The Labor-Market Costs of Dementia for Patients and Families: Evidence from Colombia*

Job Market Paper

Click here for the most recent version

Alexa Grunwaldt†
Yale University

Andrea Franco Hernandez
University of Wisconsin–Madison

Christian Posso
Banco de la República de Colombia

November 2026

Abstract

Serious illness can reduce a person's ability to work while raising the household's need for care. Other household members may respond by working less to provide care or by working more to replace lost income. We study these competing responses after a first recorded diagnosis of Alzheimer's disease or a related dementia (ADRD) in Colombia. We link administrative health records to monthly formal-employment records and household rosters. Using a stacked difference-in-differences event study, we compare newly diagnosed patients and their households with similar people and households that never receive a diagnosis. Two years after diagnosis, patients' formal employment is about 10 percentage points lower for women and 14.8 points lower for men. Their quarterly earnings are about $100 lower for women and $217 lower for men (2007 USD). We find no evidence that other household members increase formal work to offset these losses. Instead, the share of other members with formal jobs is about 5 percentage points lower, and their formal earnings fall as well. Because patients' work may decline as symptoms emerge before diagnosis, we interpret their estimates as the decline surrounding diagnosis rather than the effect of disease onset. For other household members, pre-diagnosis estimates are close to zero, and we interpret their declines as effects of a relative's diagnosis.

*The findings, interpretations, and conclusions expressed in this paper are solely those of the authors and do not necessarily reflect the views of the Banco de la República de Colombia and its Board of Directors.

†Department of Economics, Yale University. Email: alexa.grunwaldt@yale.edu. I am grateful to my advisors, Orazio Attanasio, Cormac O'Dea, and Costas Meghir, for their guidance and advice.

1 Introduction

A serious illness rarely affects only the person who is sick. It can push the patient out of work and pull other household members into caregiving. Relatives may then work less to provide care, or work more to replace the patient's lost income. Which response dominates determines whether the family cushions the shock or compounds it. Answering that question requires observing the patient and the rest of the household before and after the illness, in data that do not rely on recall or self-report.

A large literature studies these responses in Europe and the United States, and a central lesson is that family labor supply works as a form of insurance. In Denmark, for example, spouses work more after a partner's death, which brings large income losses, but not after severe nonfatal health shocks, which public programs insure well (Fadlon and Nielsen 2021). This lesson comes mainly from countries with generous disability insurance and formal long-term care. Yet most people with dementia live in low- and middle-income countries, where neither is widely available and where families provide most of the care (World Health Organization 2021). In these settings, both pressures created by illness are at their strongest. Because income losses are less insured, families have more reason to work more. Because care is scarce outside the home, they also have more reason to work less. Which force dominates is an open empirical question, and evidence from high-income countries cannot answer it.

In this paper, we study household labor-market responses to a first recorded diagnosis of Alzheimer's disease or a related dementia (ADRD) in Colombia. We link administrative health records to monthly formal-employment records and to household rosters. This lets us follow patients and the people they live with before and after diagnosis. We find that, in the formal labor market, families do not cushion the shock. Patients' formal employment and earnings fall steadily after diagnosis, and the formal employment and earnings of other household members fall as well. Labor-market losses extend from patients to their families.

ADRD is a particularly consequential case. Cognitive decline makes it difficult to keep working, and because the disease is progressive, the need for supervision and care grows over

time rather than fading. Colombia is an informative setting for studying how households adjust. Formal long-term care is limited, which leaves families with few alternatives to providing care themselves. Intergenerational cohabitation is common, so the most likely caregivers often live in the same household as the patient. And a large informal sector gives households a way to adjust that payroll records do not capture.

We combine three data sources. The Registro Individual de Prestación de Servicios de Salud (RIPS) records medical encounters and identifies each patient's first observed ADRD diagnosis. The Planilla Integrada de Liquidación de Aportes (PILA) records monthly formal employment and earnings, which we observe from 2008 through 2020. The second and third waves of SISBEN identify who lives in each household and how members are related. With these data we analyze two complementary samples: working-age patients, and working-age members of households that contain a patient. The two samples contain different patients, so their estimates are not an accounting of losses within the same households. Together, however, they show whether labor-market losses stop with the patient or reach the family.

The raw data show the event clearly. Health care use rises around the diagnosis date, and patients' formal employment and earnings then diverge from those of people who are never diagnosed. But the two groups also differ before diagnosis, so a simple comparison would mix the consequences of diagnosis with preexisting differences.

We therefore use a risk-set matched, stacked difference-in-differences design. For each calendar quarter in which first diagnoses occur, we form a risk set of newly diagnosed units and eligible never-diagnosed units observed in that quarter. We estimate propensity scores from characteristics measured before diagnosis and, within each risk set, match each diagnosed unit to its ten nearest never-diagnosed neighbors (three in the household analysis). We then align each matched group on the diagnosis date, stack the groups, and estimate the event study with risk-set-by-calendar-quarter fixed effects, so that diagnosed units are compared only with never-diagnosed units from the same risk set in the same quarter. As a result, diagnosed and comparison units face the same calendar-time conditions. Each never-

diagnosed unit also takes its event date from the patient it is matched to, rather than from an arbitrary assignment.

The losses for patients are large, and they more than double between the first and second years after diagnosis. Four quarters after diagnosis, patients' formal employment is about 4.1 percentage points lower for women and 5.4 points lower for men relative to matched comparisons. Their quarterly earnings are about $49 lower for women and $90 lower for men (2007 USD). By eight quarters, formal employment is about 10 percentage points lower for women and 14.8 points lower for men. Quarterly earnings are about $100 lower for women and $217 lower for men.

Other household members do not offset these losses by working more in the formal sector. Instead, their own formal employment falls. The share of other members with formal jobs is about 4 percentage points lower four quarters after diagnosis and 5 points lower after eight quarters. Formal employment and quarterly formal wages decline for both female and male members. This pattern is consistent with caregiving demands adding to the patient's losses rather than offsetting them.

The similarity between women and men stands out. In several high-income countries, the labor-market cost of caring for a sick relative falls disproportionately on daughters and wives. In Colombia, female and male household members respond similarly.

Two issues shape how we interpret these results. First, symptoms may affect patients' work before the first recorded diagnosis, and some of their pre-diagnosis coefficients differ from zero relative to the omitted quarter. We therefore interpret the patient estimates as the deterioration in labor-market outcomes surrounding diagnosis, rather than as the effect of disease onset. For other household members, the pre-diagnosis coefficients are close to zero, and we interpret the household estimates as causal effects of a relative's recorded diagnosis, compared with matched households in which no one is diagnosed. Second, our data cover formal employment only. They do not measure caregiving time, and they cannot distinguish a family member who leaves the labor force from one who moves into informal work.

This paper makes three contributions. First, it extends research on family labor supply responses to health shocks to a setting where both formal insurance and formal care are limited. In high-income countries, a muted family response to nonfatal shocks has been read as evidence that households are well insured. Our setting shows why this inference requires care. When an illness raises the need for care, family members may work less even when the household's income loss is poorly insured. A small or negative family labor supply response therefore does not, by itself, show that a household is insured. This matters for approaches that use family labor supply to measure the value of social insurance. Second, we measure both patients' losses and family spillovers with linked administrative records in a middle-income country, where such data are rare and where much of the evidence on caregiving and work relies on household surveys. This is a concern given evidence that household surveys increasingly misreport income and program receipt (Meyer et al. 2015). Our data also show that, unlike in several high-income countries, the formal labor-market burden falls similarly on women and men. Third, the quarter-specific matching design makes the comparison date and comparison population explicit when untreated units have no naturally observed event date.

The results also bear on policy debates that are active in Colombia and across Latin America. Law 2281 of 2023 created a National Care System, but the Constitutional Court struck the law down because the required fiscal-impact analysis was missing (Ruling C-161 of 2024), and it lost effect when the 2025–2026 legislative session ended in June 2026. The government adopted a National Care Policy in February 2025 (CONPES 4143), and bills filed in Congress in 2026 would create a National Care System by statute. Recent laws also give formal workers who care for relatives the right to request flexible schedules or remote work (Law 2297 of 2023, as amended by Law 2466 of 2025). Our estimates speak to these policies in three ways. First, they measure the formal labor-market losses that caregiver protections are meant to prevent. Because our sample predates these policies, the estimates provide a baseline for evaluating the caregiver protections now in force and any care system that

Congress adopts. Second, the earnings that family members lose are part of the return to public care services such as day centers and respite care, which allow family members to keep working. Our estimates are therefore an input into the cost-benefit analysis of these services. Third, in Colombia a formal job also carries pension contributions and contributory health coverage, so the losses we document extend beyond current earnings. Because diagnosis is recorded in administrative health data, support could begin at diagnosis rather than after household income has already fallen. SISBEN, Colombia's main targeting instrument for social programs, is one channel through which this could happen.

The remainder of the paper proceeds as follows. Section 2 discusses the related literature. Section 3 describes the data, samples, and outcomes, along with patients' health-care use around diagnosis. Section 4 presents the matched, stacked difference-in-differences design. Section 5 reports results for patients and households. Section 6 discusses mechanisms and policy implications, and Section 7 concludes.

2 Related Literature

2.1 Informal care and household labor supply

The starting point for this paper is the household allocation of time between market work and unpaid production. Care for an older or disabled relative is economically valuable household production, but it can require family members to reduce hours, accept more flexible work, or leave employment altogether. At the same time, a patient's earnings loss and medical expenses can create an offsetting income effect: other family members may work more to replace lost resources. Even when a shock creates no care needs, this added-worker response tends to be modest: when husbands lose their jobs in Austria, wives' labor supply rises only moderately and replaces a very small share of lost household income, with the presence of children shaping the response (Halla et al. 2020). The sign of the household labor-supply response is therefore theoretically ambiguous. It depends on the intensity and duration of

care, the availability of formal substitutes, household composition, and the flexibility of local labor markets (Aguiar et al. 2012).

An extensive empirical literature documents labor-market costs of informal care. Using SHARE data, Bolin et al. (2008) find that providing care to older relatives reduces hours worked. Van Houtven et al. (2013) find that caregiving reduces employment among men who provide personal care, makes women more likely to retire, and lowers the hours and wages of women who keep working, while Jönsson (2022) and Korfhage and Fischer-Weckemann (2024) document substantial employment and earnings costs across European settings. Several studies emphasize daughters' responses to parental care needs (Crespo and Mira 2014; Maestas et al. 2024; Ramirez Lizardi et al. 2025). These findings support a time-constraint mechanism, but their magnitude varies considerably across family structures and institutional settings. Care responsibilities can also have lasting career costs. Adda et al. (2017) show that caregiving costs following childbirth arise from skill loss during employment interruptions, forgone earnings opportunities, and selection into occupations that are easier to combine with family responsibilities.

Policy variation in the availability of formal care provides especially informative evidence on substitution between family care and market work. Using a Norwegian reform that expanded publicly funded home care, Løken et al. (2017) find that formal care increases the earnings of only daughters and reduces their long sickness absences, with little effect on sons or daughters with siblings. The result suggests that siblings and public services can substitute for an individual's caregiving time. Consistent with this broader point, Mommaerts and Truskinovsky (2020) show that care supplied by adult children and spouses responds differently to aggregate labor-market conditions. The opportunity cost of time and the presence of alternative caregivers shape who adjusts employment when care needs rise.

Chandra et al. (2023) review the economics of ADRD. Dementia is especially care-intensive: in the United States, Langa et al. (2001) estimate that older adults with mild, moderate, and severe dementia receive about 8.5, 17.4, and 41.5 more hours of informal care

per week than those with normal cognition, an additional cost of more than $18 billion a year. Evidence on how dementia affects family members' employment is more limited and does not point uniformly toward large labor-market effects. Using Danish administrative data, Altındağ et al. (2026) find limited employment responses among adult children of dementia patients, although their mental-health-care use changes. Caregiving can also harm caregivers' own health: Coe and Van Houtven (2009) find that continued care for an elderly parent raises depressive symptoms and lowers self-rated health among married caregivers. Denmark combines extensive public services with a labor market and household structure quite different from Colombia's. A small or zero response in such a setting need not imply that dementia has little effect where long-term-care substitutes are scarce.

