharan and Prakash (2017) demonstrate that bicycle provision not only improves school attendance and grade completion but also raises girls' aspirations, expands their autonomy, and can positively influence labor market outcomes in the long run. These programs serve as low-cost, symbolic, and functional assets that shift both individual behavior and household dynamics. However, evidence on their long-term impacts—especially on broader life trajectories and in contexts with entrenched gender norms—remains scarce.

To address this gap, we leverage new follow-up data from a cluster randomized controlled trial (RCT) conducted across 100 schools in Zambia's Southern Province. The Bicycle for Education and Empowerment Program (BEEP) distributed bicycles to adolescent girls living at least three kilometers from secondary school to improve educational access. One-year results from Fiala et al. (2022) documented improvements in school attendance, test scores, and self-reported empowerment. Building on this earlier work, we revisit the same communities five years after the intervention—using a *new sampling strategy* to identify eligible girls—and provide one of the first long-term assessments of the durability and multidimensional effects of bicycle-based empowerment programs.

Our findings reveal a nuanced and unexpected pattern of impacts, which we term an "empowerment paradox". On the one hand, girls who received bicycles report significantly higher psychological empowerment: greater perceived control over their lives, stronger aspirations, and increased decision-making power. On the other hand, these subjective gains do not translate into conventionally expected empowerment outcomes. Girls in the treatment group were eight percentage points more likely to have married before age 18 and eleven percentage points more likely to have become pregnant, com-

pared to the control group. We find no significant long-run effects on secondary school attendance, although the introduction of Zambia's universal secondary education policy in 2022, which eliminated school fees, may have muted any relative differences (UNESCO, 2024). Despite this, girls who received bicycles report significantly greater household food security and improved socioeconomic status, along with lower rates of domestic violence and suggestive evidence of reductions in intimate partner violence.

These contrasting results complicate the narrative that empowerment necessarily delays crucial life transitions among adolescent girls. While access to bicycles increased girls' sense of control and improved their material circumstances, it may also have accelerated decisions around marriage and childbearing. Yet, our evidence suggests these life events were not simply outcomes of constraint or disempowerment. Girls in the treatment group were 63% more likely to receive a bride price, reported fewer pregnancies outside of marriage, and were tentatively less likely to be in abusive relationships. These findings point to improved assortative matching in the marriage market and suggest that girls may have exercised greater agency in choosing higher-quality relationships earlier, trading off longer schooling for improved partnership outcomes and economic security.

This paper contributes to three streams of literature. First, it addresses long-standing challenges in the measurement of empowerment. Much of the literature relies on self-reported indicators such as aspirations or self-efficacy (Dhar et al., 2022; Edmonds et al., 2021; Nahar and Mengo, 2022; Charmes and Wieringa, 2003; Buvinic, 2017; Glennerster et al., 2018), which may not align with realized behavioral outcomes. Our study combines both subjective and objective indicators—such as educational attainment, marital and reproductive outcomes—and demonstrates that psychological empowerment may not always translate into the expected life trajectories. These findings call for more multidimensional and context-sensitive approaches to measuring empowerment.

Second, we contribute to the literature on assortative matching in Sub-Saharan African marriage markets. Prior research highlights educational homogamy as a key driver of marriage patterns (Ashraf et al., 2020; Pesando, 2021; Kollamparambil, 2020).[1] Our findings extend this work by showing that economic status and food security—improved through access to bicycles—also influence sorting in the marriage market. Girls in the treatment group appear to have married earlier to secure better matches, consistent with recent findings from Cameroon showing that delaying marriage can reduce

[1]Educational homogamy denotes that husband and wife have the same level of education. Educational hypergamy implies that the wife has less education than her husband, and educational hypogamy denotes that the wife has higher education than her husband (Akem and Wirba, 2024).

the likelihood of achieving wealth- or education-based matches (Akem and Wirba, 2024). These results challenge the normative assumption that later marriage is always more empowering.

Third, we contribute to the broader conceptualization of empowerment by questioning whether standard indicators—such as delayed marriage or continued education—are sufficient to capture agency. Drawing on Amartya Sen's capability approach (Sen, 1979), we argue that empowerment must be understood as context-dependent and individually defined. Girls may prioritize dignity, household well-being, or quality of relationships over formal schooling. In this light, earlier marriage may reflect not constraint but strategic choice—enabled by the mobility, independence, and symbolic capital conferred by a bicycle. Our findings thus call for a more flexible and culturally grounded understanding of what it means to be empowered.

Our paper is structured as follows: Section 2 describes the study setting and intervention. Section 3 outlines the sampling frame, data collection, and validity of the experimental design. Section 4 presents the empirical strategy. Section 5 reports the key findings, and Section 6 concludes with implications for policy.

2 Study Setting and the Intervention

The study took place in three districts of Zambia's Southern Province—Kalomo, Mazabuka, and Monze (see Figure A1)—all located in the Southern Province. This region is characterized by low population density, high poverty, and predominantly rural settlements.[2]

In 2017, World Bicycle Relief (WBR) implemented the "Bicycle for Empowerment and Education Program" (BEEP) in 100 schools across these three districts. The program aimed to reduce travel time and improve school retention rates for girls in grades 5, 6, and 7 who lived more than three kilometers from school, by providing them with bicycles. Using a randomized controlled trial, Fiala et al. (2022) studied the impact of this program on commute time, school attendance, punctuality, and various measures of girls' empowerment.

In this paper, we investigate the long-term effects of the BEEP program on perceived as well as realized economic and social empowerment outcomes for girls five years after implementation. We

[2] According to 2022 data from the Zambia Statistics Agency (Agency, 2022), 51% of households live in extreme poverty, a 13-percentage-point increase from the poverty rate in 2015. ZamStats defines extreme poverty based on the food poverty line, which is equivalent to the cost of a food basket that meets the minimum nutritional requirements of a household with six members.

revisit the same school clusters that were part of the original randomization in Fiala et al. (2022) to recruit girls who received bicycles in 2017 and those who did not. In the original study, 45 schools were randomly assigned to receive the BEEP program.[3] The control group consisted of 55 schools where girls did not receive bicycles.

