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Hazed and Confused: The Effect of Air Pollution on Dementia

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We study whether long-term cumulative exposure to airborne small particulate matter ($PM_{2.5}$) affects the probability that an individual receives a new diagnosis of Alzheimer's disease or related dementias. We track the health, residential location, and $PM_{2.5}$ exposures of Americans aged sixty-five and above from 2001 through 2013. The expansion of Clean Air Act regulations led to quasi-random variation in individuals' subsequent exposures to $PM_{2.5}$. We leverage these regulations to construct instrumental variables for individual-level decadal $PM_{2.5}$ that we use within flexible probit models that also account for any potential sample selection based on survival. We find that a 1 $\mu g/m^3$ increase in decadal $PM_{2.5}$ increases the probability of a new dementia diagnosis by an average of 2.15 percentage points (pp). All else equal, we find larger effects for women, older people, and people with more clinical risk factors for dementia. These effects persist below current regulatory thresholds.

Key words: Air pollution, Particulate matter, Dementia, Alzheimer's disease

JEL Codes: I18, Q53

1. INTRODUCTION

Research shows that airborne particulate matter (PM) increases mortality. This effect persists around the world and over time, from the historically high exposures in London in the 1960s (McMillan and Murphy, 2017) and China in the 2000s (Li *et al*., 2019) to the historically low exposures in the U.S. in the 2000s (Deryugina *et al*., 2019). Research also shows that air pollution constrains the production and the productivity of human capital (Graff-Zivin and Neidell, 2013). For instance, daily pollution spikes have been found to reduce students' scores on high-stakes exams (Ebenstein *et al*., 2016). Among working-age adults, daily pollution spikes have been found to reduce performance of both manual and cognitive tasks (Chang *et al*., 2016; Archsmith *et al*., 2018). However, prior research has not applied causal methods to evaluate whether airborne PM degrades human capital later in life apart from mortality.

Our study is the first to use a causal research design to evaluate whether long-term, later-in-life exposure to airborne small particulates (*i.e.* $PM_{2.5}$, particulates smaller than 2.5 microns in diameter) plays a role in causing dementia. Medical research has documented associations between long-term, later-in-life exposure to $PM_{2.5}$ specifically and the probability of individuals

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receiving a new dementia diagnosis; although as with other suspected causes of dementia, the precise mechanisms remain unknown (Block *et al*., 2012; Underwood, 2017; Peters *et al*., 2019). Furthermore, these associations may not be causal due to omitted variables, errors in measuring individuals' pollution exposures, or selection bias.

We develop a research design to account for potential biases due to prior residential sorting (driven by pollution, health, and/or preferences), measurement error in pollution, and selection on survival. Specifically, we estimate the effects of individuals' later-in-life exposure to $PM_{2.5}$ for up to a decade, the longest duration of quasi-random variation available to us. This conditionally exogenous variation resulted from the Environmental Protection Agency's (EPA) expansion of the Clean Air Act (CAA). Based on air quality monitor readings from 2001 to 2003, the EPA began to enforce a maximum threshold on $PM_{2.5}$, prompting local regulators to cleanup polluted areas beginning in 2004. The regulatory incentives for cleanup were larger in nonattainment counties that exceeded the maximum threshold on $PM_{2.5}$. The incentives caused differences within counties as well. As a result, individuals with the same $PM_{2.5}$ exposures from 2001 to 2003 experienced different $PM_{2.5}$ exposures over the next decade.

We use this individual-level variation from the EPA's nonattainment designations as instruments to identify how cumulative $PM_{2.5}$ exposure from 2004 to 2013 affected the probability of receiving a new diagnosis of dementia during this period among Medicare beneficiaries aged sixty-five and above who did not have dementia in 2004. Specifically, we use county nonattainment status flexibly interacted with individual-level $PM_{2.5}$ from 2001 to 2003 as instruments for the individual's cumulative $PM_{2.5}$ exposure from 2004 to 2013. In addition to addressing bias from omitted variables, including genetics, earlier-in-life exposure, and other latent risk factors for dementia, our estimators also address the inevitable error in measuring an individual's pollution exposure.

We apply this design to thirteen years of individual-level data on a random sample of millions of Americans aged sixty-five and above. These data track their diagnosis dates for many illnesses including Alzheimer's disease and related dementias, their demographics, and their sequence of residential addresses from 2001 through 2013. We use these residential addresses to link to measures of individual-level $PM_{2.5}$ exposure using data from EPA air quality monitors.

We estimate year-specific probit models that allow for heterogeneity in the effects of $PM_{2.5}$ across individuals and across exposure duration while flexibly controlling for individual characteristics associated with dementia risk, including race, gender-by-integer-age interactions, baseline medical expenditures, baseline exposure to $PM_{2.5}$, fully interacted sets of baseline medical conditions, and the socioeconomic composition of individuals' baseline neighbourhoods (defined as a U.S. Census block group). Furthermore, we include core-based statistical area (CBSA) fixed effects to absorb spatial variation in diagnostic standards, health care quality and access, and latent environmental quality. Finally, we account for the fact that our main estimation sample is limited to individuals who survived through the model year following Heckman (1979). Specifically, we estimate the probability of survival in a separate first stage, using additional instruments constructed from data on individuals' diagnoses of cancers that, based on medical literature, are unrelated to dementia.

We find that a 1 $\mu g/m^3$ increase in average $PM_{2.5}$ concentrations increases the probability of receiving a new dementia diagnosis by the end of the decade by an average of 2.15 percentage points (pp). For reference, a 1 $\mu g/m^3$ increase in average $PM_{2.5}$ was 9.1% of the decadal mean and 59% of the decadal standard deviation during the period 2004–13. The estimated marginal effects are larger at lower levels of $PM_{2.5}$. We also find that the estimated marginal effects of $PM_{2.5}$ increase with age, illness, and duration of exposure, and that they are larger for women relative to men and larger for Black or African-American individuals relative to non-Hispanic White individuals.

We conduct additional analyses to explore the possibility that nonattainment designations are conditionally associated with unobserved earlier-in-life factors that cause dementia, which would violate the exclusion restriction assumption of our instrumental variables. First, we estimate a model with dementia in 2004 as the outcome. The point estimate is negative, small in absolute value, and statistically indistinguishable from zero. This suggests that our model is unlikely to be confounded by unobserved differences in earlier-in-life or other factors that contribute to differences in dementia diagnoses and are conditionally associated with our instruments. Second, we evaluate other placebo health outcomes that may be linked to earlier-in-life factors but have no known link to $PM_{2.5}$. We do not find a relationship between these placebo outcomes and individuals' cumulative $PM_{2.5}$ exposures.[1] Third, our results persist across a wide range of alternative modelling decisions including controlling for ancillary measures of air pollution exposure.

These findings indicate that air pollution's effects on dementia make its detriments to health and human capital substantially larger than previously realised. Incorporating these effects will be important for comprehensively evaluating the ongoing efforts to improve air quality worldwide.

2. LATER-IN-LIFE $PM_{2.5}$ EXPOSURE AND NEW DEMENTIA DIAGNOSES

2.1. *Existing knowledge from the medical literature*

Recent research has documented a positive association between long-term cumulative exposure to fine-particulate air pollution later in life and dementia (Block *et al*., 2012; Underwood, 2017; Peters *et al*., 2019). In addition, the literature has identified a number of potential pathways to explain this association, even if the details of the accumulation process remain yet unknown. Two physiological hallmarks of Alzheimer's disease specifically are the accumulation of tau protein and amyloid beta (Iaccarino *et al*., 2021), and recent research has established a link between this accumulation and $PM_{2.5}$ exposure (Park *et al*., 2021). Research has also found relationships consistent with other potential neurological mechanisms underlying the link between $PM_{2.5}$ and dementia and/or Alzheimer's disease (Alemany *et al*., 2021), including neuroinflammation caused by accumulation of $PM_{2.5}$ in brain tissue (Maher *et al*., 2016; Kang *et al*., 2021), and associations between long-term, later-in-life exposure to $PM_{2.5}$ and accumulated $PM_{2.5}$ in the brain, smaller brain volume, and higher rates of brain infarcts or areas of necrosis and accelerated rates of brain atrophy, which is predictive of Alzheimer's disease (Wilker *et al*., 2015; Younan *et al*., 2020).

Each of these potential pathways between cumulative $PM_{2.5}$ exposure and a diagnosis of dementia is potentially moderated by a number of factors. These factors may include differences in $PM_{2.5}$ chemical composition (Li *et al*., 2020), earlier-in-life exposure, cardiovascular risk (Grande *et al*., 2020), and genetics. While less than half of the genetic factors that contribute to late-onset dementia have been identified (Ridge *et al*., 2016), recent research has found that genes play a role in moderating environmental factors' relationship to cognitive decline and dementia, including moderating the relationship between $PM_{2.5}$ and dementia, specifically (Cacciottolo *et al*., 2017; Kulick *et al*., 2020; Alemany *et al*., 2021).[2]

1. In contrast, we find statistically significant positive effects for two outcomes with known links to $PM_{2.5}$ (chronic obstructive pulmonary disease (COPD) and chronic kidney disorder).