Evidence from middle-income countries is particularly thin. van Gameren and Velandia Naranjo (2015) find that self-reported caregiving demands reduce labor-force participation among older women in Mexico. Where public old-age insurance is limited, families are a central source of support for older members. In Malaysia, which had neither Social Security nor Medicare, Lillard and Willis (1997) find that adult children's transfers to parents are consistent with repayment of parents' investments in their education, are larger for widowed parents and parents in poor health, and are exchanged for parents' time spent on child-care and housework. Colombia is a useful setting because intergenerational cohabitation is common, access to formal long-term care is limited, and informal work is an important adjustment margin. These features can amplify the family's care burden, but they can also make formal payroll records an incomplete measure of total labor supply. A household member who disappears from PILA may have stopped working, moved to informal work, or shifted toward intermittent employment that better accommodates care.

2.2 Cognitive decline and patients' economic outcomes

A second literature studies how cognition affects patients' own economic behavior. Cognitive ability predicts financial decision-making, portfolio allocation, and who manages household

finances. Agarwal and Mazumder (2013) show that individuals with stronger cognitive skills make fewer costly financial mistakes, and Christelis et al. (2010) link cognition to stock-market participation. Smith et al. (2010) find that numeracy affects family financial outcomes, especially for the spouse who makes financial decisions, and helps determine which spouse takes that role. Angrisani and Lee (2019) show that declines in the cognition of a household's financial decision-maker are associated with reductions in household wealth. Hsu and Willis (2013) find that as the financial decision-maker's cognition declines, older couples eventually shift financial management to the other spouse, often well after money-handling difficulties begin, and that couples with the most at stake are much more likely to make the switch after a memory-disease diagnosis. A diagnosis can therefore prompt a reallocation of responsibilities within the household. These findings imply that the economic consequences of ADRD can begin before severe disability or institutionalization.

Genetic-risk studies provide related evidence that vulnerability to Alzheimer's disease is associated with economic outcomes well before a recorded diagnosis. Jeong et al. (2024) find that higher genetic risk predicts lower employment, earnings, and household wealth as well as less long-term-care planning. Shin et al. (2019) relate Alzheimer's genetic risk to portfolio composition. Awareness is also consequential: Mazzonna and Peracchi (2024) find larger wealth declines among adults who do not recognize their cognitive deterioration. The relationship also runs in the other direction. Using cross-country differences in pension, tax, and disability policies, Rohwedder and Willis (2010) find that early retirement substantially lowers the cognitive performance of people in their early sixties. Together, this literature makes clear that cognition and economic behavior evolve jointly, complicating any design that treats the clinical diagnosis date as the biological onset of disease.

Closely related evidence comes from Bonilla Mejía et al. (2025), who link Colombian RIPS health records to PILA employment records. They document employment and wage gaps emerging several years before dementia diagnosis, particularly among working-age adults, and also show post-diagnosis trajectories. Their results emphasize the importance of pre-

diagnosis deterioration when interpreting diagnosis-centered comparisons.

Against this background, our patient analysis characterizes formal employment and earnings around the first recorded ADRD diagnosis among working-age individuals with at least some formal employment in the two years around diagnosis. We use contemporaneous risk-set matching on demographic and pre-period labor-market characteristics before estimating quarterly event-study profiles. This comparison helps account for observable differences in labor-market attachment and calendar-time conditions.

2.3 Health shocks, diagnosis, and social insurance

The paper also relates to research on the labor-market consequences of adverse health events. Lenhart (2019) documents persistent earnings losses following sudden health declines, and Conti et al. (2025) find lasting employment and earnings effects following menopause diagnoses. Other work shows that health shocks can affect outcomes outside employment when insurance is incomplete (Andersen et al. 2026). Spouses' labor supply is also used to value public insurance against health-related income losses. Haller et al. (2024) find that wives earn more when their husbands enter disability insurance under stricter eligibility rules and use this response to bound the insurance losses from tightening eligibility. Benefit rules also shape the work of people with health limitations. In Hungary, a lower earnings limit for disability-insurance recipients substantially reduced their labor supply, with little effect on who entered the program (Krekó et al. 2024). This literature emphasizes that an illness affects not only medical spending but also household income, benefit receipt, and the allocation of work across family members.

ADRD raises an unusually difficult timing problem within this literature. Symptoms are progressive, family members may adapt before a diagnosis is recorded, and contact with the health-care system is itself endogenous. Evidence of pre-diagnosis losses in Colombia reinforces this concern (Bonilla Mejía et al. 2025). Self-reported caregiving measures introduce an additional measurement problem because reported provision and receipt of care need not

coincide (Giovannetti and Wolff 2010). Our administrative health records address part of this problem by identifying the first date on which ADRD is recorded and by showing how medical use evolves around it.

2.4 Contribution and empirical approach

This paper connects these literatures by studying patients' formal labor-market outcomes and the responses of other household members around a first recorded ADRD diagnosis. Our main contribution relative to Bonilla Mejía et al. (2025) is to extend the analysis from diagnosed individuals to their co-resident household members. Linking SISBEN household rosters to RIPS and PILA allows us to examine whether other working-age members increase formal work to replace lost income or experience employment and earnings declines of their own. We study patients and household members in complementary samples, so these estimates do not constitute an accounting of total losses within the same families.

This household perspective is particularly relevant in a setting with widespread intergenerational cohabitation, limited formal long-term care, and substantial informal employment. It allows us to assess whether relatives' formal labor-market responses offset or compound patients' losses. Because we do not directly observe caregiving time or informal work, we interpret these responses as evidence about the distribution of formal labor-market losses within families, rather than as a direct estimate of the effect of providing care.

Our empirical approach is related to matched event-study designs that construct comparison groups at the time of an adverse event. We follow the logic of Adams-Prassl et al. (2023), who match treated units to comparable controls before estimating dynamic differences in outcomes. We repeatedly evaluate comparison units' eligibility and pre-period characteristics in the calendar quarters in which diagnoses occur, construct contemporaneous matched risk sets, and then stack those risk sets for estimation. This implementation makes the comparison population and calendar-time counterfactual explicit. Matching improves comparability on observed characteristics, but the interpretation of the event-study

estimates still depends on the assumptions discussed in the empirical-design section and on the distinction between recorded diagnosis and disease onset.

3 Data and Analysis Samples

3.1 Health records and health-care use around diagnosis

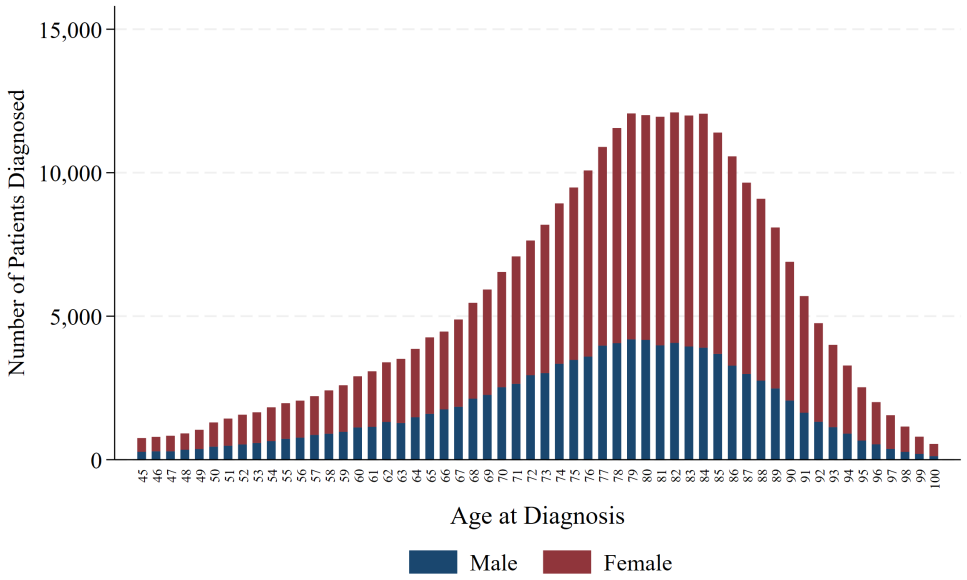
RIPS contains records of general-practitioner visits, emergency-room visits, hospitalizations, procedures, and diagnoses in Colombia from 2010 through 2021. Each record includes an individual identifier that can be linked to PILA, along with age, sex, diagnosis codes, and the date and type of encounter. We define a patient's diagnosis date as the first observed RIPS encounter containing an Alzheimer's disease or related dementia diagnosis. The event is therefore the first *recorded* diagnosis in these data rather than the biological onset of disease.

Figure 1 shows the age and sex distribution of patients at their first recorded ADRD diagnosis. Diagnoses rise steeply with age and plateau at about 12,000 patients per year of age between ages 79 and 84. About two-thirds of diagnosed patients are women, and the female share is highest at the oldest ages. Fewer than one in ten patients diagnosed at age 45 or older are diagnosed by age 60, so the working-age patients in our labor-market analysis are a small, younger part of the diagnosed population.

For the descriptive health analysis, we follow diagnosed patients around their first recorded diagnosis and a random sample of individuals who are never observed with an ADRD diagnosis in RIPS. As in the labor-market analyses, we keep patients first diagnosed in 2013 or later, so every patient has at least three years of earlier RIPS records in which a prior diagnosis would appear. We aggregate health-care use to the semester level and measure general-practitioner visits, emergency-room visits, hospitalizations, and procedures. The figures follow these outcomes from two years before through two years after the actual or assigned index date.

Table 1 describes the individuals shown in Figures 2 and 3: 284,764 diagnosed patients

Figure 1: Patients with a First Recorded ADRD Diagnosis, by Age at Diagnosis and Sex



Notes: The figure shows how many patients received their first recorded ADRD diagnosis in RIPS at each age from 45 to 100, separately for men and women.

and 38,273 never-diagnosed individuals, measured in the year before the index date. Diagnosed patients are nearly 29 years older on average and are somewhat less likely to be male. They also used more health care in the year before the index date: about 2.3 more general-practitioner consultations (5.96 versus 3.64), about 0.10 more emergency-room visits (0.25 versus 0.16), and about three times as many hospitalizations (0.13 versus 0.04). Because the never-diagnosed individuals are much younger and are not matched to patients, their utilization levels are not a counterfactual for patients. All of the health-care comparisons in this paper are descriptive.

Figure 2 shows how patients' health-care use evolves around the first recorded diagnosis. It reports general-practitioner consultations, hospitalizations, procedures, and emergency-room visits in each semester from two years before through two years after diagnosis. Two years before the event, patients average about 2.1 general-practitioner visits, 0.03 hospitalizations, 2.6 procedures, and 0.08 emergency-room visits per semester, and each measure rises during the year before diagnosis. Use then rises sharply at diagnosis. General-practitioner visits climb from about 2.8 per semester in the semester before diagnosis to about 4.8 in

Table 1: Health Characteristics of Patients and Never-Diagnosed Individuals in the Year before the Index Date

Characteristic	Patients	Never diagnosed	Difference
Age at index date	77.341	48.523	28.818***
	(10.906)	(16.160)	
Male	0.341	0.373	−0.031***
	(0.474)	(0.484)	
GP visits, prior year	5.955	3.641	2.314***
	(10.013)	(9.193)	
Emergency visits, prior year	0.250	0.155	0.095***
	(0.846)	(0.659)	
Hospitalizations, prior year	0.134	0.042	0.092***
	(0.576)	(0.297)	
Unique people	284,764	38,273	