3 Sampling Frame, Data Collection, and Validity

3.1 Sampling Frame

In this study, we recruited a *new sample* of adolescent girls aged 15 to 19 years from the original three districts. The sample was drawn from communities surrounding the same 100 schools included in the original randomized controlled trial conducted by Fiala et al. (2022). Importantly, we were unable to link data from this study to the original dataset, as the local Institutional Review Board (IRB) did not permit the transfer of personally identifying information.

While Fiala et al. (2022) surveyed 25 girls per school, World Bicycle Relief had distributed an average of 80 bicycles to female students in each of the targeted schools. As a result, we sampled from the broader pool of eligible girls-meaning that, by chance, some of the girls included in this study may also have participated in the earlier survey. Our recruitment strategy was designed to ensure a representative sample of adolescent girls from the relevant age group and communities linked to the original school clusters.

To identify eligible respondents, we first visited the 100 schools sampled in Fiala et al. (2022) and gathered information on the surrounding communities from which students typically commute-specifically those located more than three kilometers from the school. From this list, we randomly selected four communities per school. Enumerator teams were then deployed to these communities to recruit five girls per location, resulting in a final sample of 20 girls per school catchment area, or cluster.[4]

Girls were recruited using a random walk procedure. Enumerators began at a central landmark in

[3] Specifically, there were two treatment arms: (i) in 25 schools, families of beneficiary students paid a small fee (roughly 50 Kwacha ≈ $5) to cover maintenance costs; and (ii) in 20 schools, families were not required to pay any fee. In this study, we do not consider possible differences between these two treatment arms.

[4] The target sample size of 2,000 girls was determined using power calculations conducted in Optimal Design Software (Spybrook et al., 2011). Based on these calculations, we required 100 clusters with an average of 20 girls per cluster to detect a minimum effect size of 0.2 standard deviations with 80% power and a two-tailed p-value below 0.05. The assumed intra-cluster correlation coefficient (ICC) was 0.07, informed by the range of ICC values observed for primary outcomes in Fiala et al. (2022).

each community (e.g., a church or community hall), spun a pencil to determine direction, and then followed that path. They were instructed to visit every third household. Households were screened for eligibility using three criteria: (i) the presence of a girl aged 15–19, (ii) who had attended the school in the catchment area in 2017, and (iii) who had received a bicycle (only for catchment areas corresponding to the 45 treatment schools). If a household did not meet these criteria, enumerators continued to the next eligible household following the random walk procedure, until five eligible girls had been recruited from the community.

3.2 Data Collection

3.2.1 Protocols

Data collection took place between June and December 2022 (see Figure A2). Data were collected via standardized questionnaires administered on tablets with the assistance of an enumerator. The sensitive sections of the survey, such as those on violence experiences or sexual behavior, were programmed as computer-assisted self-interviews (ACASI) to maximize confidentiality and thus decrease possible under-reporting, as in Steinert et al. (2018). The survey was translated from English to Tonga, and the accuracy of translations and semantic context were carefully piloted. Each interview lasted between 60 and 90 minutes.

Enumerators were recruited by the survey firm IPSOS. All enumerators were female, fluent in Tonga, and extensively trained in interview techniques and research ethics. Interviews were held at participants' homes or close to schools (for girls still attending school). Each interview took place in a setting where privacy and confidentiality could be guaranteed. To reduce the interviewing burden and possible fatigue, enumerators encouraged participants to take a 10-minute break after completing half of the interview.

3.2.2 Primary Outcomes

The primary outcomes of the study are:

1. *Educational attainment*, capturing both the total number of school years girls have completed (based on the highest degree completed) and school attendance by counting the number of days missed in the previous week of school;

2. *Early marriage*, defined as any union occurring before the bride has reached the age of 18 years.

3. *Pregnancy*, defined as a dummy variable indicating whether a girl had ever been pregnant or was currently pregnant (coded as 1) or had never been pregnant (coded as 0).

4. *Violence victimization*, constructed as a dummy variable that was coded as 1 if the girl had been exposed to any form of violence in her household (domestic violence) or to any intimate partner violence (for girls who indicated being married or having a boyfriend).

We further conducted analyses disaggregated by specific types of violence, including physical, emotional, or sexual domestic violence and physical, emotional, or sexual intimate partner violence (IPV). Finally, the survey also captured basic socio-demographic information, including household composition, income and employment, asset ownership, and food security. We provide a detailed account of all outcome measures used in Appendix 6.

3.2.3 Secondary Outcomes

The secondary outcomes of the study are:

1. *Girls' empowerment*, which is constructed as an aggregate index that includes sub-indices of bargaining power, decision-making power, aspirations, fertility preferences, locus of control, and prosociality. To create the empowerment index and its sub-indices, the items for each index were aggregated into a continuous scale score through a confirmatory factor analysis.[5]

2. *Income generation*, which was coded as a dummy variable with 1 indicating that the girl engaged in any productive activities and 0 indicating none.

3. *Sexual harassment*, coded as a dummy variable with 1 indicating that the girl reported having experienced whistling or calling names while moving around in a way that made her feel worried or unsafe, and 0 indicating no such experience.

[5]We construct the indices using the following equation:

$$EMP_i = y_i emp_{i1} + ... + y_k emp_{ik} + \delta_i \quad (1)$$

where EMP_i denotes the empowerment scale score, emp_{ik} the respective empowerment indicators for individual i, y_k the weights (factor loadings) for each indicator, and δ_i is a stochastic error term. We employed maximum likelihood estimation with missing values. This aggregation method assumes joint normality and missingness at random so that observations with missing responses on single indicators are not omitted through listwise deletion (Acock, 2013).

3.3 Validity of the Randomization

Due to budget constraints and logistical challenges, we conducted interviews in the catchment areas of 92 out of the original 100 schools. Our sample included all 45 treatment schools from the original intervention conducted by Fiala et al. (2022) and 47 control schools. In total, we interviewed 1,615 adolescent girls—676 from the treatment group and 939 from the control group. The sequence of catchment areas visited on each data collection day was randomly determined. As a result, the final sample of 92 school areas can be considered a random subset of the original 100 schools.

Table A1 presents the baseline characteristics of participating girls and compares the treatment and control groups. The average age was 17 years in the treatment group and 16.5 years in the control group. Over 90 percent of respondents identified as Christian, with the majority belonging to the Tonga tribe. Maternal orphanhood affected 4–5 percent of girls, while paternal orphanhood was more common, affecting 12–13 percent. In terms of parental education, more than 40 percent of mothers and approximately 50 percent of fathers had completed secondary schooling. The average household size was approximately eight members, with about one-third of girls living in polygamous families. Means and standard deviations for these variables across the full sample of 1,615 girls are provided in Table A2.