2. These issues make it difficult to allocate the shares of dementia cases due to genetic risk factors for dementia itself and due to environmental factors directly. Earlier research (*e.g.* Gatz *et al*., 1997) provided such shares under the strong assumption of additive separability between environmental factors and genetics.

2.2. *An overview of our research design*

The medical literature described above, along with our data and policy setting, described in Sections 3 and 4, respectively, inform several aspects of our research design. We preview this research design here.

We follow prior medical studies and assess the role of later-in-life, long-term exposure to $PM_{2.5}$ as measured by single- or multiple-year annual average ambient concentrations in explaining new diagnoses of dementia (Cacciottolo *et al*., 2017; Grande, 2020; Shi *et al*., 2020; Mortimais *et al*., 2021; Ran *et al*., 2021; Shi *et al*., 2021; Li *et al*., 2022; Wang *et al*., 2022).[3] Specifically, we observe the timing of individuals' initial diagnosis (or lack thereof) and how it relates to thirteen years (2001–13) of annual average exposure to $PM_{2.5}$ for them individually based on their precise residential locations each year, allowing us to measure individual-specific exposure histories.[4]

We depart from the prior medical literature by employing a causal research design to account for potential sources of confounding. Specifically, we observe quasi-random variation in individuals' $PM_{2.5}$ exposures beginning in 2004. As a result, we are able to model the effects of $PM_{2.5}$ exposure across a full decade (2004–13), conditional on baseline levels of $PM_{2.5}$ (2001–3).

In Section 5, we present a flexible probit model of how cumulative exposure to $PM_{2.5}$ affects the probability of an individual receiving a new dementia diagnosis. We allow for heterogeneity by letting this effect vary flexibly with the level of cumulative $PM_{2.5}$ exposure over the sample and with the levels of the other controls. We feature models using increasing durations of $PM_{2.5}$ exposure. Finally, we include an extensive set of individual and neighbourhood characteristics that may be correlated with new dementia diagnoses. These controls are described in detail in Section 3.

Even with this extensive set of controls, identifying the effect of cumulative $PM_{2.5}$ exposure on a new diagnosis of dementia presents several challenges. These include scope for measurement error in $PM_{2.5}$ exposure, the potential for sorting on latent health, genetics, and earlier-in-life pollution exposures, and selection on survival. Our econometric approach, described in Section 5, is designed to account for each of these challenges.

First, to address measurement error in $PM_{2.5}$ exposure and any geographic differentials in unobserved factors, we follow prior work (Chay and Greenstone, 2005; Auffhammer *et al*., 2009) and develop instrumental variables from the quasi-random variation in $PM_{2.5}$ exposures (conditional on baseline) that was induced by the CAA regulations. Our control function approach (Rivers and Vuong, 1988) relies on the familiar assumptions of relevance and exogeneity for two-stage least squares (2SLS). The policy environment and the variation-inducing CAA regulations are described in detail in Section 4.

Second, to address selection based on survival, we employ a selection-correction approach (Heckman, 1979; Heckman and Robb, 1986). To implement this approach, we use a set of additional instruments from the medical literature that are correlated with survival, but independent of the unobserved determinants of dementia. We also present a Lee (2009)-style bounds approach in Supplementary Material, Appendix H that does not rely on this additional set of instruments.

3. Like nearly all of the large-scale studies using secondary data, we cannot observe progression or severity of dementia over time. Clinical research commonly refers to this as "incident dementia" or "incidence of dementia." Peters *et al*. (2019) provide a review.

4. Dementia is an absorbing state. Therefore, we model the occurrence of the initial diagnosis and exclude from our sample those who had been diagnosed previously.

In addition, we consider the potential for sorting on genetics and omitted earlier-in-life factors. Prior research found that individuals' residential exposures to $PM_{2.5}$ do not differ by apolipoprotein E (APOE) genotypes (Cacciotolo *et al*., 2017). In addition, Shin *et al*. (2019) find "no correlation between Alzheimer's Disease polygenic risk score and net worth, housing assets and nonfinancial assets." This indicates that dementia-related genetics are not associated with sorting into neighbourhoods based on economic status. These studies provide evidence that genetic factors are unlikely to be correlated with our instrument. To test this directly, we examine the estimates of instrumented $PM_{2.5}$ exposure on the presence of a dementia diagnosis by 2004. In addition to genetics, this assesses whether our results are likely to be explained by association between our instruments and any omitted earlier-in-life factors including other clinical risk factors, prior exposure to $PM_{2.5}$, or different chemical compositions of $PM_{2.5}$.

3. DATA AND MEASURES

3.1. *Medicare data and sample*

The U.S. Medicare programme provides universal health insurance for citizens over age sixty-five.[5] The U.S. Centers for Medicare and Medicaid Services (CMS) maintains a comprehensive national database on beneficiaries, including their addresses at each point in time, medical claims and diagnoses, and demographics. We track individuals from as early as 1999 through the end of 2013.[6] Our featured estimation sample starts with a random 20% sample of all traditional Medicare (TM) beneficiaries who were sixty-five and older on 1 January 2004. We then limit our sample to those who lived in counties with $PM_{2.5}$ monitors, and for whom we can observe their health and residential locations.[7]

3.2. *Measuring dementia and its risk factors*

CMS's Chronic Conditions Data Warehouse (CCW) files use codes on Medicare insurance claims to track if and when each individual is diagnosed with specific chronic medical conditions. A dementia diagnosis is based on the presence of multiple symptoms of cognitive impairment that significantly impact daily functioning.[8] Examples include memory loss, impaired judgement, loss of spatial awareness, depression, and behavioural changes. Alzheimer's disease is the primary type of dementia, accounting for 60–80% of all cases. Our claims-based approach to identifying dementia diagnoses is well validated (Lee *et al*., 2019).

5. We analyse "traditional" Medicare (TM) administrative records from CMS. CMS manages and pays claims for services provided to TM enrollees. Beneficiaries can opt out of TM and enroll in a private Medicare Advantage (MA) managed care plan. MA enrollees are left out of most studies of Medicare because MA plans historically did not report claims to CMS. We are able to overcome these limitations and include MA enrollees in some specifications described in Supplementary Material, Appendix Table J2.

6. Due to the provenance of our data, we complement the random 20% sample with an independent, random 20% sample of those also aged sixty-five by 1 January 2004 who purchased standalone prescription drug insurance plans through Medicare Part D at any point between 2006 and 2010 without the aid of low-income subsidies (Center for Medicare and Medicaid Services 2022a, 2022b, 2022c).

7. We provide additional details about sample cuts and data definitions in Supplementary Material, Appendix A.

8. The International Classification of Diseases, 10th Revision (ICD-10) (World Health Organization (2011) defines Alzheimer's disease (G30) as "A degenerative disease of the brain characterized by the insidious onset of dementia. Impairment of memory, judgment, attention span, and problem solving skills are followed by severe apraxias and a global loss of cognitive abilities. The condition primarily occurs after age 60, and is marked pathologically by severe cortical atrophy and the triad of senile plaques; neurofibrillary tangles; and neuropil threads."

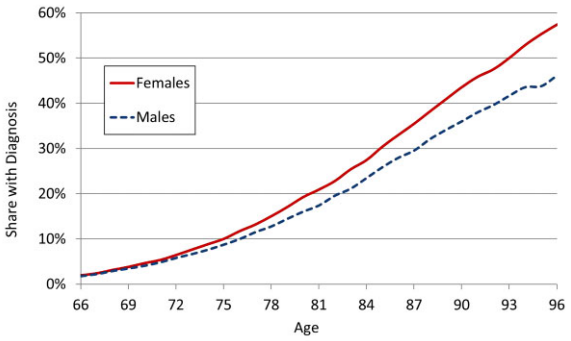

FIGURE 1
Dementia diagnosis by age and gender in 2013.

Figure 1 shows how the fraction of individuals diagnosed with dementia in Medicare data varies with age and gender in 2013. Diagnosis rates increase gradually with age through the mid-70s before accelerating in the late 70s and beyond. The diagnosis rate is higher for women, and this gender gap widens with age. Conditional on age, diagnosis rates also differ by race. Diagnosis rates are generally higher for people denoted by CMS as "Black or African-American" and lower for "Asian/Pacific Islander" relative to "Hispanic" or "non-Hispanic White." We account for this heterogeneity by creating a vector of demographics, denoted X_i. This vector includes race code indicators and indicators for each of the 52 possible sex-by-integer-age combinations from age 75 through 100 in 2013.[9]

We further utilise the administrative CCW files to measure clinical risk factors. Specifically, we create a vector of health characteristics, denoted H_i. This includes indicators for whether the individual in 2004 had each one of the 32 possible combinations of hypertension, diabetes, congestive heart failure, ischaemic heart disease, and stroke. These are the known diagnostic risks for dementia (Alzheimer's Association, 2019). We further measure baseline health by including in H_i a fourth-order polynomial function of total expenditures on all services covered by Medicare Parts A and B in 2004.[10]

U.S. Census data provide socioeconomic characteristics of the Census block group where the individual lived in 2004 according to CMS records.[11] We define neighbourhood as the individual's Census block group and create a vector of neighbourhood characteristics, denoted W_i. This vector includes median household income, per-capita income, mean and median house value, median rent, median house age, fractions of the housing stock that are owner occupied, renter occupied, and vacant, fraction of residents over age 65, fractions of residents who report being White, Black, and Hispanic, and the fractions of residents in each of seven educational-attainment bins.[12] These variables account for non-clinical factors associated with different risks

9. Seventy-five is the minimum age in 2013 within our estimation sample because that sample is limited to people who were sixty-five or older on 1 January 2004. Centenarians are grouped into two gender-specific bins because their small numbers prevent us from precisely estimating age-specific coefficients. Our results are unaffected by adding age-specific bins beyond age 100.