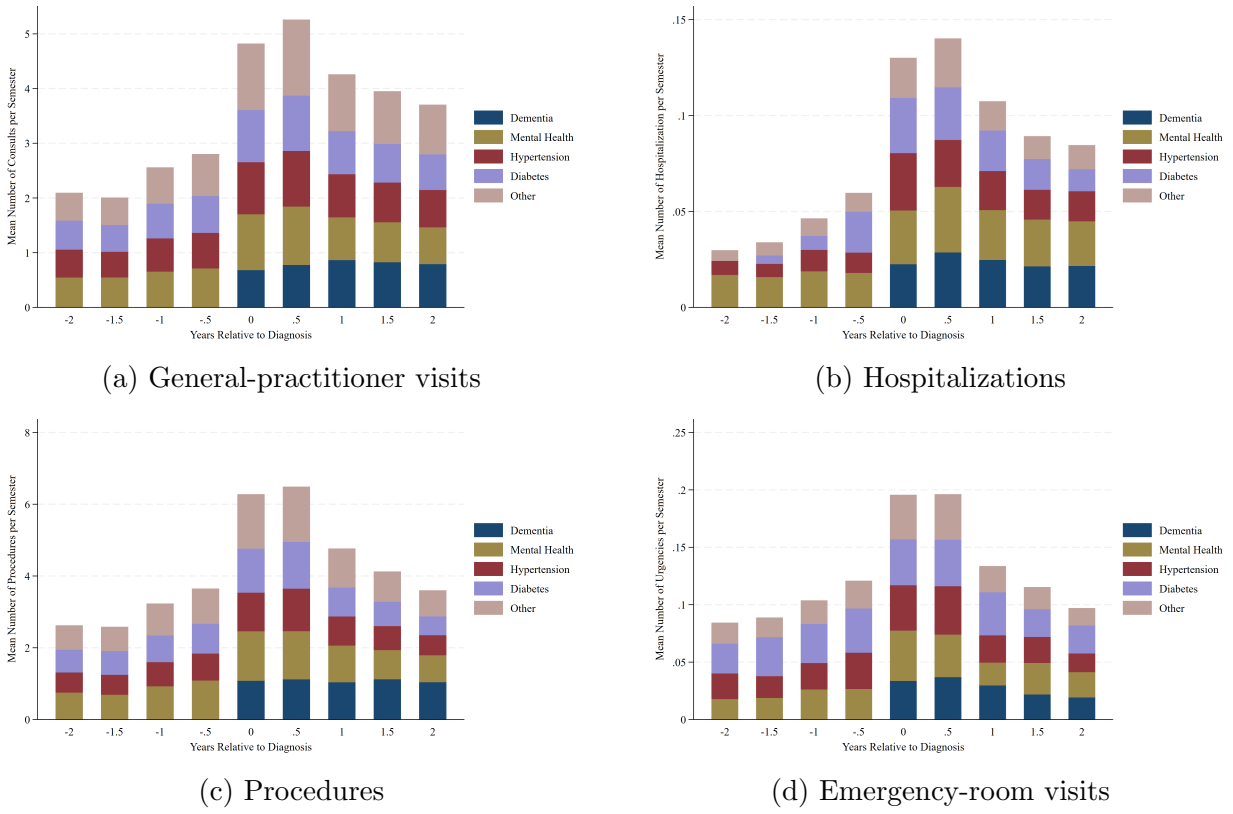
Notes: Means, with standard deviations in parentheses. Patients have a first recorded ADRD diagnosis in 2013 or later; never-diagnosed individuals are never observed with one. The index date is the diagnosis date for patients and an assigned date for never-diagnosed individuals, and health-care use counts encounters in the year before that date. The difference column is patients minus never-diagnosed individuals. A never-diagnosed individual can enter the sample at more than one index date, and the unique-people row counts each person once. No matching or weighting is applied. The sample is the same as in Figures 2 and 3. Stars mark statistically significant differences: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

the diagnosis semester and 5.3 in the next; procedures rise to about 6.5, hospitalizations to about 0.14, and emergency-room visits to about 0.20. Most series peak in the diagnosis semester or the one after it.

Utilization declines after the first post-diagnosis semester but does not return to where it started. Two years after diagnosis, patients still average about 3.7 general-practitioner visits and 3.6 procedures per semester; hospitalizations remain near 0.08 and emergency-room visits near 0.10. By then, procedures have returned to about their level in the semester before diagnosis and emergency-room visits are below it, while general-practitioner visits and hospitalizations remain higher. Encounters coded to dementia and other mental-health conditions account for part of the increase, while hypertension, diabetes, and other diagnoses remain important throughout the window.

Figure 3 shows the same measures for never-diagnosed individuals around an assigned reference quarter. Each never-diagnosed individual is randomly assigned a patient's diagnosis quarter, with eligible quarters restricted to those within the individual's own observation

Figure 2: Health-Care Use among Patients around the First Recorded ADRD Diagnosis



(a) General-practitioner visits

(b) Hospitalizations

(c) Procedures

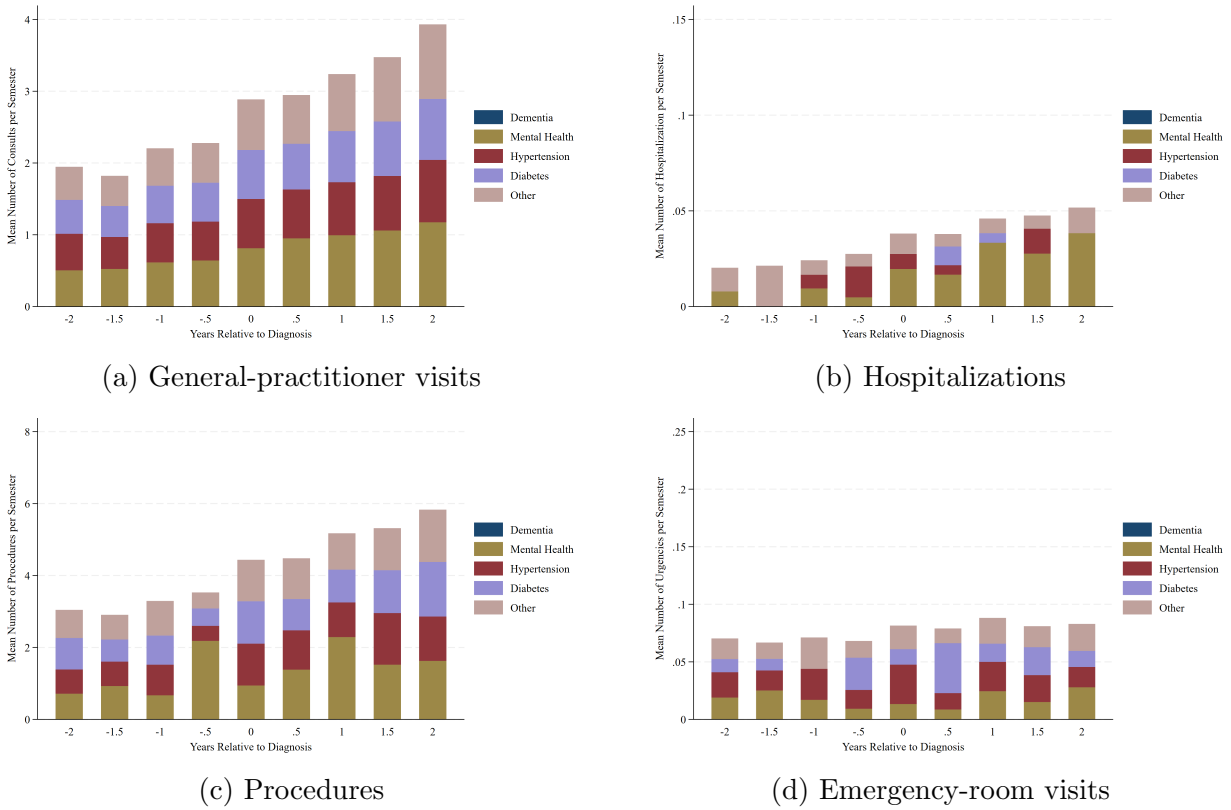
(d) Emergency-room visits

Notes: Each bar is the average number of encounters per patient in a six-month period, from two years before to two years after the first recorded ADRD diagnosis (year 0 on the horizontal axis). Bar segments split encounters by the primary diagnosis recorded for the visit. Averages are unweighted. The figure is descriptive: it does not compare patients with a control group.

period. Their health-care use also increases over the window, and it steps up at the assigned date: general-practitioner visits rise from about 2.3 to 2.9 per semester and procedures from about 3.5 to 4.4. The pattern differs from the patients' in two ways. First, the patients' increase at diagnosis is much larger. Their general-practitioner visits rise by about 2.0 per semester and procedures by about 2.6, and their hospitalizations and emergency-room visits rise by roughly 115 and 60 percent, compared with about 40 and 20 percent among never-diagnosed individuals. Second, never-diagnosed individuals' use keeps rising after the assigned date, reaching about 3.9 general-practitioner visits and 5.8 procedures per semester two years later, whereas the patients' use peaks within a year of diagnosis and then declines. The younger age and unmatched composition of this group mean that its levels and trends are not a counterfactual for patients, but the comparison shows that the patients' spike and

subsequent decline are not simply a feature of centering health records on an index date.

Figure 3: Health-Care Use among Never-Diagnosed Individuals around an Assigned Date



(a) General-practitioner visits

(b) Hospitalizations

(c) Procedures

(d) Emergency-room visits

Notes: Each bar is the average number of encounters per person in a six-month period, from two years before to two years after an assigned index date (year 0 on the horizontal axis): a patient's diagnosis quarter, drawn at random from those within the individual's observation period. These individuals are never observed with an ADRD diagnosis. They are much younger than the patients in Figure 2 (see Table 1) and are not matched to them, so the figure shows how health-care use moves around an arbitrary date, not what patients' use would have been without ADRD. Bar segments split encounters by the primary diagnosis recorded for the visit. Vertical axes match Figure 2 except for general-practitioner visits.

The concentration of health-care use around the event shows that the first recorded diagnosis marks a medically salient period. It is a period in which patients intensify contact with providers and families may receive new information and face new time demands. The rise in utilization before diagnosis is consistent with diagnostic workup or prodromal symptoms.

3.2 Formal employment records

PILA records monthly contributions to Colombia's social-security system and therefore captures employment in the formal sector. Our extract covers 2008 through 2020 and includes

every individual who is formally employed at least once in 2012.[1] Because the extract is defined by formal employment in 2012 and ends in 2020, the patient and household labor-market analyses keep only patients first diagnosed from 2013 through 2018. Starting in 2013 means the extract's 2012 employment condition always falls in a year before a patient's diagnosis, and ending in 2018 means every patient, household, and comparison unit can be followed for the full eight quarters after the index date. The records identify employers, reported formal earnings, days worked, and selected contribution categories. We classify an individual as formally employed in a month when PILA records a contribution associated with formal work. Monthly formal earnings equal the sum of earnings recorded across employers and are set to zero in months in which the individual is not observed in formal employment. Because PILA does not capture informal employment, a decline in observed formal employment may represent either an exit from work or a transition from formal to informal employment.

For the patient labor-market analysis, we focus on patients who are aged 45 to 60 and at least two years below the sex-specific statutory retirement age at diagnosis and who have some formal employment around diagnosis. We require formal employment in at least one of the 24 months from one year before through one year after the index date. Because this window extends past diagnosis, an individual with no formal employment in the year before the index date enters the sample only if formally employed in the year after it; Section 4.4 discusses the implications. The resulting risk-set sample contains 5,834 diagnosed patients and 1,948 unique comparison candidates before matching. Comparison individuals are never observed with an ADRD diagnosis in RIPS and must satisfy the same age, employment, and observation requirements relative to their assigned index dates when they enter a diagnosis-quarter risk set. The covariates used in the matching procedure are measured strictly before the index quarter, as described in Section 4.

Table 2 reports pre-index characteristics before matching. The two groups have simi-

[1] All PILA outcomes in the paper, for patients, comparison individuals, and household members, come from this extract.

lar sex composition, but patients are 1.45 years older on average. Their pre-period share formally employed is 12.3 percentage points higher, and their mean pre-period quarterly earnings are approximately $84 higher. These differences are descriptive rather than causal. They motivate matching patients to contemporaneous comparison individuals with similar observed characteristics.

Table 2: Pre-Diagnosis Characteristics of Working-Age Patients and Comparison Individuals, before Matching

Characteristic	Patients	Comparison	Difference
Male	0.551	0.549	0.002
	(0.497)	(0.498)	
Age at index date	50.502	49.053	1.449***
	(4.286)	(4.153)	
Quarterly earnings, pre-period average	722.053	638.435	83.618***
	(1037.002)	(932.936)	
Share formally employed, pre-period average	0.791	0.668	0.123***
	(0.376)	(0.444)	
Unique people	5,834	1,948	

Notes: Means, with standard deviations in parentheses. Patients are working-age individuals with a first recorded ADRD diagnosis and some formal employment around it; comparison individuals are never diagnosed and meet the same age and employment requirements. All characteristics are measured before the index date (the diagnosis date for patients and an assigned date for comparison individuals). The share formally employed is the share of each quarter in which the individual appears as formally employed in PILA, averaged over the four quarters before the index date. Quarterly earnings are the sum of the individual's formal wages in each quarter, in 2007 USD, averaged over the same four quarters. A comparison individual can be eligible at more than one index date, so tests of differences cluster standard errors by person. The difference column is patients minus comparison individuals. Table 4 reports these characteristics before and after matching. Stars mark statistically significant differences: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

3.3 Household rosters

SISBEN is used to determine eligibility for major social programs and covers a large share of Colombian households, particularly those with low and moderate incomes. We use household rosters from the second and third SISBEN waves, conducted around 2005–2007 and 2011–2013, to identify individuals recorded as residing with a patient who is subsequently diagnosed with ADRD and to measure each individual's relationship to the patient. Because household composition is observed only at the time of the SISBEN interview, "household

member" refers to recorded co-residence at that date rather than confirmed co-residence at the date of diagnosis. Members who moved out before diagnosis would generally attenuate the household estimates.