As shown in Table A1, the treatment and control groups were well balanced across most baseline characteristics except one: girls in the treatment group were, on average, slightly older than those in the control group (Column 3).

4 Empirical Specification

The objective is to estimate the long-term impact of World Bicycle Relief's BEEP program on the specified outcome measures.[6] Specifically, we compare outcomes for girls in the catchment areas of schools that received bicycles to those in the catchment areas of control schools.[7]

We estimate the average effect of being assigned to the treatment group, known as the intent-to-

[6]We adhere to our Pre-Analysis Plan (PAP), which is available and time-stamped at RCT ID: AEARCTR-0011073. While our empirical analysis follows the PAP, we also report additional exploratory analyses to assess outcomes not initially specified, which allows us to gain a better understanding of the potential mechanisms underlying our main findings.

[7]In Fiala et al. (2022), two different treatment arms were analyzed, Payment Arm (T1)and No Payment Arm (T2). The difference between these was that families of students in No Payment Arm (T2) were exempted from paying a fee to cover bicycle maintenance. However, in this study we examine the combined effect of both treatment arms for increased statistical power.

treat effect (ITT), on each outcome variable $Y_{i,s,t=1}$ using the following regression:

$$Y_{i,s,t=1} = \alpha + \beta T_s + e_{i,s} \tag{2}$$

where T_s is an indicator variable for the treatment arm. It equals 1 if school s in the catchment area was assigned to receive the BEEP program (i.e., the bicycle distribution), and 0 otherwise. The term $e_{i,s}$ represents the error term for individual i and school cluster s. Our coefficient of interest is β, which indicates the long-term impact of having received a bicycle. We cluster the standard errors at the school level, as this was the unit of randomization.

To address the issue of multiple hypothesis testing across our four primary outcomes, we implemented a correction for the potential false discovery rate (Fink et al., 2014; Anderson, 2008; Benjamini et al., 2006). We opted for the Benjamini-Hochberg method, which offers a more nuanced approach compared to the conservative Bonferroni adjustments (Benjamini and Hochberg, 1995). We report both the conventional p-values and the sharpened q-values while estimating the primary outcomes.

5 Main Results

5.1 Usage of Bicycles

An essential first step in our analysis is to document the first-stage take-up—the extent to which girls continued to use the bicycles distributed during the original intervention. Among girls residing in the catchment areas of the original treatment schools and who received a bicycle from World Bicycle Relief in 2017, 87 percent reported still using their bicycles five years later. Respondents reported using their bikes for a range of daily activities, most commonly for commuting to school (61 percent), buying groceries or household goods (48 percent), visiting a doctor or health facility (40 percent), and fetching water (31 percent).

Notably, bicycle use extended beyond the primary recipients. A majority of girls reported that other family members also used the bicycles: 54 percent cited use by their fathers, 32 percent by mothers, 42 percent by brothers, and 33 percent by sisters. These patterns highlight the broader household-level utility of the intervention, suggesting that the bicycles functioned not only as mobility tools for adolescent girls, but also as shared assets with potential spillover benefits.

The high rate of continued usage underscores the durability and importance of the intervention,

even several years after the initial distribution of the bicycles. This persistence is important for both cost-effectiveness and impact evaluation: it implies that a one-time in-kind transfer can generate sustained mobility improvements and potentially amplify household welfare. Moreover, the shared use of bicycles suggests that such interventions may indirectly benefit other household members, thereby increasing the overall return on investment. A detailed breakdown of usage patterns is provided in Table A3.

5.2 The Empowerment Paradox in Long-Term Educational and Social Outcomes

We report the long-term impacts of the bicycle intervention on primary outcomes in Table 1. Our findings reveal several important and, in some cases, unexpected patterns.

First, we find no statistically significant differences in educational achievement between girls in treatment and control schools (Columns 1–2, Table 1). Girls in the control group completed an average of 8.4 grades, compared to 8.6 in the treatment group. School absenteeism in the previous week was similarly low: 1.2 days in the control group versus 1.1 days in the treatment group. A likely explanation for these null effects is the roll out of Zambia's universal secondary education policy in January 2022, which eliminated public secondary school fees (UNESCO, 2024). Although the reform has not yet been formally evaluated, it plausibly conferred widespread educational benefits—particularly for girls in the control group—thereby attenuating detectable treatment effects. Similar patterns have been observed in other contexts: in Uganda, the introduction of Universal Secondary Education expanded access for disadvantaged students, complicating the identification of marginal impacts from targeted interventions (Asankha and Takashi, 2011); in Ghana, the Free Senior High School policy improved enrollment but created system-wide shifts that affected the implementation and evaluation of concurrent programs (Osei and Vincent, 2024). This policy-induced convergence may have masked the intervention's impacts on educational attainment.

More surprisingly, we find statistically significant increases in early marriage and teenage pregnancy among girls in treatment schools (Columns 3–4, Table 1). Specifically, 12 percent of girls in the treatment group reported being married before age 18, compared to just 4 percent in the control group. Likewise, 33 percent of girls in the treatment group reported current or prior pregnancy, 11 percentage points higher than the control group (22 percent). These differences are statistically significant and remain robust after applying false discovery rate (FDR) adjustments. These findings contrast with the hypotheses pre-specified in our pre-analysis plan, and they diverge from prevailing assumptions in the

empowerment literature that improved access to education and economic resources delays marriage and childbearing. For example, Bandiera et al. (2020) found that a program in Uganda combining vocational training with sex education significantly reduced early marriage and pregnancy. Similarly, Buchmann et al. (2018) found that a six-month empowerment program in Bangladesh, coupled with financial incentives, reduced underage marriage by 24 percent and early childbearing by 15 percent. In Zambia, Hegdahl et al. (2022) evaluated a program that provided cash transfers, school fee coverage, and community dialogues; the intervention led to greater contraceptive use and lower rates of unwanted pregnancy. In light of this prior evidence, our findings are puzzling and we provide possible explanations for these results in Section 5.4.