10. Medicare Parts A and B cover virtually all medical services aside from prescription drugs and long-term care. This includes doctors' services, preventive care, durable medical equipment, hospital outpatient services, laboratory tests, imaging, hospital inpatient services, nursing facilities, and hospice care.

11. A block group contains 600–3000 residents on average (U.S. Census).

12. United States Census Bureau (2022) and Geolytics (2022).

of dementia. Supplementary Material, Appendix Table A1 provides summary statistics for each of the variables represented in X_i, H_i, and W_i.

Finally, we create indicators (denoted $C_{i,t}$) for the geographic regions where individuals lived in each year of our model. Specifically, we include 977 indicators for the U.S. Census Bureau's CBSAs and the non-CBSA rural areas of each state.[13] In our model, these indicators will absorb the effects of otherwise unobserved factors. First, they help to absorb any effects of residential sorting across CBSAs on the basis of latent risk factors for dementia. Second, they help to absorb the effects of environmental factors that could be spatially correlated with both $PM_{2.5}$ and dementia, *e.g.* the presence of lead pipes or extreme temperatures which may cause morbidities that are risk factors for dementia. Third, they absorb all differences between geographic areas in health care delivery that might contribute to differences in diagnostic decisions, including patients' access to medical care and physicians' treatment styles.

3.3. *Measuring $PM_{2.5}$ exposure*

In 1997, the EPA established monitoring protocols for $PM_{2.5}$, and by 1999, an initial national network of regulatory-grade $PM_{2.5}$ monitors was put into place. We use annual average $PM_{2.5}$ concentrations recorded at each of these monitors from 2001 through 2013 (United States Environmental Protection Agency 2022a, 2022b). We use data from a balanced panel of 485 monitors that operated continuously through our study period to avoid measurement error that could be introduced if new monitors tend to be located in more or less polluted areas (Grainger and Schreiber, 2019).[14] In a sensitivity check, we instead use data from all 1722 monitors.

We measure an individual's exposure to $PM_{2.5}$ in year t, $PM2.5_{i,t}$, based on concentrations at their residential address in that year. The CMS data include ZIP + 4 Codes for each individual's sequence of addresses from 2004 to 2013.[15] We use this information to measure the individual's cumulative exposure to $PM_{2.5}$ incorporating changes in $PM_{2.5}$ experienced as a result of moving.[16] Individuals in our data live in 2.7 million distinct ZIP + 4 Codes during 2004–13. We use the latitude and longitude coordinates of each monitor and each ZIP + 4 to assign the annual average concentration at each residence.[17] Specifically, we calculate the geographical distance between each ZIP + 4 centroid and each monitor. Then, for each centroid-year combination, we calculate a weighted average of concentrations recorded at all monitors with the weights given

13. There are 927 CBSAs in the U.S., which are defined by the Office of Management and Budget as one or more counties anchored by an urban center of at least 10,000 people plus adjacent counties that are socioeconomically tied to the urban center by commuting. For people living outside of CBSAs, we create an additional 50 state-specific, rural dummy variables.

14. Following the literature, we drop individuals living in unmonitored counties. See Supplementary Material, Appendix A for details.

15. ZIP + 4 Codes are close to street addresses in terms of spatial precision: each code corresponds to a single mail delivery point such as a house, one floor of an apartment building, or one side of a street on a city block.

16. Thirty-one percent of individuals in our data move at least once, 17% move between counties, and 10% move between states. These rates are similar to those reported by the Census Bureau for individuals aged sixty-five and above. We are unable to observe seasonal migration by people with more than one residence because we only observe the residential address on record with CMS. Fortunately, the scope for measurement error is small. Jeffery (2015) estimates that seasonal migrators only account for 2–4.1% of the Medicare population based on addresses on Medicare claims for primary care and emergency room visits.

17. Geographic coordinates of ZIP + 4 centroids were purchased from GeoLytics, which created them from the Census Bureau's TIGER/line Shapefiles and U.S. Postal Service records.

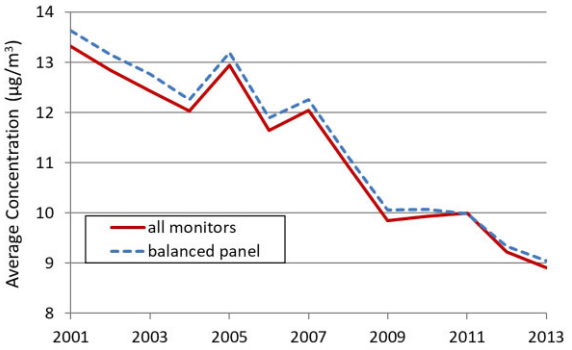

FIGURE 2
Average residential concentration of $PM_{2.5}$ by year

Notes: The figure reports the annual average concentrations of fine-PM based on place of residence for people aged sixty-five and above on Medicare.

by the square of the inverse distance.[18] Thus, as the distance from a ZIP + 4 centroid to a monitor increases, the weight assigned to that monitor decreases.

Figure 2 shows that annual average concentrations of $PM_{2.5}$ at the residences of the U.S. Medicare population declined substantially during the 2000s, from over 13 $\mu g/m^3$ (micrograms per cubic metre of air) in 2001 to about 9 $\mu g/m^3$ in 2013. This is true regardless of whether we measure exposure using the balanced panel of monitors (the dashed line) or the full set of monitors (solid line).

We denote our measure of interest, the individual's average cumulative exposure to $PM_{2.5}$ from 2004 to year t, as $\text{durPM}_{i,t}$. We construct it by combining the described ZIP + 4-specific annual $PM_{2.5}$ concentrations with individuals' residential ZIP + 4 histories from 2004 to t according to $\text{durPM}_{i,t} = \sum_{s=2004}^{t} \text{PM2.5}_{i,s}/(t - 2004)$. Finally, we create a measure of the baseline $PM_{2.5}$ concentrations at the locations where individuals lived in 2004. We denote this measure as basePM_i and construct it as the average concentration over the three years 2001–3. These three years are the years that the EPA based its nonattainment designations on, as discussed in the next section.

4. CAA REGULATION OF $PM_{2.5}$

The CAA of 1970 established national standards for concentrations of regulated air pollutants. The EPA designated counties containing monitors that exceeded these standards as nonattainment. States with nonattainment counties were required to coordinate with local regulators to bring those counties into compliance with the standards. States that failed to bring their counties into attainment faced penalties including loss of federal highway funds.

Due to its pernicious effects on human health, PM has been subject to sustained and evolving federal regulation (US EPA, 2005). Beginning in 1971, the EPA regulated total suspended particulates (TSPs). In light of evidence that health effects were driven by the smallest particulates, the EPA replaced the TSP standard with a standard on PM_{10} (particulates smaller than 10 microns in diameter) in 1987 and a standard on $PM_{2.5}$ in 1997. Each new standard was followed

18. This method of interpolation, with weights given by the distance raised to a negative exponent, is a predominant method in the environmental economics literature.

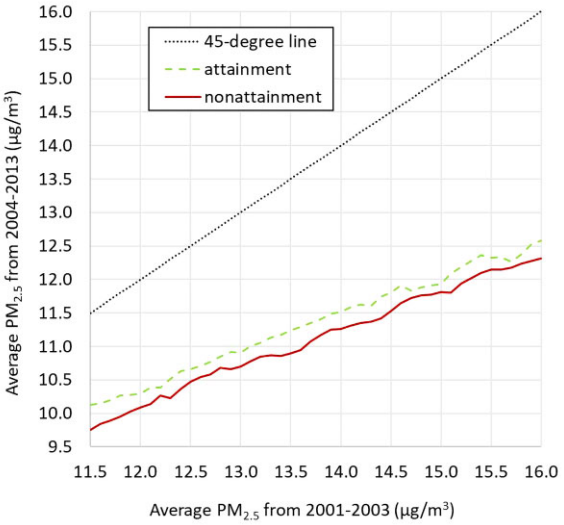

FIGURE 3

Post-regulatory $PM_{2.5}$ exposure 2004–13, by county attainment status and pre-regulatory exposure 2001–3

Notes: The nonattainment and attainment lines represent estimates from regressing individual exposure from 2004 to 2013 on indicators for 0.10 $\mu g/m^3$ bins of baseline exposure from 2001 to 2003 interacted with county attainment status. Additional covariates include CBSA dummies.

by new nonattainment designations. These designations have the ability to affect pollution in both nonattainment and attainment counties because pollution travels across county boundaries. However, the designations for PM have induced relatively larger pollution reductions in nonattainment counties. Prior research used the TSP standard (Chay and Greenstone, 2005; Isen *et al.*, 2017) and the PM_{10} standard (Bento *et al.*, 2015) to create instruments for TSP and PM_{10} exposures, respectively. In this paper, we use the $PM_{2.5}$ standard to develop instruments for $PM_{2.5}$ exposures.