We identify working-age SISBEN household members ages 24–60 and observe their formal employment in the PILA to construct quarterly measures of their formal-sector outcomes.[2] The principal employment outcome is the share of working-age household members observed in formal employment during the quarter. We also measure total quarterly formal earnings separately for female and male working-age members. Household formal earnings and members' days worked also describe households before diagnosis (Table 3). All of these measures capture formal-sector activity only; household members who move into informal work are no longer observed as employed in PILA.

Table 3 describes the household sample before matching. It contains 14,215 households containing an ADRD patient and 18,475 comparison households. The patients in these households are 70 years old at diagnosis on average, and 39 percent are male. The PILA characteristics are measured over the four quarters preceding the actual or assigned index quarter; the SISBEN characteristics describe the household roster observed in the corresponding survey wave.

The unmatched groups differ substantially. Working-age members of ADRD households are 6.8 years older on average and are somewhat less likely to be male. Before diagnosis, they work approximately 34 fewer days per quarter, and their households record approximately $313 less in quarterly formal earnings. The SISBEN rosters also show that ADRD households are smaller: they contain 5.94 members on average, compared with 6.55 in the comparison group. They are less likely to be intergenerational households, although co-residence across generations is common in both groups. These differences reinforce the need to compare ADRD households with similar households observed in the same calendar period.

[2]Unlike the patient analysis, the household analysis places no age restriction on the patient: it includes the households of patients diagnosed at any age, and the patient's own employment and earnings are excluded from all household measures.

Table 3: Pre-Diagnosis Characteristics of ADRD and Comparison Households, before Matching

Characteristic	ADRD households	Comparison households
Panel A. PILA-linked working-age household members		
Male	0.57	0.61
Age	41.13	34.33
	(8.34)	(9.00)
Days worked per quarter	53.95	88.14
	(36.30)	(5.30)
Household quarterly formal earnings (2007 USD)	628.26	941.55
	(811.28)	(746.81)
Members ever observed in PILA	2.18	2.36
	(0.45)	(0.63)
Panel B. Household composition in SISBEN		
Household members	5.94	6.55
Children under age 18	1.90	2.00
Adults age 18 or older	4.04	4.55
Male share	0.473	0.472
Female share	0.527	0.528
Intergenerational household	0.82	0.91
Panel C. Diagnosed patient		
Patient's age at diagnosis	70.02	–
Patient is male	0.39	–
Households	14,215	18,475

Notes: Unweighted means, with standard deviations in parentheses where available. ADRD households include a member, of any age, with a first recorded ADRD diagnosis; comparison households have no diagnosed member. Panel A describes working-age household members linked to the PILA formal-employment records. Household quarterly formal earnings add up the earnings of these members, excluding the patient, in 2007 USD. Members ever observed in PILA counts the working-age members who appear in the formal records at any point. Sex, age, and days worked come from the descriptive sample and are averaged over the four quarters before the index quarter; earnings, members in PILA, and Panels B and C come from the matching sample before matching. Panel B describes the full SISBEN II or III household roster, which includes children, so it can differ from Panel A. An intergenerational household contains a co-resident child, grandchild, or parent. Panel C describes the diagnosed patient in each ADRD household; comparison households have no patient. Table 5 compares these households before and after matching.

3.4 Comparison population and outcome timing

The comparison samples are drawn from individuals who are never observed with an ADRD diagnosis in RIPS and are not recorded in SISBEN as residing with an ADRD patient. For the patient analysis, the unit of comparison is the individual. In each diagnosis quarter, comparison individuals must satisfy the same age, employment, and observation requirements as the patients with whom they may be matched. A comparison individual may enter the eligible pool in multiple diagnosis quarters; age, employment, and other time-varying covariates are recalculated relative to each potential index date.

For the household analysis, the unit of comparison is a SISBEN household in which no member is observed with an ADRD diagnosis. Comparison households must have an observed SISBEN roster and at least one working-age member who can be linked to PILA. As in the patient analysis, household eligibility and pre-period covariates are evaluated relative to the index quarter of the treated household's risk set.

The frequency of observation differs across outcome families. We measure health-care utilization by semester. The profiles in Figures 2 and 3 run from two years before through two years after the actual or assigned index date. PILA is observed monthly, but all labor-market figures in the paper aggregate these records to quarters. The unweighted profiles display quarters -4 through 8. The matched event studies display quarters -4 through 8, with quarter -1 omitted. Quarter zero contains the patient's first recorded ADRD diagnosis, or the assigned index date for a comparison unit.

All matching covariates are constructed using information observed strictly before the index quarter. In the quarterly event studies, quarter -1 is the omitted reference period.

4 Matched, Stacked Difference-in-Differences Design

A comparison of patients diagnosed with ADRD with the full population would be difficult to interpret. Diagnosed patients differ from never-diagnosed individuals in age, prior health-

care use, and attachment to the formal labor market. Households containing a diagnosed patient may likewise differ in size, earnings, and employment before diagnosis. In addition, never-diagnosed individuals have no naturally observed event date around which to measure outcomes.

We address these problems using a matched, stacked difference-in-differences design in the spirit of Adams-Prassl et al. (2023). The design proceeds in three steps. First, for each diagnosis quarter, we construct a contemporaneous risk set containing newly diagnosed units and eligible never-diagnosed comparison units. Second, we match each treated unit to observationally similar members of its risk set using characteristics measured before diagnosis. Third, we align each matched set around the treated unit's diagnosis date, stack the resulting panels, and compare changes in outcomes within risk sets. This approach makes both the comparison population and its calendar-time environment explicit.

4.1 Calendar-time risk sets

The treated unit is an individual in the patient analyses and a household containing a newly diagnosed patient in the household analyses. For each calendar quarter in which at least one patient receives a first recorded ADRD diagnosis, we form a risk set containing the newly diagnosed units and all never-diagnosed comparison units that satisfy the relevant age, sample, and observation requirements in that quarter.

A comparison unit may enter more than one risk set. For example, an individual who remains observable and eligible throughout several diagnosis quarters may be considered as a potential comparison in each of those quarters. We recalculate age, employment history, earnings, and other time-varying covariates using only information available before each potential index quarter. The same individual can therefore have different covariate values in different risk sets.

This construction resembles incidence-density sampling in that comparison units are evaluated at the time a treated event occurs. It differs from conventional incidence-density sam-

pling because we restrict the comparison population to individuals who are never observed to receive an ADRD diagnosis. The advantage of the rolling construction is that it compares treated and untreated units in the same calendar environment without assigning every never-diagnosed individual a single arbitrary diagnosis date.

When several treated units are diagnosed in the same quarter, we estimate one propensity-score model using the risk set for that quarter. We then construct a separate matched set for each treated unit. Each set contains one treated unit and its selected comparison units and inherits the treated unit's diagnosis date. We retain the original person or household identifier throughout the procedure so that repeated appearances of a comparison unit can be accounted for in estimation and inference.

4.2 Propensity scores and matching

Within each risk set, we estimate a logit model for the probability that a unit belongs to the newly diagnosed group. All covariates are measured before the index quarter. The propensity-score specification differs between the patient labor and household labor analyses.

Patient labor-market outcomes. For the patient labor analysis, the model includes sex, age at the index date in five-year categories, pre-period earnings, and formal-employment history during the preceding four quarters. The sample restriction described in Section 3, formal employment in at least one of the 24 months around the index date, applies to patients and comparison individuals alike.

Household labor-market outcomes. For the household analysis, the model includes pre-period household quarterly formal earnings, the number of working-age household members who ever appear in PILA, the share of the preceding four quarters in which at least one such member was formally employed, and an indicator for whether the household contained at least one stably employed member. A household has a stably employed member if at least one working-age member is formally employed in at least 3 of the 4 preceding quarters.

This differs from the number of members who ever appear in PILA, which counts every working-age member observed in the formal-employment records at any point.

In the labor-market specifications, pre-period earnings enter the propensity score as a categorical rather than continuous measure. We distinguish zero earnings from terciles of positive earnings within the relevant risk set. This follows the logic of the earnings-tercile pre-match in Adams-Prassl et al. (2023). Coarsening earnings reduces the risk of separation in the quarter-specific logits and avoids imposing a linear relationship between earnings and the probability of diagnosis.

We match each treated unit to its nearest neighbors on the estimated propensity score within the same risk set, using `psmatch2` (Leuven and Sianesi 2003). The baseline specification uses ten neighbors in the patient analysis and three neighbors in the household analysis, where the pool of comparable units is smaller. Matching is conducted with replacement and is restricted to treated units on common support. A comparison unit may therefore be matched to more than one treated unit within or across diagnosis quarters.

When several comparison units are tied at the distance of the final neighbor, we retain all tied units rather than selecting among them arbitrarily. We normalize their weights so that the comparison units in each matched set jointly receive the same total weight as the treated unit. Treated units receive a weight of one. This normalization gives each retained treated unit equal influence even when ties cause the number of comparison units to vary across matched sets.

The resulting estimand is an average treatment effect for treated units on common support. Treated units for which the data contain no sufficiently comparable never-diagnosed observation are excluded. The estimand therefore applies to the subset of diagnosed patients or households whose observed characteristics overlap with those of the comparison population, not necessarily to all individuals diagnosed with ADRD.

4.3 Balance and composition of the matched samples

We assess the matching procedure using covariate means and propensity-score overlap. Tables 4 and 5 report means before and after matching for the patient labor and household labor samples, and Figure 11 displays propensity-score overlap for the household analysis. Because statistical tests can reject small differences in large samples, we focus on the economic magnitude of remaining imbalances.

Table 4 reports balance for the patient labor sample. Before matching, diagnosed patients are 1.45 years older than comparison individuals, their pre-period share formally employed is 12.3 percentage points higher, and their mean pre-period quarterly earnings are about $84 higher. Matching removes nearly all of the age and earnings differences. After matching, mean age is 50.25 among patients and 50.21 among comparisons, the male shares differ by 0.9 percentage points, and mean pre-period quarterly earnings differ by about $14; none of these differences is statistically significant.

Table 4: Patient and Comparison Characteristics before and after Matching

	Before matching			After matching		
Characteristic	Patients	Comparison	Difference	Patients	Comparison	Difference
Male	0.551	0.549	0.002	0.545	0.536	0.009
	(0.497)	(0.498)		(0.498)	(0.499)	
Age at index date	50.502	49.053	1.449***	50.248	50.213	0.035
	(4.286)	(4.153)		(4.136)	(4.113)	
Quarterly earnings, pre-period	722.053	638.435	83.618***	727.438	741.602	−14.163
	(1037.002)	(932.936)		(1032.413)	(947.918)	
Share formally employed, pre-period	0.791	0.668	0.123***	0.811	0.822	−0.011
	(0.376)	(0.444)		(0.346)	(0.338)	
Unique people	5,834	1,948		5,602	1,797	

Notes: Means, with standard deviations in parentheses. Before matching includes all eligible patients and comparison individuals. After matching keeps the patients who have a comparable match (common support) and weights comparison individuals by their matching weights from `psmatch2` (Leuven and Sianesi 2003). All characteristics are measured before the index date. The share formally employed is the share of each quarter in which the individual appears as formally employed in PILA, averaged over the four quarters before the index date. Quarterly earnings are the sum of the individual's formal wages in each quarter, in 2007 USD, averaged over the same four quarters. A comparison individual can be eligible at more than one index date, so tests of differences cluster standard errors by person. Differences are patients minus comparison individuals. Stars mark statistically significant differences: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Matching also nearly closes the gap in formal-employment history. The pre-period share formally employed is 0.811 among diagnosed patients and 0.822 among matched comparisons, a difference of 1.1 percentage points that is not statistically significant.

The overlap restriction retains 5,602 of 5,834 diagnosed patients, or approximately 96 percent of the original treated sample. The matched comparison group contains 1,797 unique individuals. Because comparison individuals can be used in several matched sets, the number of unique controls is not the effective number of weighted comparison observations. Comparing the treated columns before and after matching shows who is excluded: the 232 dropped patients are older than those retained (about 56.6 versus 50.2 years at the index date), had lower pre-period quarterly earnings (about $592 versus $727), were less often formally employed before the index date (about 0.31 versus 0.81), and are more often male (about 0.70 versus 0.55).

Table 5 reports the corresponding results for the household analysis. Before matching, ADRD households have substantially lower formal earnings and weaker attachment to formal employment than comparison households. Mean pre-period household earnings are approximately $628 among treated households and $942 among comparison households. The share with at least one member formally employed in at least 3 of the 4 preceding quarters is 61.0 percent among treated households, compared with 85.1 percent of comparison households, and treated households are somewhat smaller.