We also examine the long-term impacts of the intervention on girls' experiences of violence. Across both treatment and control groups, rates of family and/or intimate partner violence (IPV)—including from boyfriends, husbands, or other household members—were generally high: 72 percent of girls in the control group reported experiencing some form of violence, compared to 67 percent in the treatment group. This five-percentage-point reduction is statistically significant at the 10 percent level but does not survive FDR adjustment. To unpack these patterns, we disaggregated violence outcomes into domestic violence (Table 2) and IPV among partnered girls (Table 3). Among all girls, the program significantly reduced exposure to physical domestic violence by 10 percentage points, emotional violence by 8 percentage points, and neglect by 11 percentage points (Columns 1–3, Table 2); all effects are significant at the 1 percent level. For girls with a current or former partner (N = 812), we find no statistically significant effects on IPV, although all coefficients are negative, suggesting treatment-associated reductions in IPV ranging from 0 to 7 percentage points. The lack of significance may be due to lower statistical power in this subsample.

Our findings on girls' violence experiences align with evidence from a growing body of studies showing that economic empowerment—especially via cash transfers—can reduce IPV. For instance, Mexico's Oportunidades program reduced physical abuse by 5–7 percentage points (Bobonis et al., 2013). Similar results have been found in Togo (Briaux et al., 2020), Peru (Díaz and Saldarriaga, 2022), Kenya (Haushofer et al., 2019), and Ecuador (Hidrobo et al., 2016), with reductions in IPV ranging from 3–8 percentage points. A recent meta-analysis by Eggers Del Campo and Steinert (2022), covering 19 RCTs and over 44,000 participants, concluded that economic empowerment interventions were associated with significant reductions in pooled measures of emotional, sexual, and physical IPV. Taken together, this literature points to the potential of asset-based or economic interventions to mitigate violence against women and girls. Drawing a parallel, our findings suggest that the bicycle

intervention—though not a cash transfer—may have had similar effects by enhancing girls' mobility, visibility, and bargaining power within the household. The observed reductions in domestic violence and tentative declines in IPV indicate that even modest in-kind transfers can reshape intra-household dynamics and promote physical safety for adolescent girls.

5.3 Long-Term Impacts on Girls' Perceived Empowerment

In this section, we present estimates of the bicycle program's impact on multiple dimensions of girls' subjective empowerment. Table 4 reports the intent-to-treat (ITT) effects on pre-specified secondary outcomes, suggesting significant and multifaceted improvements in girls' empowerment.

We find a statistically significant increase of 0.11 *standard deviations* on the aggregate empowerment index among girls in treatment schools (Column 1, Table 4). This corresponds to a 300 percent scale-point increase relative to the control group mean. The composite index aggregates a range of psychological and behavioral indicators, allowing for a holistic evaluation of how the intervention influenced girls' agency and self-perception. These results align with a growing literature suggesting that mobility-enhancing interventions—such as access to bicycles—can shift perceptions of autonomy and future possibilities, even in the absence of direct economic inputs.

Disaggregating the index into sub-components (Columns 2–7) reveals that the observed improvement is largely driven by increases in decision-making power, aspirations for the future, and locus of control. These are domains often linked to internal or perceived empowerment, which may respond more quickly to symbolic or status-enhancing assets like bicycles. The program appears to have strengthened girls' sense of control over their lives, optimism about their future, and ability to influence household-level decisions. These findings are consistent with other empowerment interventions that emphasize psychosocial capital. For instance, Bandiera et al. (2020) show that vocational training coupled with life skills improves girls' aspirations and decision-making in Uganda, and Buchmann et al. (2018) similarly document improvements in forward-looking behaviors through empowerment-focused programs in Bangladesh.

While not statistically significant, girls in treatment schools reported a five-percentage-point lower incidence (32 percent vs. 37 percent; Column 8) of street harassment, which is similar to what Fiala et al. (2022) found. Similarly, we find no detectable effects on girls' participation in income-generating activities (Column 9). This null result may suggest that while the program improved perceptions of empowerment, it did not immediately alter girls' economic behavior–a distinction that reinforces the

importance of distinguishing between internal empowerment and realized outcomes. These findings echo evidence from economic empowerment programs that document stronger effects on agency and aspirations than on labor market participation, especially in constrained rural settings (Peterman et al., 2024).

5.4 Unpacking the Empowerment Paradox

This section explores suggestive evidence on the mechanisms that may explain the long-term impacts of the bicycle intervention, focusing in particular on the surprising increases in teenage pregnancy and early marriage among girls in treatment schools. As shown in Table 1 (Columns 3 and 4), these outcomes diverge from conventional expectations in the empowerment literature, where improved agency and autonomy are typically associated with delayed fertility and marriage (Bandiera et al., 2020; Hegdahl et al., 2022; Fiala et al., 2022). A meta-review by Upadhyay et al. (2014) similarly documents strong negative associations between women's empowerment and fertility rates, unintended pregnancies, and early marriage. Yet, paradoxically, we also observe significant gains in girls' self-perceived empowerment—specifically in their decision-making power, future aspirations, and locus of control (Columns 3, 4, and 6 in Table 4). These findings collectively point to what we term an *Empowerment Paradox*—a pattern in which adolescent girls report higher psychological empowerment but exhibit behavioral outcomes—early marriage and pregnancy—that are typically interpreted as disempowering.

To help unpack this paradox, Table 5 presents treatment effects on additional exploratory outcomes that may shed light on the observed divergence between perceived and realized empowerment.[8] We first find a modest but statistically significant reduction in food insecurity, alongside a substantial improvement in the socioeconomic index (0.78 *standard deviations*), suggesting that the bicycle intervention improved the material well-being of treated girls and their households. Notably, girls in treatment schools were 29 percentage points more likely to report receiving a bride price (Column 3) and 14 percentage points more likely to report that their pregnancy occurred within marriage (Column 4). These findings suggest that, while early transitions into marriage and motherhood did occur, they may have taken place under more favorable and secure conditions—marked by improved household resources, enhanced social legitimacy, and potentially greater partner support.