In 1997, the EPA set the regulatory standard for average annual $PM_{2.5}$ concentrations at 15.05 $\mu g/m^3$. In April 2003, state and local regulators were given a February 2004 deadline to provide $PM_{2.5}$ monitor data from the years 2001 to 2003 and to self-report any nonattainment monitors to the EPA, where nonattainment was defined by the monitor's three-year average $PM_{2.5}$ concentrations from 2001 to 2003. Based on these reports, the EPA formally defined each monitored county to be in attainment or nonattainment in January 2005.[19] For counties with multiple monitors, the designations were based on the monitor with the highest three-year average from 2001 to 2003.

We define 2004 as the start of the regulatory period because local regulators learned which counties would be nonattainment between April 2003, when they received the EPA's request for data, and February 2004, when they were required to submit their status. EPA monitor data show $PM_{2.5}$ concentrations declining at a similar rate in both attainment and nonattainment counties prior to 2004, and then declining at a faster rate in nonattainment counties after 2004. These

19. Supplementary Material, Appendix Figure B1 shows the locations of attainment and nonattainment counties with air quality monitors. In 2005, 132 of the monitored counties containing approximately 27% of the U.S. population were classified as nonattainment. Another 528 counties containing 43% of the U.S. population were classified as attainment. The remaining counties lacked monitoring data and were designated "unclassifiable" and not subjected to additional regulation. Supplementary Material, Appendix Figure B2 shows the location of the monitors.

trends, shown in Supplementary Material, Appendix Figure C1, are analogous to the evidence that Chay and Greenstone (2005) first presented on the validity of using CAA regulation of PM as a quasi-experiment.

Figure 3 provides the intuition for how we use county nonattainment designations to isolate quasi-random variation in individuals' average $PM_{2.5}$ exposures from 2004 to 2013, conditional on baseline concentrations from 2001 to 2003.[20] The nonattainment and attainment lines plot the coefficients obtained by regressing the individual-level measure of decadal $PM_{2.5}$ exposure, $durPM_{i,2013}$, on indicators for 0.1 $\mu g/m^3$ bins of $basePM_i$ interacted with county attainment status, after absorbing CBSA dummies. Comparing the nonattainment and attainment lines with the 45° line shows that post-regulatory reductions in $PM_{2.5}$ were larger, on average, for individuals with larger baseline concentrations. This pattern is consistent with prior studies that used CAA regulatory standards to develop instruments for PM exposures.

The key insight from Figure 3 is that the nonattainment line lies below the attainment line for all levels of average $PM_{2.5}$ from 2001 to 2003. This difference is statistically significant at the 1% level. This shows that when we compare individuals in the same CBSA who were in the same residential $PM_{2.5}$ bin for pre-regulatory exposure (2001–3), those who lived in nonattainment counties were subsequently exposed to *lower* $PM_{2.5}$ during 2004–13 than those in attainment counties. This follows from the incentives that regulators faced to target their mitigation efforts at nonattainment counties (Chay and Greenstone, 2005; Isen *et al*., 2017). In addition, the vertical distance between the nonattainment and attainment lines decreases with baseline $PM_{2.5}$ concentrations from 2001 to 2003.[21] This follows from the EPA policy in which a county's attainment status is linked to its dirtiest monitor, thus incentivizing local regulators to target pollution "hot spots" (Auffhammer *et al*., 2009; Bento *et al*., 2015).

5. ESTIMATING THE CAUSAL IMPACT OF DECADAL $PM_{2.5}$ ON DEMENTIA

We model how cumulative exposure to $PM_{2.5}$ over the decade from 2004 to 2013 affects the probability of an individual receiving a new dementia diagnosis. First, we consider a contemporaneous, decadal model where the decade is treated as a single time period. Second, we extend this framework to instead aggregate cumulative, year-specific impacts over the decade.

5.1. *Decadal model of new dementia diagnoses*

Let $y_{i,t}$ indicate whether individual i has received a dementia diagnosis by the end of year t and let $\Delta y_i = y_{i,2013} - y_{i,2004}$ denote the change in dementia status between 2004 and 2013. Because dementia has no cure, it is an absorbing state and, by definition, Δy_i is equal to zero for anyone with dementia in 2004. Therefore, we model whether individual i is *newly* diagnosed with dementia by the end of 2013, conditional on having not received a dementia diagnosis before the end of 2004.[22]

20. As noted in Chay and Greenstone (2005), attainment status does not induce quasi-random variation in pollution levels, but rather quasi-random variation in changes in pollution. Equivalently, in our case, attainment status induces quasi-random variation in decadal pollution exposure, conditional on pre-regulatory baseline pollution.

21. The scaling of the vertical axis in Figure 3 makes this trend hard to discern. It is easier to discern in Figure 4. Fitting a linear trend to the vertical distance between the nonattainment and attainment lines in Figure 3 reveals that a 1 $\mu g/m^3$ increase in baseline exposure is associated with a 0.02 $\mu g/m^3$ reduction in the vertical distance between the lines.

22. We begin with a model of new diagnosis of dementia, which is standard in clinical research on dementia. In principle, we could instead begin with a model describing an individual's dementia status in both 2004 and 2013 to derive equation (4.1). Such a model is shown in Supplementary Material, Appendix G. Our discussion of identification below

We model a new dementia diagnosis using a probit model where Δy_i^* denotes the latent propensity to become newly diagnosed with dementia:

$$\Delta y_i^* = h(\text{durPM}_{i,2013}; \alpha_i) + \eta_i,$$

and where an individual is diagnosed with dementia if their latent propensity is positive, *i.e.* $\Delta y_i = 1[\Delta y_i^* > 0]$.

The parameter of interest, α_i, represents the causal effect of decadal exposure to $PM_{2.5}$ on Δy_i^*, holding all other factors constant.[23] All other factors that determine Δy_i^* are denoted by the error η_i. Following Angrist and Pischke (2009), we decompose η_i into a linear function of observable controls, X_i, H_i, W_i, C_i, basePM_i, and an error, e_i:

$$\eta_i = \beta_x X_i + \beta_H H_i + \beta_W W_i + \beta_C C_{i,2013} + f(\text{basePM}_i; \beta_{\text{basePM}}) + e_i.$$

Combining the two previous equations yields our equation of interest:

$$\Delta y_i^* = h(\text{durPM}_{i,2013}; \alpha_i) + \beta_x X_i + \beta_H H_i + \beta_W W_i + \beta_C C_{i,2013} + f(\text{basePM}_i; \beta_{\text{basePM}}) + e_i. \tag{4.1}$$

In the simplest specification of this model, we specify $h(\text{durPM}_{i,2013};\alpha_i) = \alpha\ \text{durPM}_{i,2013}$. In a more flexible specification of the decadal model, discussed in Section 5.2, we allow for non-linearities and heterogeneity along observable dimensions in the impact of $\text{durPM}_{i,2013}$ on the probability of a new diagnosis of dementia. In Section 5.3, we present a model that allows for additional non-linearity and heterogeneity with respect to the duration of exposure to $PM_{2.5}$.

We use α together with the other model parameters to recover the average marginal effect (AME) of changes in $\text{durPM}_{i,2013}$ on the probability of a new diagnosis, $\text{Prob}(\Delta y_i = 1)$. We discuss the controls, X, H, W, C, basePM, and the error, e, in the following paragraphs.

In Section 3, we defined the vectors of controls X, H, W, and C. The vector X_i includes indicators for race and gender-specific indicators for each integer age. H_i includes indicators for each unique combination of pre-existing clinical risk factors for dementia (hypertension, diabetes, congestive heart failure, ischaemic heart disease, and stroke) and a fourth-order polynomial function of individual medical spending in 2004. W_i includes Census block group variables describing the socioeconomic characteristics of individuals living in individual i's neighbourhood in 2004. Finally, $C_{i,2013}$ is a vector of indicators for each individual's 2013 CBSA.

The final control is a fourth-order polynomial function, $f(\cdot)$, of basePM_i. This controls for any residual effects of pre-regulatory sorting into more polluted neighbourhoods by individuals who are more likely to receive a future dementia diagnosis. In addition, the inclusion of $f(\text{basePM}_i)$ means that α specifically measures how cumulative $PM_{2.5}$ exposure from 2004 to 2013 affects the probability of a new dementia diagnosis, conditional on pre-regulatory, baseline concentrations.