The matched samples are considerably more similar. Mean pre-period household earnings are about $916 among treated households and $897 among matched comparisons. The mean number of working-age members who ever appear in PILA, excluding the patient, is 2.233 among treated households and 2.217 among matched comparison households. The share with at least one member formally employed in at least 3 of the 4 preceding quarters differs by 1.7 percentage points. Because the matched samples are large, this difference and the difference in members observed in PILA are statistically significant, as is the difference of 0.16 members in household size, but none of the three exceeds 0.07 standard deviations.

Table 5: ADRD and Comparison Household Characteristics before and after Matching

	Before matching			After matching		
Characteristic	ADRD	Comparison	Difference	ADRD	Comparison	Difference
Panel A. PILA labor-market characteristics						
Quarterly household earnings, pre-period	628.258 (811.278)	941.551 (746.805)	−313.293***	916.215 (863.497)	896.789 (733.277)	19.426
Has a stably employed member, pre-period	0.610 (0.488)	0.851 (0.356)	−0.242***	0.906 (0.292)	0.923 (0.267)	−0.017***
Members ever observed in PILA	2.177 (0.454)	2.360 (0.626)	−0.183***	2.233 (0.511)	2.217 (0.494)	0.016**
Panel B. Household composition in SISBEN						
Household members	5.938 (3.055)	6.550 (3.295)	−0.613***	5.698 (2.860)	5.855 (3.000)	−0.157***
Panel C. Diagnosed patient						
Patient's age at diagnosis	70.02	–		69.54	–	
Patient is male	0.39	–		0.39	–	
Unique households	14,215	18,475		7,733	11,622	

Notes: Means, with standard deviations in parentheses. Before matching includes all eligible ADRD and comparison households. After matching keeps the ADRD households that have a comparable match (common support) and weights comparison households by their matching weights from `psmatch2` (Leuven and Sianesi 2003). Panel A comes from the PILA records before diagnosis. Household quarterly formal earnings add up the earnings of working-age members, excluding the patient, in 2007 USD. A household has a stably employed member if at least one working-age member is formally employed in at least 3 of the 4 quarters before diagnosis. Members ever observed in PILA counts the working-age members who appear in the formal records at any point. Panel B comes from the SISBEN II or III roster and was not used in the matching. Panel C describes the diagnosed patient in each ADRD household, who can be any age; it does not apply to comparison households and was not used in the matching. A comparison household can be eligible at more than one index date; the before-matching means count every eligible date, and the last row counts each household once. Tests of differences cluster standard errors by household. Differences are ADRD minus comparison households. Stars mark statistically significant differences: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Achieving this balance changes the composition of the treated sample. The common-support restriction retains 7,733 of 14,215 treated households, or approximately 54 percent. In particular, the share with a member formally employed in at least 3 of the 4 pre-period quarters rises from 0.610 in the full treated sample to 0.906 in the matched treated sample. The 6,482 households dropped from the match are far less attached to formal work: their mean pre-period quarterly formal earnings are about $285, compared with $916 among retained households, and about 26 percent have a stably employed member, compared with

91 percent. The patients themselves are similar in the two samples: their mean age at diagnosis is 69.5 years in retained households and 70.0 years in the full treated sample, and 39 percent are male in both. The household estimates therefore apply to households that were strongly attached to formal employment before diagnosis and that resemble an observed never-diagnosed household. They need not describe the response of all households containing a patient with ADRD.

Taken together, the tabulated means show that matching improves comparability in age, earnings, formal-employment history, and household size. In the patient sample, no matching covariate differs significantly after matching. In the household sample, the remaining differences in stable employment, members in PILA, and household size, which is not a matching covariate, are statistically significant but small, and the overlap restriction selects a much more formally attached group of treated households. Finally, all pooled diagnostics can conceal diagnosis quarters with weaker overlap.

4.4 Weighted event-study regression

We estimate the event study on the stacked matched sample. Let i index the original individual or household, let g denote the risk set's index quarter, and let q index calendar quarters. Each (i, g) pair is a distinct stacked unit. A treated unit appears in the risk set associated with its own diagnosis quarter, while a never-diagnosed comparison unit can appear in several risk sets and therefore contribute several (i, g) pairs. Define

$$D_{igq}^{k} = T_{ig} \times \mathbf{1}\{q - g = k\},$$

where T_{ig} equals one if unit i is diagnosed in index quarter g and zero for the never-diagnosed comparison units included in that risk set. Thus, D_{igq}^{k} identifies observations of a treated unit that are k quarters from diagnosis; it is zero in every period for comparison units. We estimate

$$Y_{igq} = \alpha_{ig} + \lambda_{gq} + \sum_{k \in \{-4,-3,-2,0,1,\ldots,8\}} \delta_k D^k_{igq} + \varepsilon_{igq}, \tag{1}$$

where Y_{igq} is the outcome of interest, α_{ig} is a unit-by-risk-set fixed effect, and λ_{gq} is a risk-set-by-calendar-quarter fixed effect. The unit-by-risk-set fixed effects absorb all time-invariant differences between each treated or comparison unit and the other units in the same matched risk set. Because a reused comparison unit receives a different fixed effect in each risk set, its level is allowed to differ across appearances. The risk-set-by-calendar-quarter fixed effects absorb everything that the units in a risk set share in a given quarter. Because all units in risk set g share the index quarter g, these fixed effects also absorb any path relative to the index date that treated units have in common with their comparisons, such as mean reversion in employment after the pre-index employment screen. The coefficients δ_k are therefore identified only from comparisons between treated and comparison units in the same risk set and quarter.

Quarter -1, the quarter immediately preceding diagnosis, is omitted. The coefficients δ_k compare the within-unit change of treated units at event quarter k with the contemporaneous change among weighted never-diagnosed comparisons in the same risk set, relative to quarter -1. Sex-specific effects come from estimating Equation 1 separately on the female and male samples.

The regression is estimated using analytic weights equal to the propensity-score matching weight. Treated units receive weight one, while never-diagnosed comparison units receive the normalized matching weights described above. Standard errors are clustered by the original individual in the patient analysis and by the original household in the household analysis. Clustering on the original identifier allows for serial correlation and for dependence created when the same comparison unit appears in more than one matched sample.

The never-diagnosed comparison group is important because the patient labor analysis conditions on formal employment in the two years around the index date. Even without ADRD, selecting workers on employment within a fixed window can produce mechanical

employment patterns inside and outside that window. Applying the same eligibility rule to comparison individuals relative to their assigned index dates allows their outcomes to capture these patterns, and the risk-set-by-calendar-quarter fixed effects remove the part that patients share with their comparisons. This does not fully neutralize them, because for individuals with no formal employment in the year before the index date the rule requires employment in the year after it, when diagnosed patients are less likely than comparisons to work. Patients in this subgroup are therefore selected on post-diagnosis employment, which can understate the estimated losses for them; anyone employed at least once in the preceding year faces no requirement on later employment.

4.5 Identification and interpretation

The identifying assumption is that in the absence of a recorded ADRD diagnosis, outcomes for treated units must have evolved similarly to those of their matched, weighted never-diagnosed comparison units after conditioning on the matching covariates and the fixed effects in Equation 1. Matching makes this assumption more plausible by restricting comparisons to units with similar observed characteristics in the same calendar environment.

The event-study coefficients before diagnosis provide evidence about this assumption. Coefficients that differ systematically from zero before the event may reflect prodromal symptoms, anticipatory household adjustment, differential selection into diagnosis, or remaining differences between treated and comparison units. For that reason, the shape and magnitude of the pre-diagnosis estimates are part of the identifying evidence rather than a secondary specification check.

The treatment is the first ADRD diagnosis recorded in RIPS, not the biological onset of dementia. Cognitive decline may begin years before the first diagnosis appears in the administrative data and may already affect health-care use, employment, and household decisions. Conversely, the recorded diagnosis may itself convey new information, initiate treatment, affect eligibility for benefits, or alter the behavior of patients, relatives, and

employers. For patients, we therefore interpret the coefficients as differences in economic trajectories surrounding the first recorded diagnosis. For other household members, the estimand is the effect of a relative's recorded diagnosis, including the care needs, income losses, and new information that follow it, rather than the effect of the disease's biological onset.

The choice of comparison group reflects our economic question. We seek to measure the formal labor-market deterioration associated with ADRD for patients and their co-resident family members, not only the effect of receiving a diagnosis at one date rather than another. For that purpose, the most relevant observable benchmark is a matched individual or household with no ADRD diagnosis recorded in the observation window. This comparison approximates how formal employment and earnings would have evolved without a recorded dementia event, including the losses associated with the progressive condition rather than only the immediate information conveyed by diagnosis. The contemporaneous risk sets, matching on pre-index outcomes, and risk-set-by-calendar-quarter fixed effects make this benchmark more credible than an unconditional comparison with the never-diagnosed population. In particular, applying the same employment restriction to both groups allows the controls to capture employment changes that would occur even without ADRD after selecting workers on formal employment around the index date.

An alternative benchmark would compare newly diagnosed patients with patients who are eventually diagnosed but have not yet received a recorded diagnosis. That comparison concerns a diagnosis occurring now rather than later among eventually diagnosed patients, which is closer to the comparison used by Fadlon and Nielsen (2021) for the timing of acute health shocks. For ADRD, however, diagnosis often follows gradually emerging symptoms and health-care contact, so its timing is less plausibly exogenous. Patients awaiting diagnosis may already have impaired cognition and declining formal employment, making them an imperfect proxy for the absence of the disease, and using them as controls could attenuate the estimated deterioration. The never-diagnosed comparison has a different vul-

nerability: unobserved differences in underlying health or disease risk between diagnosed and never-diagnosed units could exaggerate the decline attributed to ADRD. We use the never-diagnosed benchmark because it matches the paper's question about the burden of the disease, and we interpret the estimates with this vulnerability in mind.

Finally, matching addresses differences in observed characteristics only. Requiring contemporaneous risk sets, measuring covariates before diagnosis, comparing within units over time, and examining pre-diagnosis coefficients all strengthen the counterfactual. None of these features makes diagnosis randomly assigned. For patients, whose employment already moves before diagnosis, the estimates are most credible as measures of the differential deterioration in formal labor-market outcomes surrounding recorded diagnosis. For other household members, the pre-diagnosis coefficients are close to zero (Figures 8–10), which supports the parallel-trends assumption, and we interpret these estimates as causal effects of a relative's recorded diagnosis for households with adequate overlap in the comparison population. If anything, they understate the total cost of the disease to relatives: matching on pre-period outcomes and measuring changes from quarter -1 absorb any adjustment relatives made before diagnosis, and comparison households may include members with undiagnosed dementia.

5 Results

We report two sets of outcomes: patients' own formal employment and earnings, and the formal employment and earnings of other working-age members of their households. We begin with unweighted patient profiles and then turn to the matched event-study estimates for patients and households.

5.1 Unweighted patient labor-market trajectories

We first compare patients' unweighted quarterly labor-market outcomes with those of eligible never-diagnosed individuals. Figure 4 aligns patients at their recorded diagnosis quarter and comparison individuals at their assigned index quarter. It displays quarters -4 through 8, separately by sex. Formal earnings are set to zero in months without a recorded contribution before being aggregated to the quarter. The earnings profiles therefore reflect both employment exits and changes in earnings among those who remain in PILA.

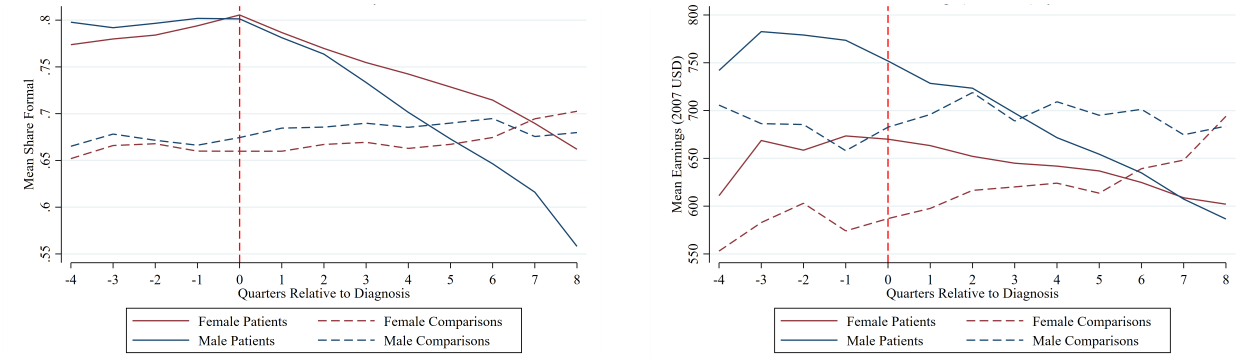
The groups differ in levels before the event. In quarter -1, formal employment is about 0.79 among female patients and 0.80 among male patients, compared with about 0.66 among female comparisons and 0.67 among male comparisons. Patients' quarterly earnings are also higher: roughly \$675 for women and \$775 for men, compared with about \$575 and \$660 in the corresponding comparison groups. These differences make the raw profiles unsuitable for causal inference.