Viewed through this lens, these outcomes may reflect not disempowerment, but a form of realized

[8] We report results to rule out alternative explanations in Table A4.

empowerment, in which girls leveraged their improved status to make strategic life choices within the constraints of local norms and opportunities. Specifically, the evidence points to upward economic mobility as a plausible mechanism, improving girls' attractiveness in local marriage markets and facilitating better partnership matches. The combined improvements in bride price, household welfare, and tentative reductions in partner violence (Table 3) suggest that treated girls were more likely to enter higher-quality unions. This pattern is consistent with assortative matching dynamics, wherein girls with greater assets or status are better able to attract well-resourced partners. This interpretation echoes findings from Ashraf et al. (2020), who show that in bride-price societies, educational interventions in Indonesia and Zambia led to higher bride prices for more educated daughters—further reinforcing the idea that girls' improved social positioning shaped both the timing and quality of their life transitions.

Moreover, recent work by Akem and Wirba (2024) highlights that delaying marriage can reduce women's chances of marrying up the socioeconomic ladder, especially in settings where early matches with wealthier men are time-sensitive. In this light, the early marriages observed in our treatment group may not reflect diminished empowerment, but rather a strategic choice to secure favorable partnerships while the opportunity exists. The fact that 14 percentage points more of these pregnancies occurred within marriage—alongside tentative reductions in IPV—strengthens the interpretation that these transitions may reflect not coercion or constraint, but increased control and stability.

6 Conclusion

This paper examines the long-term effects of a conditional in-kind program that provided bicycles to adolescent girls in rural Zambia in 2017, focusing on both realized and perceived empowerment outcomes. We find that in-kind asset transfers can have sustained effects. Five years after the intervention, 87 percent of recipients continued to use their bicycles—primarily for commuting to school—suggesting that the asset remained with the intended beneficiaries. This persistence is associated with improvements in food security, household socioeconomic status, self-perceived empowerment, and reductions in family violence. These findings indicate that symbolic and functional assets like bicycles may generate effects comparable to, or greater than, equivalent cash transfers (Skoufias et al., 2013; Cunha, 2014).

The study also documents increases in early marriage and adolescent pregnancy among treated

girls—outcomes traditionally viewed as inconsistent with empowerment. However, these patterns occurred alongside improvements in household economic conditions, a higher likelihood of bride price transfers, and greater incidence of pregnancy within marriage. Taken together, the evidence suggests that treated girls exercised increased agency in navigating life transitions within prevailing social norms, rather than experiencing diminished autonomy. These results underscore the importance of interpreting empowerment through a contextually grounded lens that considers local constraints and cultural expectations.

Our findings contribute to a growing literature that calls for multidimensional frameworks to assess empowerment, consistent with Sen's capability approach. Rather than relying solely on internal beliefs or standardized indicators, we emphasize the importance of measuring the real opportunities individuals have to pursue valued life outcomes—opportunities that are shaped by both institutional structures and social norms.

Finally, this study adds to the limited body of evidence on the long-run impacts of female empowerment programs. As Bouguen et al. (2019), Bandiera et al. (2020), and Dhar et al. (2024) note, most interventions are evaluated only in the short term. By tracking outcomes five years post-intervention, we provide new insights into the sustained behavioral and economic effects of a mobility-enhancing intervention. Overall, our findings have implications for the design and evaluation of gender-focused programs, highlighting the potential of simple, scalable interventions to expand young women's capabilities, while cautioning against universal benchmarks that may obscure complex, locally meaningful forms of empowerment (Ashraf et al., 2020).

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Table 1: Long-Term Treatment Effects on Primary Outcomes

Dependent Variable:	*Educational Outcomes*		*Well-being Outcomes*		
	Highest Grade Completed	Days Missed	Early Marriage	Pregnancy	Violence Victimization
	(1)	(2)	(3)	(4)	(5)
Treatment	0.19	-0.15	0.08***	0.11***	-0.05*
	(0.17)	(0.12)	(0.02)	(0.03)	(0.03)
	[0.27]	[0.20]	[0.00]	[0.01]	[0.15]
Control Group Mean	8.38	1.22	0.04	0.22	0.72
Observations	1259	1258	1615	1608	1615

NOTES: Standard errors clustered at the school level are reported in parentheses. All columns report coefficients from linear probability models. The dependent variables in Columns (1) and (2) are continuous. Column (1) measures the total number of school years completed, based on the highest grade attained. Column (2) captures the number of school days missed in the previous week. Columns (3) to (5) are binary indicators: Column (3) equals 1 if the respondent was married before age 18; Column (4) equals 1 if she is currently or has ever been pregnant; Column (5) equals 1 if she experienced any form of domestic or intimate partner violence. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. P-values adjusted for multiple hypothesis testing using the Benjamini–Hochberg procedure are reported in square brackets.

Table 2: Long-Term Treatment Effects on Domestic Violence

Dependent Variable:	Physical Violence	Emotional Violence	Neglect	Sexual Violence	Any Domestic Violence
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.10***	-0.08***	-0.11***	-0.00	-0.10***
	(0.03)	(0.02)	(0.03)	(0.01)	(0.03)
Control Group Mean	0.40	0.32	0.42	0.02	0.63
Observations	1612	1615	1610	1614	1615

NOTES: Standard errors clustered at the school level are reported in parentheses. All columns present coefficients from linear probability models. The dependent variable in Column (1) is a binary indicator equal to 1 if the respondent reports experiencing any form of physical domestic violence from a household member (e.g., being hit, beaten, or burned). Column (2) captures emotional domestic violence, coded as 1 if the respondent reports being threatened or made to feel ashamed by a household member. Column (3) measures neglect, defined as a binary variable equal to 1 if the respondent reports not being cared for or not having her basic needs met despite the availability of resources. Column (4) captures sexual violence perpetrated by a household member, coded as 1 if the respondent discloses unwanted sexual contact or forced intercourse. Column (5) is an aggregate measure of domestic violence, equal to 1 if the respondent reports experiencing any of the four forms of abuse described above. For a detailed list of items used to define each form of violence, see Appendix 6. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Long-Term Treatment Effects on Intimate Partner Violence (IPV)

Dependent Variable:	Physical Violence	Emotional Violence	Controlling Behaviors	Sexual Violence	Any Form of IPV
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.00	-0.02	-0.05	-0.03	-0.07
	(0.02)	(0.04)	(0.03)	(0.03)	(0.04)
Control Group Mean	0.10	0.25	0.21	0.20	0.48
Observations	810	811	812	808	808