Finally, e_i is an error term that represents any other determinants of a new dementia diagnosis that are not controlled for by a linear function of X_i, H_i, W_i, $C_{i,2013}$, and $f(\text{basePM}_i)$. The model imposes no assumption about the relationship between our variable of interest, $\text{durPM}_{i,2013}$,

explicitly accounts for the fact that the error in equation (4.1) captures changes in unobserved dementia determinants, conditional on not having dementia in 2004.

23. Epidemiological "stress" models that consider life histories are discussed in Deaton and Paxson (1998).

and e_i.[24] In fact, e_i most likely contains factors that would lead it to be correlated with $\text{durPM}_{i,2013}$, coming from (i) omitted variables, (ii) measurement error, and (iii) factors related to selection.

One example of an omitted variable in e_i that may be correlated with $\text{durPM}_{i,2013}$ is earlier-in-life exposure. While we do not specify the direct impact of earlier-in-life $PM_{2.5}$ exposure, we allow for earlier-in-life exposure to affect new dementia diagnoses and to be correlated with $\text{durPM}_{i,2013}$.[25] Another example is latent health. If individuals had sorted on unobserved health factors, including genetics, the error term and $\text{durPM}_{i,2013}$ may be correlated. Like earlier-in-life exposure, we will not specify the direct impacts of these latent health measures, but we do not rule out their presence in e_i.

Measurement error in $\text{durPM}_{i,2013}$ could also be present in e_i. All large-scale data on air pollution are based on ambient measures, such as satellite imaging or government monitors. While the regulatory-grade monitors that we use are well validated, each one only measures pollution at a single place.[26] As a result, all available measures of pollution likely differ from what individuals actually breathe. This can arise from individual differences in indoor air, daily mobility, and activities, or from the interpolation between geography-based measures required to develop individual-level measures.

Finally, new dementia diagnoses are only measured for those individuals who survive until the end of the model's time period. This could induce a correlation between $\text{durPM}_{i,2013}$ and e_i among survivors if latent health that determines survival is (conditionally) correlated with latent health that affects the probability of a new dementia diagnosis.

5.2. *Identification and estimation*

Relevant omitted variables, measurement error, and sample selection mean that estimating equation (4.1) under the assumption that $\text{durPM}_{i,2013}$ and e_i are independent is unlikely to yield a consistent estimate of α. We use a two-pronged approach to overcome these challenges. First, to address omitted variables and measurement error in $\text{durPM}_{i,2013}$, we leverage the conditional variation in $\text{durPM}_{i,2013}$ across individuals that was induced by CAA regulations as described above. Second, to address selection based on survival, we employ a selection-correction approach.

Instrumenting for pollution. As discussed in Section 4, $PM_{2.5}$ regulations led to lower levels of $PM_{2.5}$ over 2004–13 for people living in nonattainment counties relative to people in attainment counties in the same CBSA and the same levels of $PM_{2.5}$ over 2001–3. The EPA solely relied on 2001–3 to make its nonattainment designations. This is the essence of the quasi-experiment that we rely on to isolate conditionally exogenous variation in $\text{durPM}_{i,2013}$. More formally, we isolate this variation using a control function approach with a vector of instruments, Z_i. The five elements of Z_i include an indicator for residing in a nonattainment county in 2004 and interactions between this indicator and $f(\text{basePM}_i)$. This set of instruments is designed

24. We make an assumption in Section 5.2 regarding the independence of e_i and the vector of controls and instruments.

25. Because we allow prior exposure to be an element of the error term, rather than explicitly model its impact, we cannot answer questions directly related to lifetime exposure. In our model, α captures the causal effect of later-in-life decadal pollution on the probability of a new dementia diagnosis, holding all else constant, including earlier-in-life exposure.

26. The federal regulatory-grade monitors that we use for our analysis represent the best available information on ambient $PM_{2.5}$ in the U.S. Supplementary Material, Appendix B provides further information on EPA's approach to validating $PM_{2.5}$ measurements.

to capture the between- and the within-county variation in decadal $PM_{2.5}$ induced by the CAA, as discussed in Section 4. Our "first-stage equation" is given by

$$\text{durPM}_{i,2013} = \delta_Z Z_i + \delta_X X_i + \delta_H H_i + \delta_W W_i + \delta_C C_{i,2013} + f(\text{basePM}_i; \delta_{\text{basePM}}) + \varepsilon_i, \quad (4.2)$$

where the covariates other than Z_i are the same as in equation (4.1).

We assume that (e_i, ε_i) is distributed jointly normal with mean zero and var(e_i) normalised to one, and is independent of the instruments, Z_i, and controls, X_i, H_i, W_i, $C_{i,2013}$, and $f(\text{basePM}_i)$.[27] Under this assumption, the order condition is satisfied, as the controls are exogenous and can serve as instruments for themselves, while the scalar $\text{durPM}_{i,2013}$ is treated as endogenous and is instrumented with Z_i.

We denote the residuals from an estimation of equation (4.2) via ordinary least squares (OLS) as $\hat{\varepsilon}_i$. Following Rivers and Vuong (1988), these residuals are then added as an additional control to equation (4.1), which is then treated as a standard probit model and estimated using maximum likelihood.[28]

The equivalence of control function estimation in linear models and 2SLS is well established (*e.g.* Hausman, 1978). In non-linear models like ours, the estimators are not equivalent, but the intuition of 2SLS remains applicable. This gives rise to the term two-stage conditional maximum likelihood (2SCML) that Rivers and Vuong (1988) use to describe the approach that we rely on. Our 2SCML approach requires the standard conditions for consistency of the 2SLS estimator, *i.e.* that the controls are exogenous, that the instruments, Z_i, are partially correlated with $\text{durPM}_{i,2013}$, and that the instruments, Z_i, are exogenous.[29]

The mean-independence assumption that guarantees exogeneity of the controls, *i.e.* $E[e_i|X_i, H_i, W_i, C_{i,2013}, f(\text{basePM}_i)] = E[\varepsilon_i|X_i, H_i, W_i, C_{i,2013}, f(\text{basePM}_i)] = 0$, is equivalent to the assumption that the functional forms specified in the decomposition of η and in equation (4.2) are sufficiently flexible to capture the relationships between the controls and η_i and the controls and $\text{durPM}_{i,2013}$.[30] Three features of our research design support the credibility of this functional-form assumption. First, as discussed in Section 3, our controls are extensive. Second, our model is saturated within some control vectors (*e.g.* integer-age-by-gender dummies and the full-factorial of baseline health conditions) and flexible in other control vectors (*e.g.* fourth-order polynomial functions of medical spending and baseline pollution). Third, the estimated AMEs are relatively insensitive to adding additional interactions and additional flexibility in unsaturated control vectors.[31]

The first condition on the instruments, Z_i (relevance), can be directly validated with empirical testing, while the second condition (exogeneity) cannot be. A violation of the key identifying

27. While assuming joint normality is standard in this class of models, Rivers and Vuong (1988) note that it is actually stronger than the sufficient condition that e_i is normal and homoscedastic given ε_i, the instruments, Z_i, and controls, X_i, H_i, W_i, $C_{i,2013}$, basePM_i. We also assume that the technical assumptions of Rivers and Vuong hold, namely that the data are i.i.d. and the parameter vector lies in the interior of a compact, convex subset of Euclidean space.

28. The Rivers and Vuong (1988) approach estimates a scaled version of the parameters in equation (4.1) where the scaling factor depends on the variance of ε_i and the covariance between ε_i and e_i. While the unscaled coefficients can be recovered, this is not necessary. As discussed in Wooldridge (2015), the scaled coefficients are sufficient for estimating the average structural function (Blundell and Powell, 2013) and the AME of $\text{durPM}_{i,2013}$ on $\text{Prob}(\Delta y_i = 1)$.

29. In a linear model, consistency requires that the controls and instruments are uncorrelated with the error. We are estimating a Probit model which requires the stronger assumptions of independence and normality.

30. A necessary condition for Z_i to be a valid instrument for $\text{durPM}_{i,2013}$ is conditional independence, *i.e.* that Z_i is independent of η_i conditional on the controls. Combining this conditional independence assumption with the additional assumption that (e_i, ε_i) is mean independent of the controls is then sufficient for (e_i, ε_i) to be mean independent of both Z_i and the controls.

31. See, for example, the discussions in Sections 5.3, 5.4, 7.2, and Supplementary Material, Appendix J.

assumption of exogeneity would mean that some unobserved factor remaining in e_i causes individuals of the same age, race, sex, and baseline health who experienced the same residential $PM_{2.5}$ concentrations across 2001–3 and lived in neighbourhoods with the same socioeconomic conditions, nevertheless sorted into attainment versus nonattainment counties within the same CBSA on the basis of factors associated with different probabilities of receiving a new dementia diagnosis from 2005 to 2013 and yet did not have dementia prior to 2005. We follow prior studies and assume that nonattainment status is independent of measurement error in $PM_{2.5}$ exposure in counties that contain air pollution monitors (Chay and Greenstone, 2005; Isen *et al.*, 2017).