The trajectories nonetheless show a clear post-index divergence. By quarter 8, formal employment is about 0.66 among female patients and 0.56 among male patients, declines of roughly 13 and 24 percentage points from quarter -1. Over the same interval, the comparison series rise slightly, by about 4 percentage points for women and 1 point for men. Patient earnings fall by roughly \$70 for women and \$185 for men, while comparison earnings rise, by about \$120 for women and \$25 for men. These values are approximate readings of unweighted levels in the figure, not matched treatment effects.

Several features can produce this pattern even in the absence of an effect of diagnosis. Patients and comparison individuals differ before weighting; the groups may enter at different calendar dates; and selection on pre-index formal employment can generate subsequent mean reversion. The raw profiles therefore motivate, rather than replace, the risk-set matched design. The estimates below compare patients with observationally similar individuals drawn from the same calendar-time risk set.

Sick leave provides a complementary measure of disruption among patients who remain

Figure 4: Unadjusted Formal Employment and Earnings of Patients and Comparison Individuals



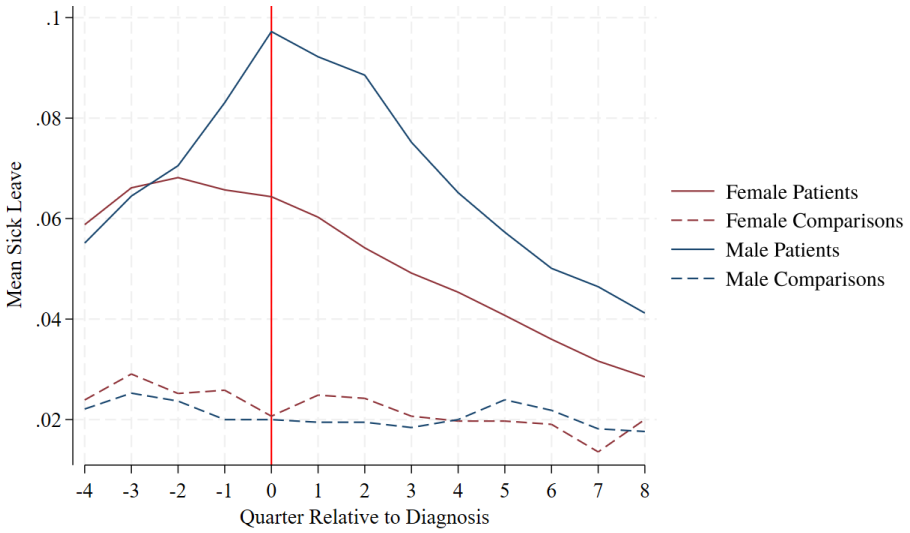
(a) Share formally employed

(b) Average quarterly earnings (2007 USD)

Notes: Each line is the raw average for one group in each quarter, from four quarters before to eight quarters after the index quarter (quarter 0): the quarter of the first recorded ADRD diagnosis for patients and the corresponding index quarter for never-diagnosed comparison individuals. Lines are shown separately for women and men. Earnings are quarterly formal earnings in 2007 USD and count zero earnings in months without formal work. No matching or weighting is applied, so gaps between the lines are not estimates of the effect of diagnosis.

formally employed. Figure 5 shows a sharp diagnosis-quarter peak for men: their recorded sick-leave rate rises from about 8 percent in quarter -1 to nearly 10 percent in quarter 0, then declines. The female patient series peaks earlier, at about 7 percent two quarters before diagnosis, and declines gradually from there. Both patient series remain above their comparison series, which fluctuate around 2 percent, but the gap narrows over time; by quarter 8 it is about 1 percentage point for women and 2 points for men. Because the denominator excludes individuals who have left formal employment, the subsequent decline in recorded sick leave does not imply recovery; it partly reflects a progressively selected group of workers who remain in PILA.

Figure 5: Unadjusted Sick Leave among Formally Employed Patients and Comparison Individuals



Notes: Each line is the share of formally employed people with recorded sick leave in each quarter, from four quarters before to eight quarters after the index quarter (quarter 0), separately for women and men. Only people still in formal employment that quarter are counted, so the group shrinks as people leave formal work. No matching or weighting is applied.

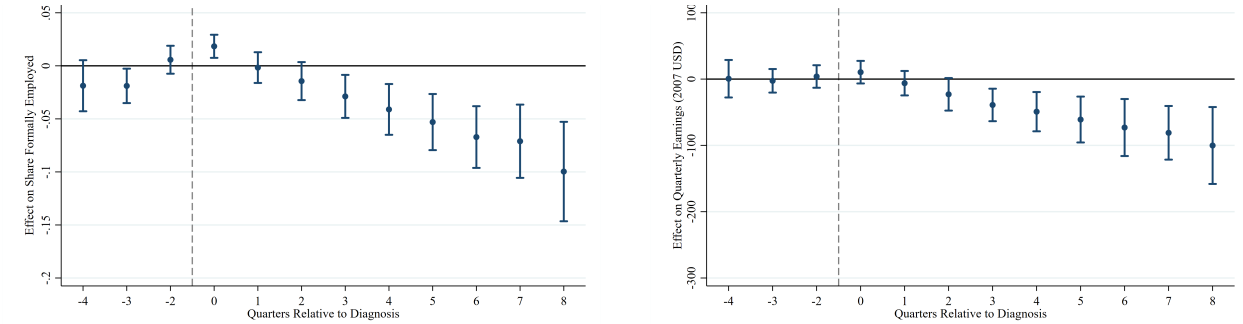
5.2 Patient formal employment and earnings

We now move from the unweighted quarterly profiles to matched event studies. These models restrict the analysis to common-support patients and contemporaneous comparison individuals with similar measured characteristics before diagnosis. The figures display quarters -4 through 8 and omit quarter -1. Table 4 shows that matching retains 5,602 of 5,834 diagnosed patients. It removes nearly all of the pre-diagnosis differences, and none of the remaining differences is statistically significant.

Figures 6 and 7 show that formal employment falls steadily after diagnosis. Four quarters after diagnosis, the estimated employment effect is about -0.041 for women and -0.054 for men. By quarter 8, it reaches approximately -0.10 for women and -0.148 for men.

The pre-diagnosis employment coefficients are not all zero. Relative to quarter -1, patients' formal employment is about 1.5 to 2 percentage points lower in quarters -4 and -3 for both women and men; the quarter -3 coefficients are statistically different from zero, and the quarter -4 coefficients are not. The quarter -2 coefficients are close to

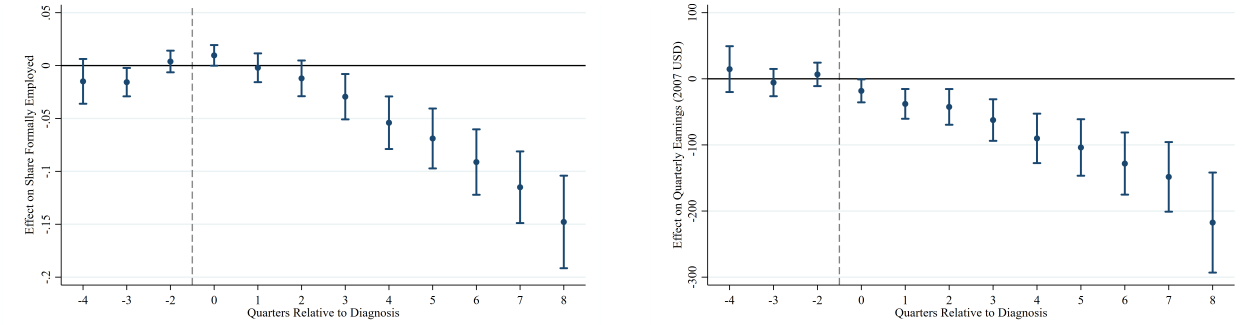
Figure 6: Female Patients' Formal Employment and Earnings around ADRD Diagnosis, Relative to Matched Comparisons



(a) Share formally employed (b) Quarterly earnings (2007 USD)

Notes: Panel (a) uses the share of the quarter in which the individual appears as formally employed; panel (b) uses the sum of the individual's formal wages in the quarter, in 2007 USD. Each point is the estimated difference between diagnosed women and their matched comparison individuals in how much the outcome has changed since quarter −1, the quarter before diagnosis, which serves as the reference and is omitted. Points before quarter 0 show whether the two groups were already diverging before diagnosis. Estimates come from the stacked, matched difference-in-differences model in Equation 1, weighted by matching weights. Bars are 95 percent confidence intervals based on standard errors clustered by person.

Figure 7: Male Patients' Formal Employment and Earnings around ADRD Diagnosis, Relative to Matched Comparisons



(a) Share formally employed (b) Quarterly earnings (2007 USD)

Notes: Panel (a) uses the share of the quarter in which the individual appears as formally employed; panel (b) uses the sum of the individual's formal wages in the quarter, in 2007 USD. Each point is the estimated difference between diagnosed men and their matched comparison individuals in how much the outcome has changed since quarter −1, the quarter before diagnosis, which serves as the reference and is omitted. Points before quarter 0 show whether the two groups were already diverging before diagnosis. Estimates come from the stacked, matched difference-in-differences model in Equation 1, weighted by matching weights. Bars are 95 percent confidence intervals based on standard errors clustered by person.

zero. Patients' employment therefore rises relative to their matched comparisons over the year before diagnosis, and for women it remains about 1.8 percentage points higher in the diagnosis quarter. The unweighted profiles in Figure 4 show a similar pre-diagnosis rise, most visibly among female patients. The omitted quarter is thus a relative high point. Measured from quarter -4 instead, the quarter-8 employment declines would be about 2 percentage points smaller for women and 1.5 points smaller for men.

Quarterly formal earnings follow the same pattern. The estimated earnings gap is approximately \$49 for women and \$90 for men at quarter 4, and about \$100 and \$217, respectively, at quarter 8. These are level changes in 2007 USD, not percentage changes. The pre-diagnosis earnings estimates are close to zero, and their confidence intervals include zero, although they are less precisely estimated than the employment coefficients. Taken together, the figures show no differential decline before diagnosis, a modest relative rise in patients' employment during the preceding year, and a large, sustained divergence after diagnosis. Because of this pre-diagnosis movement, and because a recorded diagnosis need not coincide with disease onset, we interpret the estimates as the change in formal outcomes surrounding diagnosis rather than as the effect of disease onset.

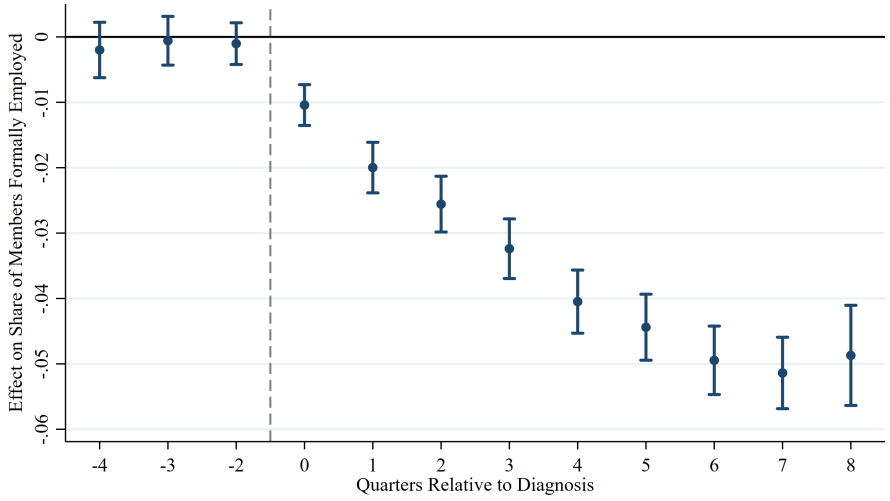
5.3 Household formal labor-market outcomes

The household matching procedure retains 7,733 of 14,215 treated households on common support. Table 5 shows that the retained treated and weighted comparison households have nearly identical pre-period earnings and similar size, and Figure 11 shows closely aligned propensity-score distributions after matching.