NOTES: Standard errors clustered at the school level are reported in parentheses. All columns present coefficients from linear probability models. The analysis is restricted to the sub-sample of girls who are currently married or in a partnership. The dependent variable in Column (1) is a binary indicator equal to 1 if the respondent reports experiencing any form of physical intimate partner violence (IPV), such as being hit, choked, or threatened with a weapon. Column (2) captures emotional IPV, coded as 1 if the respondent reports being intimidated or humiliated by a partner. Column (3) measures exposure to controlling behaviors, defined as a binary variable equal to 1 if the respondent reports being prevented from seeing family or friends, or from seeking healthcare. Column (4) captures sexual IPV, coded as 1 if the respondent discloses unwanted sexual contact or forced intercourse by her partner. Column (5) is an aggregate IPV measure, equal to 1 if the respondent reports experiencing any of the four forms of abuse described above. For a full list of items used to define each violence type, see Appendix 6. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Long-Term Treatment Effects on Measures of Empowerment

Dependent Variable:	Empowerment								
	Empowerment (Global)	Bargaining	Decision-Making	Aspiration	Fertility	Locus Control	Prosociality	Harassment	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	0.11**	0.01	0.03*	0.10**	0.04	0.09***	0.02	-0.05	0.03
	(0.04)	(0.01)	(0.02)	(0.04)	(0.20)	(0.03)	(0.03)	(0.03)	(0.02)
Control Group Mean	-0.05	-0.00	-0.01	2.10	19.09	-0.04	-0.01	0.37	0.13
Observations	1557	1615	1615	1575	1601	1557	1615	1608	1544

NOTES: Standard errors clustered at the school level are reported in parentheses. All columns present coefficients from linear probability models. Column (1) reports estimates for the overall empowerment index. Columns (2) through (7) report estimates for sub-indices capturing specific dimensions of empowerment: bargaining power (Column 2), decision-making (Column 3), aspirations (Column 4), fertility preferences (Column 5), locus of control (Column 6), and pro-sociality (Column 7). For a full list of items used to construct each sub-index, see Appendix 6. The indices were constructed using confirmatory factor analysis. Column (8) reports effects on experiences of sexual harassment, measured as a binary variable equal to 1 if the respondent reported being whistled at or called names in public in a way that made her feel worried or unsafe. Column (9) presents impacts on girls' engagement in income-generating activities, measured as a binary variable equal to 1 if the respondent reported participating in any productive work. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Exploratory Outcomes

Dependent Variable:	Food Insecurity	Socioeconomic Index	Bride Price	Pregnancy in Marriage
	(1)	(2)	(3)	(4)
Treatment	-0.06**	0.78***	0.29**	0.14***
	(0.03)	(0.12)	(0.12)	(0.05)
Control Group Mean	0.23	-0.33	0.46	0.14
Observations	1615	1615	117	442

NOTES: Standard errors clustered at the school level are reported in parentheses. All columns present coefficients from linear probability models. Column (1) reports estimates for food insecurity, defined as a binary variable equal to 1 if the respondent reported insufficient money or resources for food in the past month. Column (2) presents effects on a socioeconomic index, constructed using principal component analysis of household asset ownership and living standard indicators (e.g., access to electricity). Column (3) shows effects on bride price, a binary variable equal to 1 if the respondent reported that a bride price was paid upon her marriage. Column (4) reports estimates for pregnancy within marriage, defined as a binary variable equal to 1 if the respondent is currently married and either currently pregnant or has ever been pregnant. Estimates in Columns (3) and (4) are restricted to the sub-sample of married girls. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Tables

Table A1: Balance Table: Sample Characteristics

	Control Group	Treatment Group	Difference
	(1)	(2)	(3)
Age	16.496	17.022	0.526***
	(1.429)	(1.340)	(0.070)
Christian	0.941	0.945	0.004
	(0.235)	(0.228)	(0.012)
Tribe: Tonga	0.923	0.942	0.019
	(0.266)	(0.233)	(0.013)
Tribe: Lozi	0.020	0.024	0.003
	(0.141)	(0.152)	(0.007)
Tribe: Bemba	0.011	0.012	0.001
	(0.103)	(0.108)	(0.005)
Mother no longer alive	0.049	0.041	-0.008
	(0.216)	(0.199)	(0.011)
Father no longer alive	0.128	0.121	-0.006
	(0.334)	(0.327)	(0.017)
Mother completed secondary schooling	0.413	0.423	0.010
	(0.493)	(0.494)	(0.025)
Father completed secondary schooling	0.471	0.469	-0.002
	(0.499)	(0.499)	(0.025)
Number of female adults living in house	1.817	1.814	-0.003
	(1.301)	(1.341)	(0.066)
Number of male adults living in house	1.515	1.555	0.039
	(1.200)	(1.298)	(0.063)
Number of girls living in house	2.195	2.157	-0.038
	(1.713)	(1.848)	(0.089)
Number of boys living in house	2.261	2.246	-0.015
	(1.713)	(1.769)	(0.088)
Polygamous family	0.302	0.333	0.031
	(0.459)	(0.472)	(0.025)
Observations	939	676	1,615

NOTES: Descriptive statistics for girls in the estimation sample, presented separately for the treatment and control groups. Standard errors, clustered at the school level, are shown in parentheses. Column (3) reports the difference in means between the two groups. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Sample Characteristics

Variable	Mean	SD
Age (years)	16.72	1.42
Engaged in income-generating activity	0.04	0.21
Household assets index (0–10)	4.66	2.11
Household size	7.78	4.15
Polygamous household	0.29	0.45
Mother completed secondary school	0.34	0.48
Father completed secondary school	0.39	0.49
Observations	1,615	

NOTES: This table reports summary statistics for girls in the estimation sample. Means and standard deviations are shown for each variable. Binary variables are coded as 0/1.

Table A3: Bicycle Usage in Treatment Group

Usage	n (%)
Still using the bike	589 (87%)
Purpose of using the bike	
Going to school	412 (61%)
Going to buy groceries/goods	324 (48%)
Going to see a doctor	270 (40%)
Going to fetch water	210 (31%)
Usage by several household members	
By father	365 (54%)
By mother	216 (32%)
By brother	284 (42%)
By sister	223 (33%)
Observations	676

NOTES: Table reports number and percent of treatment group participants self-reporting usage of bicycles, purposes of usage, and usage by other household members.