We consider the earlier-in-life exposure that, as previously discussed, is an element of e_i. The EPA nonattainment designations relied only on 2001–3 concentrations and we include a flexible (fourth-order polynomial) function of $basePM_i$ (created using data from 2001 to 2003) in our empirical models. Thus, earlier-in-life exposure would bias our estimate of α only in the unlikely event that earlier-in-life exposure is not independent of nonattainment status conditional on baseline pollution and other controls. We provide support for the exclusion restriction assumption in Section 7 by estimating a model that includes a measure of earlier-in-life exposure. While the coefficient on earlier-in-life exposure itself is uninformative for evaluating the 2SCML assumptions, the fact that the estimates of the AMEs are invariant to its inclusion suggests that the omission of earlier exposure is not biasing our estimated effect of interest.

To conclude, like 2SLS estimators, our key identifying assumption is that the error in equation (4.1) is independent of our instrument, Z_i. This is likely to hold given our extensive set of controls and the sharply defined timeframe used by the EPA to make regulatory designations. We provide support for this assumption in Section 7.

Addressing selection on mortality. Prior work has found that $PM_{2.5}$ causes mortality among seniors in the U.S. (Di *et al.*, 2017; Deryugina *et al.*, 2019). For example, Deryugina *et al.* use an instrumental variables estimator to conclude that a one-day $1\,\mu g/m^3$ increase in $PM_{2.5}$ causes a 0.18% increase in mortality over three days. When we estimate the specification shown in equations (4.1) and (4.2) but with decadal mortality as the dependent variable, we find that a $1\,\mu g/m^3$ increase in average $PM_{2.5}$ from 2004 through 2013 increases mortality by 2.47 pp, equivalent to 6% of the decadal mortality rate.[32]

These results, combined with the concern that unobserved aspects of health that determine survival may be correlated with unobserved aspects of health that determine dementia, suggest that sample selection may bias the estimates of equations (4.1) and (4.2) when not accounting for selection on mortality. For example, suppose that unobserved aspects of health that determine survival are negatively correlated with unobserved aspects of health that determine dementia, *i.e.* sicker people who are more likely to die are also more likely to be diagnosed with dementia if they live. In this case, selection would bias downward the estimate of $PM_{2.5}$'s direct effect on dementia in the selected sample.[33] This would mean that our estimate of α when ignoring selection would capture both the causal effect of $PM_{2.5}$ on dementia (our object of interest) plus a compositional effect based on the set of survivors at the end of the decade.[34]

To address this selection issue, we obtain a selection-corrected estimate using a control function approach (Heckman, 1979; Heckman and Robb, 1986). To implement this approach, we

32. Supplementary Material, Appendix Table I1 provides the estimated effects of decadal $PM_{2.5}$ on mortality, *i.e.* an estimation of equations (4.1) and (4.2) with mortality as the outcome in equation (4.1).

33. A less intuitive, but nonetheless possible, concern would be that the unobserved health determining survival was positively correlated with the unobserved health determining dementia, causing an upward bias in our estimate.

34. Lee (2009) discusses this concept in detail in the context of a randomly assigned job-training program that affects whether individuals work and the level of their subsequent wages.

require an additional set of instruments.[35] In particular, the relevance and validity conditions require that the additional instruments are correlated with decadal survival but are independent of the unobserved determinants of dementia. The medical literature provides such a set of diagnoses that affect survival but do not affect dementia: prior diagnoses of a subset of non-smoking-related cancers, which are found to be unrelated to dementia outcomes (Driver *et al.*, 2012; Ganguli, 2015). To form the selection-correcting control function, we begin by estimating via maximum likelihood a probit model of decadal survival, S_i, with the same covariates as equation (4.2) plus the vector of additional instruments, M_i. We do this by specifying a latent survival propensity

$$S_i^* = \gamma_Z Z_i + \gamma_X X_i + \gamma_H H_i + \gamma_W W_i + \gamma_C C_{i,2013} + f(\text{basePM}_i; \gamma_{\text{basePM}}) + \gamma_M M_i + u_i, \quad (4.3)$$

such that $S_i = 1[S_i^* > 0]$.

In addition to the functional-form assumptions in equation (4.3), we now assume that $(e_i, \varepsilon_i, u_i)$ is distributed jointly normal and is independent of the instruments, Z_i, the instruments, M_i, and controls, X_i, H_i, W_i, $C_{i,2013}$, and basePM_i. We define M_i to include indicators for baseline diagnoses of non-smoking-related cancers (leukaemia, lymphoma, and cancers of the breast, prostrate, colon, rectum, and endometrium) from the CMS's CCW file. These seven cancers, which affect decadal survival, are assumed to be independent of latent features of health that affect the probability of a dementia diagnosis.[36] We then use the generalised residuals of equation (4.3), denoted $\hat{\upsilon}_i$, to define an additional control that we include in equations (4.1) and (4.2).[37]

To summarise, our estimation proceeds in three steps. The first step is to estimate equation (4.3) via maximum likelihood and create the generalised residuals, $\hat{\upsilon}_i$. The second step is to include $\hat{\upsilon}_i$ as an additional control in equation (4.2), estimate equation (4.2) via OLS, and recover the residuals, $\hat{\varepsilon}_i$. The final step is to include functions of $\hat{\varepsilon}_i$ and $\hat{\upsilon}_i$ as additional controls in equation (4.1). We show this version of equation (4.1) that includes the additional controls in equation (4.4), which we estimate via maximum likelihood:

$$\begin{aligned}\Delta y_i^* = {} & h(\text{durPM}_{i,2013}; \alpha_i) + \beta_X X_i + \beta_H H_i + \beta_W W_i + \beta_C C_{i,2013} \\ & + f(\text{basePM}_i; \beta_{\text{basePM}}) + \beta_{\text{CF}} \text{CF}_i + \tilde{e}_i, \end{aligned} \quad (4.4)$$

where $\tilde{e}_i = e_i - \beta_{\text{CF}} \text{CF}_i$. CF_i denotes the control function vector created with the generalised residuals from the estimation of equation (4.3) and the residuals from the estimation of equation

35. In Supplementary Material, Appendix H, we show a Lee (2009) bounds approach that does not require these additional instruments, M_i, but does employ the CAA ones, Z_i, as described above.

36. A potential concern is that non-smoking-related cancers, while not causing dementia, could be correlated with dementia through other omitted factors. For example, a competing-risks framework could lead to a negative correlation between non-smoking-related cancers and latent health affecting dementia and lead to an upward-biased estimate of α in our selection-correction model. Such a framework would likewise suggest that estimating equation (4.1) adding only the CAA-based control function would provide a downward-biased estimate. On this basis, one could interpret non-smoking-related cancers as "imperfect instruments," as defined by Nevo and Rosen (2012), and use them to partially identify α. The estimated identification region would then simply be the interval between the two estimates.

37. Generalized residuals are defined as $\hat{\upsilon}_i = S_i \lambda(\widehat{S^*}) - (1 - S_i)\lambda(-\widehat{S^*})$, where $\lambda(\cdot) = \phi(\cdot)/\Phi(\cdot)$, ϕ and Φ are the standard normal density and cumulative density function (CDF), respectively, and $\widehat{S^*} = \hat{\gamma}_Z Z_i + \hat{\gamma}_X X_i + \hat{\gamma}_H H_i + \hat{\gamma}_W W_i + \hat{\gamma}_C C_{i,2013} + f(\text{basePM}_i; \hat{\gamma}_{\text{basePM}}) + \hat{\gamma}_M M_i$. By construction, $S_i = 1$ for all observations used in the estimation of equations (4.1) and (4.2), therefore, for these observations, $\hat{\upsilon}_i = \lambda(\widehat{S^*})$ simplifying to the familiar inverse Mills ratio used in Heckman (1979).

(4.2). We set $\text{CF}_i = [\hat{\varepsilon}_i\ \hat{\varepsilon}_i^2\ \hat{v}_i\ \hat{v}_i^2]$.[38] Because we estimate $\hat{\varepsilon}_i$ and $\hat{v}_i$ in prior stages, we bootstrap standard errors over all three regressions, clustering at the Census block group level to allow for spatial correlation in diagnoses.[39]

5.3. *Allowing for heterogeneity in covariates*

In the simplest specification of the decadal model, we specify $h(\text{durPM}_{i,2013};\alpha_i) = \alpha\ \text{durPM}_{i,2013}$. However, we also estimate specifications that allow $\text{durPM}_{i,2013}$ to enter flexibly as a fourth-order polynomial and that allow for interactions between $\text{durPM}_{i,2013}$ and the vectors X_i, H_i, W_i, and CF_i by specifying:[40]

$$\begin{aligned} h(\text{durPM}_{i,2013};\alpha_i) = {} & \alpha_1\ \text{durPM}_{i,2013} + \alpha_2 \text{durPM}_{i,2013}^2 + \alpha_3\ \text{durPM}_{i,2013}^3 \\ & + \alpha_4\ \text{durPM}_{i,2013}^4 + \alpha_X\ X_i\ \text{durPM}_{i,2013} + \alpha_H\ H_i\ \text{durPM}_{i,2013} \\ & + \alpha_W\ W_i\ \text{durPM}_{i,2013} + \alpha_{CF}\ CF_i\ \text{durPM}_{i,2013}. \end{aligned} \tag{4.5}$$

In this approach, the effect of $\text{durPM}_{i,2013}$ on the latent propensity to be newly diagnosed with dementia is allowed to vary flexibly with both the level of $\text{durPM}_{i,2013}$ and the levels of individual characteristics, neighbourhood characteristics, and control function variables.[41]

5.4. *Allowing $PM_{2.5}$'s effect to vary with exposure duration*

The contemporaneous model described in Sections 5.1 and 5.2 is both parsimonious and comparable to the existing literature on the impacts of pollution exposure on health outcomes. However, as the treatment is measured as the average $\text{PM}_{2.5}$ exposure from 2004 to 2013, and a dementia diagnosis can happen at any point between 2005 and 2013, there could be an aggregation bias if the data were systematically misaligned; for example, if the AME were driven by spatial correlation between dementia diagnoses in 2010 and pollution levels in 2013. The potential for misalignment due to temporal aggregation is a universal feature of research on pollution and health due to the inability to measure pollution and health instantaneously.