Figure 8 shows that formal employment among other household members falls after diagnosis. The coefficient is approximately -0.01 in the diagnosis quarter, -0.040 four quarters later, and -0.049 at quarter 8. The decline is gradual rather than confined to the diagnosis quarter, and it levels off after about six quarters. Before diagnosis, the coefficients are all within about 0.2 percentage points of zero, and none is statistically different from

zero. Figure 9 shows a similar pattern for women and men. At quarter 8, the share of female members formally employed is about 7.2 percentage points lower, while the share of male members is about 5.3 percentage points lower. The female series keeps falling through quarter 8, whereas the male series levels off after about six quarters, at a gap of about 6 percentage points. These outcomes are household-level shares, not individual employment probabilities. The female share is defined only for households with at least one working-age female member, and the male share only for households with at least one working-age male member. Each sex-specific series is therefore estimated on a different subset of households than the pooled series in Figure 8, so the pooled coefficient need not lie between the female and male coefficients. Before diagnosis, the female and male coefficients are all within about half a percentage point of zero, and none is statistically different from zero.

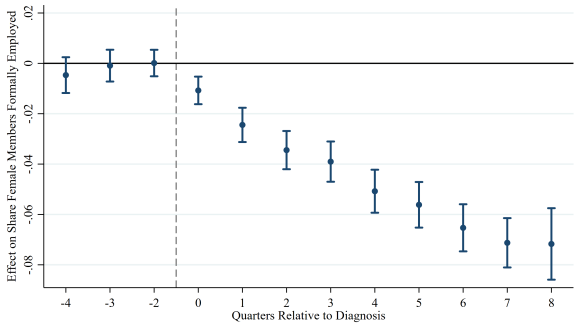
Figure 8: Share of Other Household Members in Formal Employment around ADRD Diagnosis



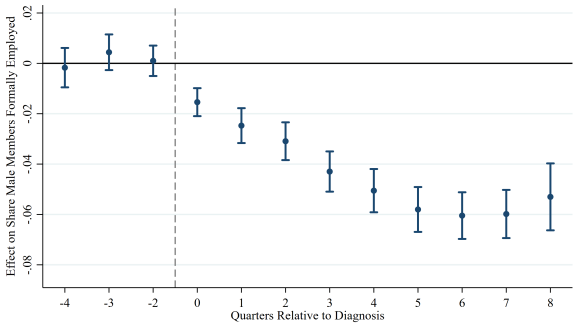
Notes: The outcome is the share of a household's working-age members, other than the patient, who are formally employed in the quarter. Each point is the estimated difference between ADRD households and their matched comparison households in how much the outcome has changed since quarter −1, the quarter before diagnosis, which serves as the reference and is omitted. Points before quarter 0 show whether the two groups were already diverging before diagnosis. Estimates come from the stacked, matched difference-in-differences model in Equation 1, weighted by matching weights. Bars are 95 percent confidence intervals based on standard errors clustered by household.

The earnings estimates reinforce the employment results. Figure 10 shows lower formal wages for both female and male members after diagnosis. At quarter 4, quarterly earnings

Figure 9: Share of Female and Male Household Members in Formal Employment around ADRD Diagnosis



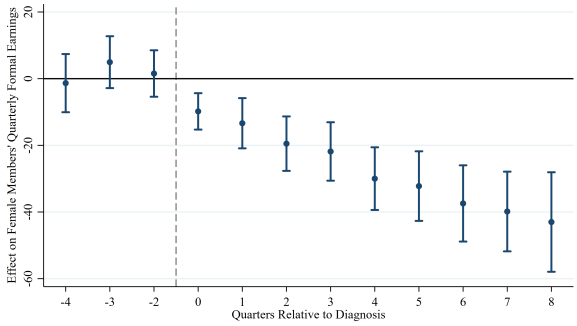
(a) Female members



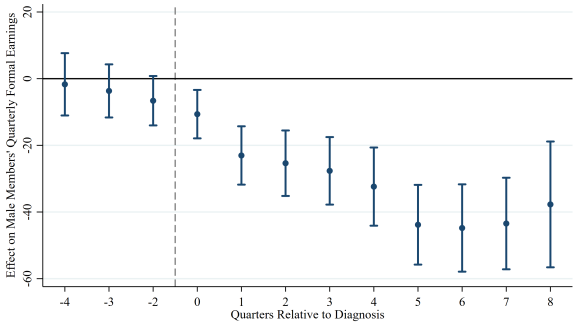
(b) Male members

Notes: Panel (a) uses the share of a household's working-age women who are formally employed in the quarter, and panel (b) the share of its working-age men. Each share is defined only for households with at least one working-age member of that sex, so the two panels and Figure 8 use different sets of households. Each point is the estimated difference between ADRD households and their matched comparison households in how much the outcome has changed since quarter −1, the quarter before diagnosis, which serves as the reference and is omitted. Points before quarter 0 show whether the two groups were already diverging before diagnosis. Estimates come from the stacked, matched difference-in-differences model in Equation 1, weighted by matching weights. Bars are 95 percent confidence intervals based on standard errors clustered by household.

Figure 10: Formal Earnings of Female and Male Household Members around ADRD Diagnosis



(a) Female members



(b) Male members

Notes: Panel (a) uses the total quarterly formal earnings of a household's working-age women, and panel (b) those of its working-age men, in 2007 USD. Each point is the estimated difference between ADRD households and their matched comparison households in how much the outcome has changed since quarter −1, the quarter before diagnosis, which serves as the reference and is omitted. Points before quarter 0 show whether the two groups were already diverging before diagnosis. Estimates come from the stacked, matched difference-in-differences model in Equation 1, weighted by matching weights. Bars are 95 percent confidence intervals based on standard errors clustered by household.

are approximately $30 lower for women and $32 lower for men. By quarter 8, the gaps are about $43 and $38, respectively, in 2007 USD; as with employment, the gap for men is largest around quarter 6, at about $45. Before diagnosis, the earnings coefficients for both sexes are close to zero. The confidence intervals overlap substantially across sex, and the estimates do not support the view that the formal labor-market response is confined to female household members. They do not, however, show that men and women provide equal amounts of care, which the data do not measure.

Because household members show no sustained differential trend before diagnosis, we interpret these declines as causal effects of a relative's recorded diagnosis (Section 4.5). One remaining concern is reverse timing: a relative who loses a formal job may have more time to take the patient to a doctor, so the job loss could prompt the diagnosis. That story implies a one-time drop around diagnosis, whereas the estimated declines keep growing for at least six quarters afterward.

6 Discussion

6.1 Families do not offset patients' losses

The central finding is that the formal labor-market costs of ADRD reach well beyond the patient. Two years after diagnosis, patients' formal employment is about 10 percentage points lower for women and 14.8 points lower for men than among matched comparisons, and their quarterly earnings are about $100 and $217 lower (Figures 6 and 7). These losses amount to roughly 15 and 28 percent of patients' quarterly earnings before diagnosis (Figure 4). The people patients live with do not make up for these losses by working more in the formal sector. Instead, the share of other working-age household members with formal jobs falls by about 5 percentage points, and the formal earnings of both female and male members decline (Figures 8–10). ADRD reduces formal work for patients and for the people who live with them.

These patterns reveal which of two competing forces dominates. A patient's lost earnings and new medical needs raise the value of additional income, which on its own would lead relatives to work more, although even after job displacement spouses replace only a small share of lost income (Halla et al. 2020). ADRD also requires supervision, transportation, help with daily activities, and coordination with providers, which raises the value of relatives' time at home. In Colombia's formal sector, the second force clearly wins: household employment falls steadily for about a year and a half and remains lower two years after diagnosis, rather than reflecting a brief absence around the diagnostic episode. Care demands are the most natural explanation, together with the other changes a diagnosis brings to living arrangements, household production, and expectations about the patient's future.

The result also matters for how economists measure the value of insurance. In high-income countries, a small family labor-supply response to a nonfatal health shock has been read as evidence that households are already well insured (Fadlon and Nielsen 2021). Our findings show that when an illness also raises the need for care, relatives can work less even though the household's income loss is poorly insured. A muted or negative family labor-supply response is therefore not, by itself, a sign of adequate insurance, and approaches that infer the value of social insurance from family labor supply (Haller et al. 2024) need to account for care demands.

6.2 Losses build over time

The timing of the responses shows how ADRD unfolds economically. Health-care use spikes around the first recorded diagnosis and then partly recedes: general-practitioner visits rise from about 2.8 per semester just before diagnosis to about 5.3 shortly after, a far larger jump than among never-diagnosed individuals around an assigned date (Section 3.1). Recorded sick leave among male patients who remain formally employed also peaks in the diagnosis quarter. Formal employment and earnings follow a different path. Rather than recovering after the diagnostic episode, they keep falling, and patients' losses more than double between

the first and second years after diagnosis. Household members' employment also declines gradually and remains lower two years after diagnosis.

This pattern fits a progressive disease. The diagnosis period brings intensive medical contact and short absences from work, while worsening impairment and growing care needs produce lasting changes in employment. The recorded diagnosis therefore marks the start of a steady erosion of formal work for patients and their families. Diagnosis can also change economic decisions directly: it gives patients and families new information about future capacity, can prompt employers to adjust duties or contracts, and can start disability or retirement applications. Some patients leave work because cognitive impairment makes job tasks difficult; others move into arrangements with fewer hours or greater flexibility.

A direct implication is that support tied only to the diagnostic episode would miss most of the loss. Income support, caregiver leave, and home-care services would need to last through at least the two years after diagnosis that we observe, a period over which the losses are still growing.

6.3 Men and women both adjust

Female and male household members respond similarly. Two years after diagnosis, the share of female members in formal employment is about 7.2 percentage points lower and the share of male members about 5.3 points lower, and their quarterly formal earnings are about $43 and $38 lower (Figures 9 and 10). This contrasts with studies from high-income countries that emphasize daughters' responses to their parents' care needs (Crespo and Mira 2014; Maestas et al. 2024; Ramirez Lizardi et al. 2025).

One explanation is that the intensive, long-lasting care that dementia requires is shared more widely in Colombian multigenerational households. Male members may provide direct care, accompany patients to appointments, take over responsibilities the patient used to carry, or adjust their work as the household reorganizes its finances or living arrangements. Household composition reinforces this sharing: SISBEN rosters include spouses,

adult children, siblings, and other relatives, and prior research finds that only children respond differently from children with siblings and that spouses differ from adult children (Løken et al. 2017; Mommaerts and Truskinovsky 2020). For policy, the implication is that caregiver protections and care services designed around women's labor supply would miss a large part of the formal-sector response in Colombia.

6.4 Why formal-sector losses matter

In Colombia, a formal job carries more than a wage: it brings pension contributions, contributory health coverage, and other protections linked to the social-security system. When patients and their relatives leave formal employment, households lose current earnings and these protections at the same time, and the effects extend into retirement through lower pension accumulation. Some relatives may continue to work informally in jobs that are easier to combine with care, but informal work does not replace these protections. The losses we document are therefore losses of both income and social protection. They also reach public budgets: formal-sector exits reduce social-security contributions just as the demand for medical and income support rises, so the costs of dementia flow through the labor market as well as through health care.

6.5 Policy implications

These findings point to an economic cost of ADRD that is substantially larger than the patient's medical spending or lost earnings alone. Because relatives also lose formal work, household income falls from several sources at once, and future pensions fall with it.

Different policies address different parts of this cost. Disability insurance or earnings replacement can compensate for the patient's lost capacity to work, but it does not relax the family's time constraint. Paid caregiver leave and the flexible-work rights in Colombia's recent caregiver laws can help relatives keep formal jobs through periods of intensive care, while day centers, respite services, and publicly supported home care can substitute directly

for family time. The Norwegian evidence that formal care raises daughters' earnings (Løken et al. 2017) shows that care policy can work partly through relatives' labor supply; our estimates suggest that in Colombia the gains would reach men as well as women.

Our setting also makes early action feasible. The first ADRD diagnosis is recorded in administrative health data, and the losses we document grow over the two years that follow. Support could therefore begin at diagnosis, through targeting channels such as SISBEN, before household income has fallen. Because our sample predates Colombia's National Care Policy and recent caregiver laws, our estimates also provide a baseline for evaluating these policies, and any national care system that Congress creates, on both margins we measure: patients' own formal earnings and the formal employment of the people they live with.