Table A4: Exploring Alternative Mechanisms

Dependent variable:	**Girl's Age at Marriage**	**Husband's Age at Marriage**	**Sexual Partners (Past Year)**
	(1)	(2)	(3)
Treatment	0.14	-0.05	-0.07
	(0.22)	(0.84)	(0.09)
Observations	117	112	803
R-squared	0.00	0.00	0.00
Control group mean	16.97	20.72	1.30

NOTES: Standard errors clustered at the school level are reported in parentheses. All columns report coefficients from linear regression models. The dependent variables in columns (1) and (2) represent age (in years) at first marriage for girls and their husbands, respectively. Column (3) reports the number of sexual partners in the past year as reported by the respondent. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendix: Outcome Measures

Overview of outcome measures (Pre-Specified)

1. Educational achievement
    - *Years of education completed*: What is the highest level of education that you have completed?
    - *School attendance*: In the last week (when schools were open), how many days did you miss school entirely?
2. Early marriage
    - Are you currently married/widowed/divorced?
3. Pregnancy
    - Have you ever been/are you currently pregnant?
4. Violence
    - Family Violence
        - In the past year, how often did your parent/another adult living with you make you feel ashamed in front of other people? (*emotional violence*)
        - ... say that they wished you were dead or had never been born? (*emotional violence*)
        - ... threaten to leave you forever or abandon you? (*emotional violence*)
        - ... hit, beat or spank you with a hand? (*physical violence*)
        - ... hit, beat or spank you with an object? (*physical violence*)
        - ... pull your hair? (*physical violence*)
        - ... burn or scald or brand you (for example with a cigarette butt) (*physical violence*)
        - In the past year, how often did you not get enough to eat or drink even though there was enough for everyone? (*neglect*)
        - ... did you not feel cared for by your parent/another adult living with you? (*neglect*)
        - In the past year, how often did someone make you look at their private parts or looked at yours? (*sexual violence*)
        - ... touch your private parts or made you touch theirs? (*sexual violence*)
        - ... try to have sex with you when you didn't want to but didn't succeed? (*sexual violence*)

  - ... have sex with you when you did not want to? (*sexual violence*)

- Intimate Partner Violence (IPV)
  - In the past year, how often did your husband/your boyfriend punch, slap, kick, whip, lash, or poke you with an object? (*physical IPV*)
  - ... choke, smother, try to drown you, or burn you intentionally? (*physical IPV*)
  - ... threaten you with a knife, screwdriver, cricket bat, gun or any other weapon? (*physical IPV*)
  - ... try to keep you from seeing friends? (*partner control*)
  - ... restrict contact with your family of birth?(*partner control*)
  - ... expect you to ask permission before seeking health care for yourself? (*partner control*)
  - ... insult you, humiliate you, or made you feel bad about yourself? (*emotional IPV*)
  - ... do things to scare or intimidate you on purpose, e.g. by the way he looked at you, by yelling or smashing things? (*emotional IPV*)
  - ... tell you that he did not love you or you do not deserve him or that he regrets ever meeting you? (*emotional IPV*)
  - ... try to have sex with you when you didn't want to but didn't succeed? (*sexual IPV*)
  - ... have sex with you when you did not want to? (*sexual IPV*)

5. Female Empowerment

- Bargaining index
  - Do you ever have small money of your own (K2 or K5) to use as you would like? This could be money you have earned or that you get from a family member.
  - Can you decide on what to spend it on your own?
  - Each year there are new fashions (e.g. hair pins) that come out. If you wanted to buy something new and had the money to do so, do you think your parents would allow you?
  - Do you feel you can to talk to your parents about what you want to be when you grow up?
  - Do you think you can talk to your parents if you have problems with relatives, friends or at school?
  - Do you feel you can talk to your parents about when you wish to get married?

- Decision-making index
  - Do you think your parents (or guardian) think about your own best interest when making decisions?
  - How often are you able to make each of the following decision fully on your own without an adult intervening?
    (a) What clothes to wear when you are not in school/working
    (b) What to do in your free time
    (c) What to eat when you are not at home
    (d) How much education you will get
    (e) Who you can be friends with
    (f) Decide when to marry of your own free will
    (g) Decide who to marry on your own
- Aspirations index
  - When you finish at school, what would you like to do?
    * Continue with education until end of secondary
    * Continue with education and go to college
    * Get an income-generating job
    * *Stay and work at home* (coded as 0)
    * *Get married* (coded as 0)
- Locus of control index
  - Let's say that one day when you are going to school you cannot find a path because of heavy rain or because a tree fell. This is a difficult situation because it is the only way to school and you are already late to school. In situations like this one or other ones similar to this one, you can usually find your way out?
  - How much control do you think you have over how much food you have to eat?
  - ... when you get married?
  - ... how many children you will have?
  - ... how well you perform in school?
  - ... what work you will do in the future?
  - ... how many household chores you have to do?

  - There are many things that can happen to you in life. Some of them will be good and some will be not so good. Do you feel you can control what happens to you in life?
  - In general, would you say you are satisfied with your life?
  - I feel my life will improve in the future
- Prosociality index
  - My parents, guardians/husband ask for my opinion on things and listen when I share my opinion
  - My friends ask me for advice when they have a problem
  - If I see something wrong in school or the neighborhood, I feel I can tell someone and they will listen
  - I can speak up in class when I have a comment or question
  - I can speak up when I see someone else getting hurt
  - I can ask my parents, guardians, husband or relatives for help when I need it

6. Harassment
   - How frequently have you been teased, whistled or called names while going to school/moving around in a way that made you feel worried or unsafe?
7. Income-generation
   - Have you done any income-generating work in the past year? For example work on farm, or in a factory, or in a shop.

Overview of Measures (Pre-Specified)

1. Food Insecurity
   - In the past month, was there a time when you were hungry but did not eat because there was not enough money or other resources for food?
2. Socioeconomic Status
   - Please tick every item in the list that you and your family have at home (*pca-weighted index*)
     - Car
     - Motorbike

- TV
- Radio
- Mobile phone
- Refrigerator
- Goats
- Cattle
- Pigs
- Sheep
- Toilet in the house
- Drinking tap water
- Electricity in the house

3. Bride price

    - Was a bride price paid for this marriage?