In this section, we extend the analysis in three ways. First, we define the outcome measure to be a new dementia diagnosis during a single year $t = [2005, 2013]$, thus avoiding the aggregation bias that could be introduced by misalignment of the data at the decadal level. Second, we only condition on surviving until the end of year t, thus incorporating the effects of $\text{PM}_{2.5}$ exposure on

38. In alternative specifications, *e.g.* Columns (3) and (2) of Table I, we consider a less flexible control function that only includes $\hat{\varepsilon}_i$ and $\hat{v}_i$, without their squares, as well as a version with only $\hat{\varepsilon}_i$, which controls for the type of endogeneity described in Section 5.2.1, but not selection on mortality.

39. Our instruments vary within Census blocks across ZIP + 4 codes. We alternatively cluster at the courser county level and find almost no impact on our results.

40. See Blundell and Powell (2003, 2004) for a discussion of estimating non-parametric, binary-response models with endogenous regressors.

41. The fact that the effect of $\text{durPM}_{i,2013}$ on new dementia diagnosis is allowed to vary with $\text{CF}_i = [\hat{\varepsilon}_i\ \hat{\varepsilon}_i^2\ \hat{v}_i\ \hat{v}_i^2]$ means that this approach nests the correlated random-coefficients model of Garen (1984) with additional assumptions. Specifically, if there exist random coefficients that satisfy the linear conditional expectation assumption of Garen (1984), they will be accounted for in our analysis. Under these assumptions, we do not find evidence of bias coming from correlated random coefficients in one's sensitivity to pollution exposure. Following Rivers and Vuong (1988) and Wooldridge (2015), once the control function, CF_i, is included in equation (4.4), $\text{durPM}_{i,2013}$ is independent of $\tilde{e}_i$ and, therefore, the non-linear functions of $\text{durPM}_{i,2013}$ in equation (4.5) are also independent of $\tilde{e}_i$. And, as we had assumed that the controls are independent of $\tilde{e}_i$, the interaction terms in (4.5) are also independent of $\tilde{e}_i$. Adding the 115 additional functions of the single endogenous economic variable, $\text{durPM}_{i,2013}$, has little impact on the results as shown in Columns (4) and (5) of Table I.

dementia for individuals who die prior to 2013. Finally, by estimating the model separately for each year, we allow for year-specific variation in all model coefficients. This additional flexibility allows the effect of nonattainment status on $PM_{2.5}$ exposure to evolve during the years after nonattainment designations were made. Moreover, it allows the effect of $PM_{2.5}$ exposure on the probability of a new dementia diagnosis to evolve with the duration of exposure. In principle, such differences could arise from biological mechanisms linking $PM_{2.5}$ to dementia, or from changes in the composition of people surviving from one year to the next.

Equations (4.6) and (4.7) describe the analogues to equations (4.4) and (4.5), respectively, where $\Delta y^*_{i,t}$ now denotes the latent propensity to become newly diagnosed with dementia during year t. We estimate equation (4.6) separately for each year 2005–13 via maximum likelihood.

$$\begin{aligned}\Delta y^*_{i,t} = {} & h(\text{durPM}_{i,t};\ \alpha_{i,t}) + \beta_{X,t} X_i + \beta_{H,t} H_i + \beta_{W,t} W_i + \beta_{C,t} C_{i,t} \\ & + f(\text{basePM}_i;\ \beta_{\text{basePM},t}) + \beta_{\text{CF},t} CF_{i,t} + \tilde{e}_{i,t},\end{aligned} \tag{4.6}$$

where $\tilde{e}_{i,t} = e_{i,t} - \beta_{\text{CF},t}\text{CF}_{i,t}$,

$$\begin{aligned}h(\text{durPM}_{i,t};\alpha_{i,t}) = {} & \alpha_{1,\text{t}}\text{durPM}_{i,t} + \alpha_{2,t}\text{durPM}^2_{i,t} + \alpha_{3,t}\,\text{durPM}^3_{i,t} + \alpha_{4,t}\,\text{durPM}^4_{i,t} \\ & + \alpha_{X,t}\,X_i\,\text{durPM}_{i,t} + \alpha_{H,t} H_i\,\text{durPM}_{i,t} + \alpha_{W,t} W_i\,\text{durPM}_{i,t} \\ & + \alpha_{CF,t}\,\text{CF}_{i,t}\,\text{durPM}_{i,t},\end{aligned} \tag{4.7}$$

and $\text{CF}_{i,t}$ denotes a control function vector, $[\hat{\varepsilon}_{i,t}\ \hat{\varepsilon}^2_{i,t}\ \hat{v}_{i,t}\ \hat{v}^2_{i,t}]$, created with the generalised residuals from the estimation of equation (4.8) (the analogue to equation (4.3)) and the residuals from the estimation of equation (4.9) (the analogue to equation (4.2)):

$$\begin{aligned}S_{i,t} = {} & 1(\gamma_{Z,t} Z_i + \gamma_{X,t} X_i + \gamma_{H,t} H_i + \gamma_{W,t} W_i + \gamma_{C,t} C_{i,t} \\ & + f(\text{basePM}_i;\ \gamma_{\text{basePM},t}) + \gamma_{M,t} M_i + u_{i,t} > 0),\end{aligned} \tag{4.8}$$

$$\begin{aligned}\text{durPM}_{i,t} = {} & \delta_{Z,t} Z_i + \delta_{X,t} X_i + \delta_{H,t} H_i + \delta_{W,t} W_i + \delta_{C,t} C_{i,t} \\ & + f(\text{basePM}_i;\ \delta_{\text{basePM},t}) + \hat{v}_{i,t} + \varepsilon_{i,t}.\end{aligned} \tag{4.9}$$

We begin by estimating equation (4.8) via maximum likelihood on the full sample of individuals. The survival outcome, $S_{i,t}$, now indicates whether individual i is still alive through the end of year t and has not previously received a dementia diagnosis. We then estimate the year-t-specific pollution equation (4.9) via OLS. This equation includes the generalised residuals from the survival function, $\hat{v}_{i,t}$. Equations (4.9) and (4.6) are estimated using the subset of people who are still alive through the end of year t and had not been diagnosed with dementia prior to year t.

We then use the year-t-specific parameter vector, $\hat{\alpha}_t$, to calculate AME_t, the average effect of a marginal increase in $PM_{2.5}$ exposure from 2004 through year t on the probability of receiving a new dementia diagnosis during year t. We additionally calculate the cumulative effect of $PM_{2.5}$ exposure from 2004 through year t on new dementia diagnoses during that period according to

$$\text{cumulative AME}_t = \sum_{s=2005}^{t} \left(\frac{\text{pop}_s}{\text{pop}_{2005}}\right) \text{AME}_s \tag{4.10}$$

by summing the year-specific AMEs, after weighting them by their corresponding shares of the original population to account for attrition due to dementia and death.[42] Finally, we bootstrap standard errors on cumulative AME_t by repeating estimation of equations (4.6)–(4.10) after

42. For example, we multiply the estimated AME in 2009 by 0.65 because 65% of the original year-2005 sample survives to the end of 2009. This adjusts for the progressive decline in sample size due to dementia and mortality.

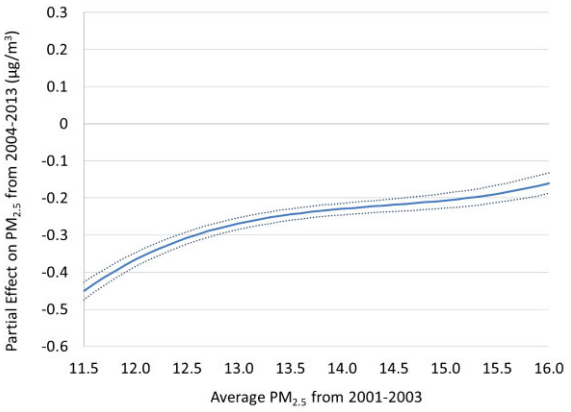

FIGURE 4

Estimated partial effect of nonattainment on $PM_{2.5}$ exposure 2004–13, by baseline concentrations 2001–3

Notes: The figure shows the average effect of the county-level nonattainment designation on the average individual-level conditional change in $PM_{2.5}$ concentrations over the period 2004–13. The zero line represents individuals living in attainment counties at the same baseline $PM_{2.5}$ concentration and holding all else in equation (4.9) constant. The dotted lines denote 95% confidence bands constructed from 1000 bootstrap replications, with clustering at the Census block group.

resampling from the original population one thousand times with replacement and clustering at the Census block group level.