7 Conclusion

This paper uses linked Colombian health, formal-employment, and household data to study how the labor-market costs of ADRD extend from patients to their families. Descriptive semester-level health profiles show that the first recorded diagnosis coincides with a sharp, partly persistent increase in outpatient and acute care that is not present among never-diagnosed individuals. Unweighted quarterly labor-market profiles then show patients' employment and earnings separating from those of never-diagnosed comparisons over the two years following diagnosis.

To address baseline differences between those groups, we construct contemporaneous comparison populations separately in each diagnosis quarter. A quarter-specific propensity-score match and stacked difference-in-differences model with risk-set-by-calendar-quarter fixed effects compare changes among common-support units within the same risk set. This approach makes the calendar date and comparison population explicit even though never-diagnosed units have no naturally observed event date.

Formal earnings decline among working-age patients after diagnosis. A relative's diagno-

sis also causes sizeable and persistent reductions in other working-age household members' formal employment and wages, with similar responses among female and male members. Thus, other members do not appear to insure the household by increasing formal work. The labor-market consequences instead extend across the family, consistent with care demands and household disruption compounding the patient's original economic shock.

The estimates show that measuring only the patient's employment misses an important part of the economic burden associated with ADRD. Linking these records to occupation, benefit receipt, informal work, and caregiving time, and estimating responses by each member's relationship to the patient, would show how families divide and finance care after diagnosis and which policies best protect them.

References

Adams-Prassl, Abi, Kristiina Huttunen, Emily Nix, and Ning Zhang. 2023. "The Dynamics of Abusive Relationships." Economics Series Working Papers 1019, University of Oxford, Department of Economics.

Adda, Jérôme, Christian Dustmann, and Katrien Stevens. 2017. "The Career Costs of Children." *Journal of Political Economy*, 125(2): 293–337.

Agarwal, Sumit, and Bhashkar Mazumder. 2013. "Cognitive Abilities and Household Financial Decision Making." *American Economic Journal: Applied Economics*, 5(1): 193–207.

Aguiar, Mark, Erik Hurst, and Loukas Karabarbounis. 2012. "Recent Developments in the Economics of Time Use." *Annual Review of Economics*, 4: 373–397.

Altındağ, Onur, Jane Greve, and Yulya Truskinovsky. 2026. "Family Spillovers of Dementia." NBER Working Paper 34635.

Andersen, Steffen, Elin Colmsjö, Gianpaolo Parise, and Kim Peijnenburg. 2026. "Breaking Bad: How Health Shocks Prompt Crime." *American Economic Journal: Applied Economics*, 18(1): 88–119.

Angrisani, Marco, and Jinkook Lee. 2019. "Cognitive Decline and Household Financial Decisions at Older Ages." *The Journal of the Economics of Ageing*, 13: 86–101.

Bolin, Kristian, Björn Lindgren, and Petter Lundborg. 2008. "Your Next of Kin or Your Own Career? Caring and Working among the 50+ of Europe." *Journal of Health Economics*, 27(3): 718–738.

Bonilla Mejía, Leonardo, María Fernanda Henao Pineda, María José Otero Arango, and Nicholas W. Papageorge. 2025. "No Calm Before the Storm: Economic Losses Prior to Dementia Diagnoses among Working-Age Colombians." Working Paper.

Chandra, Amitabh, Courtney Coile, and Corina Mommaerts. 2023. "What Can Economics Say about Alzheimer's Disease?" *Journal of Economic Literature*, 61(2): 428–470.

Christelis, Dimitris, Tullio Jappelli, and Mario Padula. 2010. "Cognitive Abilities and Portfolio Choice." *European Economic Review*, 54(1): 18–38.

Coe, Norma B., and Courtney Harold Van Houtven. 2009. "Caring for Mom and Neglecting Yourself? The Health Effects of Caring for an Elderly Parent." *Health Economics*, 18(9): 991–1010.

Conti, Gabriella, Rita Ginja, Petra Persson, and Barton Willage. 2025. "The Menopause 'Penalty.'" NBER Working Paper 33621.

Crespo, Laura, and Pedro Mira. 2014. "Caregiving to Elderly Parents and Employment Status of European Mature Women." *The Review of Economics and Statistics*, 96(4): 693–709.

Fadlon, Itzik, and Torben Heien Nielsen. 2021. "Family Labor Supply Responses to Severe Health Shocks: Evidence from Danish Administrative Records." *American Economic Journal: Applied Economics*, 13(3): 1–30.

Giovannetti, Erin R., and Jennifer L. Wolff. 2010. "Cross-Survey Differences in National Estimates of Numbers of Caregivers of Disabled Older Adults." *The Milbank Quarterly*, 88(3): 310–349.

Halla, Martin, Julia Schmieder, and Andrea Weber. 2020. "Job Displacement, Family Dynamics, and Spousal Labor Supply." *American Economic Journal: Applied Economics*, 12(4): 253–287.

Haller, Andreas, Stefan Staubli, and Josef Zweimüller. 2024. "Designing Disability Insurance Reforms: Tightening Eligibility Rules or Reducing Benefits?" *Econometrica*, 92(1): 79–110.

Hsu, Joanne W., and Robert J. Willis. 2013. "Dementia Risk and Financial Decision Making by Older Households: The Impact of Information." *Journal of Human Capital*, 7(4): 340–377.

Jeong, Yeongmi, Nicholas W. Papageorge, Meghan Skira, and Kevin Thom. 2024. "Genetic Risk for Alzheimer's Disease and Related Dementias: Cognition, Economic Behavior, and Actionable Information." NBER Working Paper 32181.

Jönsson, Linus. 2022. "The Personal Economic Burden of Dementia in Europe." *The Lancet Regional Health – Europe*, 20: 100472.

Korfhage, Thorben, and Björn Fischer-Weckemann. 2024. "Long-Run Consequences of Informal Elderly Care and Implications of Public Long-Term Care Insurance." *Journal of Health Economics*, 96: 102884.

Krekó, Judit, Dániel Prinz, and Andrea Weber. 2024. "Take-up and Labor Supply Responses to Disability Insurance Earnings Limits." *Labour Economics*, 89: 102583.

Langa, Kenneth M., Michael E. Chernew, Mohammed U. Kabeto, A. Regula Herzog, Mary Beth Ofstedal, Robert J. Willis, Robert B. Wallace, Lisa M. Mucha, Walter L. Straus, and A. Mark Fendrick. 2001. "National Estimates of the Quantity and Cost of Informal Caregiving for the Elderly with Dementia." *Journal of General Internal Medicine*, 16(11): 770–778.

Lenhart, Otto. 2019. "The Effects of Health Shocks on Labor Market Outcomes: Evidence from UK Panel Data." *European Journal of Health Economics*, 20(1): 83–98.

Leuven, Edwin, and Barbara Sianesi. 2003. "PSMATCH2: Stata Module to Perform Full Mahalanobis and Propensity Score Matching, Common Support Graphing, and Covariate Imbalance Testing." Statistical Software Components S432001, Boston College Department of Economics.

Lillard, Lee A., and Robert J. Willis. 1997. "Motives for Intergenerational Transfers: Evidence from Malaysia." *Demography*, 34(1): 115–134.

Løken, Katrine V., Shelly Lundberg, and Julie Riise. 2017. "Lifting the Burden: Formal Care of the Elderly and Labor Supply of Adult Children." *Journal of Human Resources*, 52(1): 247–271.

Maestas, Nicole, Matt Messel, and Yulya Truskinovsky. 2024. "Caregiving and Labor Supply: New Evidence from Administrative Data." *Journal of Labor Economics*, 42(S1): S183–S218.

Mazzonna, Fabrizio, and Franco Peracchi. 2024. "Are Older People Aware of Their Cognitive Decline? Misperception and Financial Decision-Making." *Journal of Political Economy*, 132(6): 1793–1830.

Meyer, Bruce D., Wallace K. C. Mok, and James X. Sullivan. 2015. "Household Surveys in Crisis." *Journal of Economic Perspectives*, 29(4): 199–226.

Mommaerts, Corina, and Yulya Truskinovsky. 2020. "The Cyclicality of Informal Care." *Journal of Health Economics*, 71: 102306.

Ramirez Lizardi, Eduardo, Elisabeth Fevang, Knut Røed, and Henning Øien. 2025. "Effects of Health Shocks on Adult Children's Labor Market Outcomes and Well-Being." *Health Economics*, 34(10): 1804–1820.

Rohwedder, Susann, and Robert J. Willis. 2010. "Mental Retirement." *Journal of Economic Perspectives*, 24(1): 119–138.

Shin, Su H., Dean R. Lillard, and Jay Bhattacharya. 2019. "Understanding the Correlation between Alzheimer's Disease Polygenic Risk, Wealth, and the Composition of Wealth Holdings." NBER Working Paper 25526.

Smith, James P., John J. McArdle, and Robert J. Willis. 2010. "Financial Decision Making and Cognition in a Family Context." *Economic Journal*, 120(548): F363–F380.

van Gameren, Edwin, and Durfari Velandia Naranjo. 2015. "Working and Caring: The Simultaneous Decision of Labor Force Participation and Informal Elderly and Child Support Activities in Mexico." *Latin American Journal of Economics*, 52(2): 117–148.

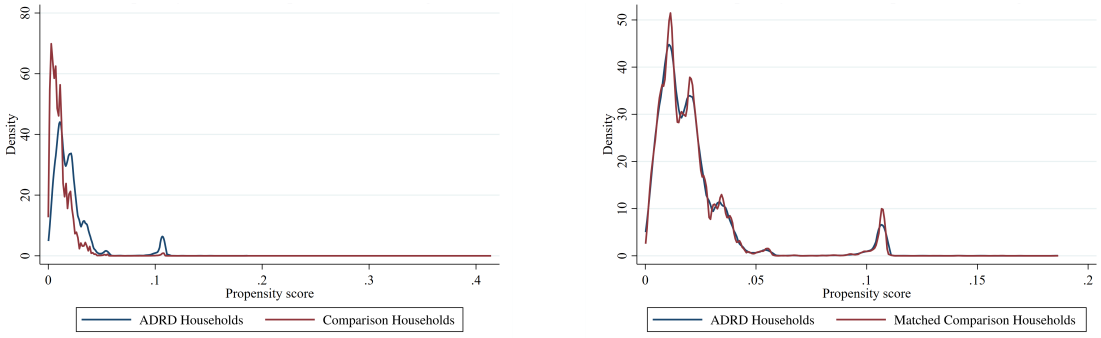
Van Houtven, Courtney Harold, Norma B. Coe, and Meghan M. Skira. 2013. "The Effect of Informal Care on Work and Wages." *Journal of Health Economics*, 32(1): 240–252.

World Health Organization. 2021. *Global Status Report on the Public Health Response to Dementia*. Geneva: World Health Organization.

A Additional Matching Diagnostics

Tables 4 and 5 compare means before and after matching. In the patient sample, matching removes nearly all of the age, earnings, and employment differences, and none of the remaining differences is statistically significant. Figure 11 shows that the treated and comparison propensity-score distributions, which differ before matching, nearly coincide after the matching weights are applied.

Figure 11: Propensity-Score Overlap between ADRD and Comparison Households



(a) Before matching (b) After matching

Notes: Each curve is the estimated distribution (kernel density) of the propensity score: the predicted probability that a household is an ADRD household, given its pre-diagnosis characteristics. Panel (a) compares all eligible households before matching; panel (b) weights comparison households by their matching weights. Curves that lie on top of each other mean the two groups have similar scores. This does not guarantee that every characteristic is balanced; Table 5 reports those comparisons.

B Variable Definitions

Formal employment An indicator equal to one when PILA records a social-security contribution associated with formal work in a month.

Formal earnings The sum of an individual's PILA-recorded formal wages across employers in a quarter, expressed in 2007 USD. Earnings equal zero in quarters without formal employment.

Share formally employed The share of a quarter in which an individual appears as formally employed in PILA.

Pre-period averages Pre-period earnings and the pre-period share formally employed average the quarterly measures over the four quarters before the index date (the diagnosis date for patients).

Stable formal employment A household has a stably employed member if at least one working-age member is formally employed in at least 3 of the 4 quarters preceding the index quarter.

Members ever observed in PILA The number of working-age household members who appear in the PILA formal-employment records at any point.

First recorded ADRD diagnosis The earliest RIPS encounter containing a qualifying Alzheimer's disease or related dementia diagnosis code.

Comparison unit An individual or household that is not observed with an ADRD diagnosis in RIPS and satisfies the relevant observation requirements in a treated unit's calendar-time risk set.

Household formal earnings The quarterly formal earnings among working-age members of the SISBEN household who can be linked to PILA.