4. Pregnancy in Marriage

    - Coded 1 if girl is both currently married and has been/currently is pregnant

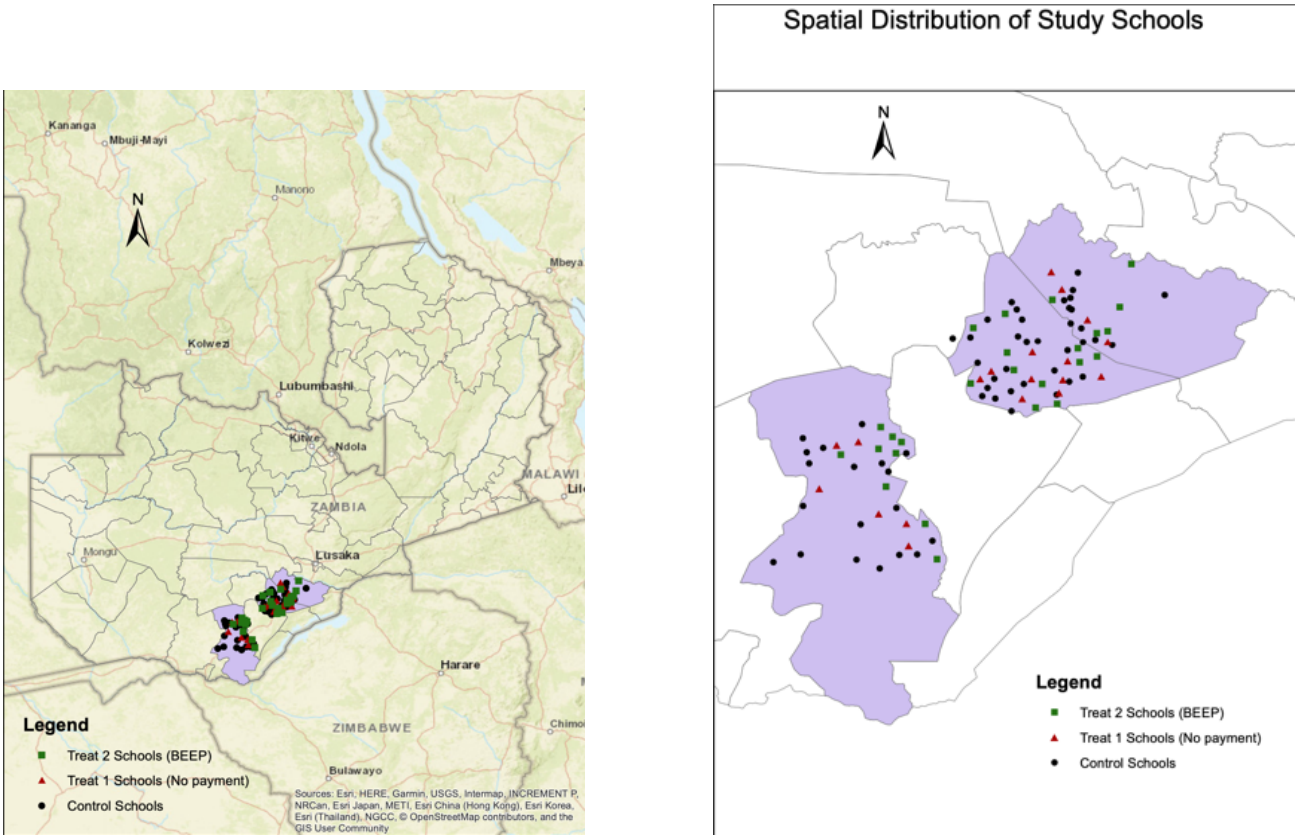

Figure A1: Distribution of schools

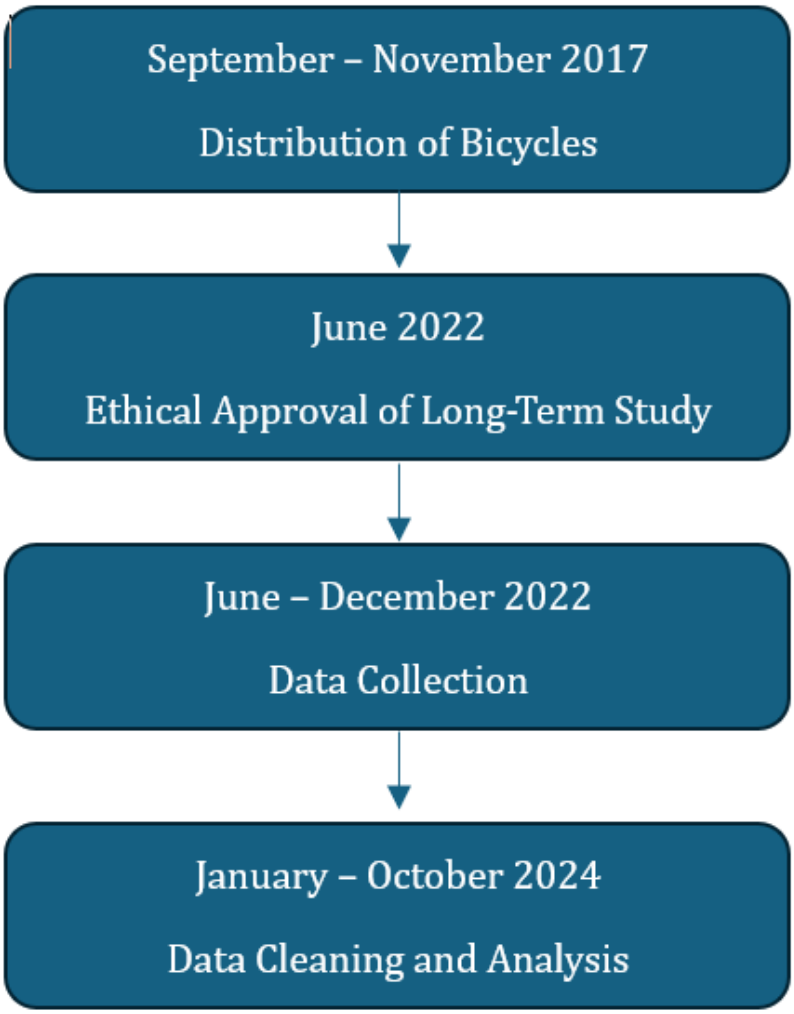

Figure A2: Timeline of the study