6. RESULTS

6.1. *$PM_{2.5}$ regulations created conditional differences in subsequent $PM_{2.5}$*

The identifying variation for our estimator comes from the fact that the EPA's nonattainment designations created quasi-random differences in $\text{durPM}_{i,t}$ for $t = [2005, 2013]$, conditional on basePM_i and the additional controls in equations (4.2) and (4.9). Figure 4 shows this identifying variation for the year 2013. Specifically, it uses the coefficients on the instruments from the year-2013 version of equation (4.9) to plot the estimated partial effect of nonattainment on $\text{durPM}_{i,2013}$ across levels of basePM_i. Similar figures plotting the estimated partial effect for $t = [2005, 2012]$ versions of equation (4.9), as well as the decadal version in equation (4.2), are shown in Supplementary Material, Appendix Figure I1. Intuitively, the partial effects are negative, showing that nonattainment status reduced pollution. In addition, as permitted (but not determined) by our construction of Z_i, the partial effects vary with baseline $PM_{2.5}$. This yields within-county identifying variation in $\text{durPM}_{i,t}$ in all years.[43] The first-stage partial R^2 of the identifying instruments is 0.047 and the F-statistic is 489 for the regression underlying Figure 4, suggesting that any finite sample bias is negligible. The size of the F-statistic reflects the number of observations (approximately 1 million) and number of Census block group clusters (approximately 140 thousand).

43. While nonattainment status caused reductions in $\text{durPM}_{i,t}$ at all levels of basePM_i, these reductions are larger at lower baseline levels.

TABLE 1
AME of cumulative $PM_{2.5}$ on the probability of a new dementia diagnosis

	(1)	(2)	(3)	(4)	(5)	(6)
(1 μg/m^3 increase in decadal $PM_{2.5}$)	0.629*** (0.058)	0.124 (0.105)	1.545*** (0.536)	2.283*** (0.565)	2.384*** (0.568)	2.151*** (0.846)
Individual and neighbourhood covariates		x	x	x	x	x
$PM_{2.5}$ control function			x	x	x	x
Survival control function				x	x	x
Polynomial functions and interactions					x	x
Heterogeneity by exposure duration						x
F-statistic on $PM_{2.5}$ instruments			496	498	498	165–489
Number of individuals: dementia function	1,179,094	1,179,094	1,179,094	1,179,094	1,179,094	989,751–2,293,270
Chi-square statistic on survival instruments				3,813	3,813	1,166–2,274
Number of individuals: survival function				2,439,904	2,439,904	2,439,904

Notes: The outcome is scaled equal to 100 if an individual was diagnosed with dementia and 0 otherwise. By 2013, 20% of the individuals in our sample who were alive in that year had been diagnosed with dementia. In Column (1), the covariates are $PM_{2.5}$ and CBSA dummies. Column (2) adds covariates for baseline health in 2004, individual demographics, demographics for the individual's Census block group, and pre-regulatory (2001–3) $PM_{2.5}$ levels at their residence. Column (3) adds a control function for $PM_{2.5}$. Column (4) adds a control function for survival. Column (5) adds additional polynomial functions of covariates. Column (6) reports a cumulative decadal AME that aggregates year-specific AMEs, along with ranges for the year-specific *F*-statistics, Chi-square statistics, and sample sizes. Year-specific estimates are reported in Table I5. Asterisks indicate statistical significance at the 10% (*), 5% (**), and 1% (***) levels using robust standard errors clustered at the block group. Standard errors in Columns (3) through (6) are bootstrapped using 1000 repetitions.

6.2. *The effect of $PM_{2.5}$ on dementia*

We find that a 1 μg/m^3 increase in average $PM_{2.5}$ concentrations starting in 2004 increases the probability of receiving a new dementia diagnosis before the end of 2013 by an average of 2.15pp. To illustrate the importance of various aspects of our identification strategy, we present the AME of cumulative $PM_{2.5}$ exposure over the decade on new dementia diagnoses from six specifications described in Section 5.

The first column of Table 1 begins with a simple, associative model of decadal $PM_{2.5}$ and dementia diagnosis over the decade. The next four columns retain the contemporaneous, decadal specification and incrementally address potential confounders that may underlie this association, as previously discussed. The final column presents our preferred specification that aggregates year-by-year marginal effects over the decade while addressing all of the potential confounders described in Section 5. In all cases, the AMEs are scaled to represent pp changes in the probability of receiving a new dementia diagnosis by the end of 2013.

Column (1) in Table 1 shows the result from a simple associative model of decadal $PM_{2.5}$ and dementia diagnosis over the decade. The only covariates are CBSA dummies. The result indicates that a 1 μg/m^3 increase in average decadal $PM_{2.5}$ is associated with a 0.63 pp higher probability of receiving a dementia diagnosis between 2005 and 2013.

Column (2) then additionally includes the observed characteristics represented by X_i, H_i, W_i, and $f(\text{base}PM_i)$ in equation (4.1). Adding these covariates reduces the conditional association

between measured decadal $PM_{2.5}$ and dementia over the decade to 0.12 pp. Thus, most of the within-CBSA association between measured $PM_{2.5}$ and new dementia diagnoses can be explained by people with observably higher baseline risks of dementia living in more polluted neighbourhoods. Notably, 99% of the decline that occurs as we move from Column (1) to Column (2) can be explained by the inclusion of X_i, H_i, and W_i. When all of these covariates are included, adding $f(\text{basePM}_i)$ only reduces the AME for $PM_{2.5}$ exposure from 2004 to 2013 by 1%. This shows that our extensive measures of individual demographics, baseline health, and neighbourhood characteristics explain almost all of the heterogeneity that contributes to any association between neighbourhood $PM_{2.5}$ in 2001–3 and new dementia diagnoses.

Column (3) adds the $PM_{2.5}$ control function to address measurement error in pollution exposure, or any residual differences driven by sorting. The resulting order-of-magnitude increase in the AME relative to Column (2) is unsurprising. First, our extensive set of geographic controls could potentially exacerbate the effect of any measurement error in pollution. Second, while the bias introduced by measurement error is ambiguous in general, prior studies have consistently found that instrumenting for (shorter-term) measures of air pollution exposure results in order-of-magnitude increases in estimates for its effects on other morbidities and mortality among older adults (see, *e.g.* Schlenker and Walker, 2016; Deschênes *et al.*, 2017; Deryugina *et al.*, 2019).[44]

Column (4) adds the survival control function to address selection on mortality.[45] Controlling for selection on survival increases the AME to 2.28 pp, a 48% increase relative to Column (3). This increase is consistent with classic selection bias caused by positively correlated latent health: individuals who were more likely to die were also more likely to develop dementia.[46]

Column (5) shows the AME from our specification shown in equations (4.4) and (4.5) that allows for additional parametric flexibility in the covariates.[47] This only increases the AME to 2.38 pp, which is about a 4% increase relative to Column (4).[48]

The final AME shown in Column (6) shows the cumulative AME at the end of the decade as shown in equations (4.6) through (4.10). This model differs from the model underlying the AME in Column (5) in three potentially important ways. First, it limits aggregation bias that could be introduced by the misalignment of the data at the decadal level. Second, it incorporates the effects of $PM_{2.5}$ exposure on dementia for people who die during the decade, almost doubling the number of observations used in estimation. Finally, it allows the effect of $PM_{2.5}$ exposure on the probability of a new dementia diagnosis to evolve with the duration of exposure, as shown in equation (4.6).

This cumulative AME indicates that a 1 $\mu g/m^3$ increase in average $PM_{2.5}$ increases the cumulative probability of a new dementia diagnosis by the end of 2013 by 2.15 pp. Comparing this

44. These studies find that instrumenting for air pollution increases their estimates for its effects on morbidity and mortality by factors ranging from 6 to 20. The twelve-fold increase in our Table I estimates sits near the middle of this range.

45. The AMEs of the survival instruments are reported in Supplementary Material, Appendix Table I2.

46. We build on this result and develop a partial-identification approach to exploring the role of selection on survival in Supplementary Material, Appendix H.

47. Supplementary Material, Appendix Table I3 reports the full results from this specification. Supplementary Material, Appendix Table I4 compares the AME for $PM_{2.5}$ from this specification to the AMEs that we estimate for other dementia risk factors that were included as covariates in the model. Note that we do not consider the coefficients on risk factors other than decadal $PM_{2.5}$ to reflect a causal relationship.

48. When we run this specification using a linear-probability model, we find an AME of 2.16 pp that is statistically significant at the 1% level.

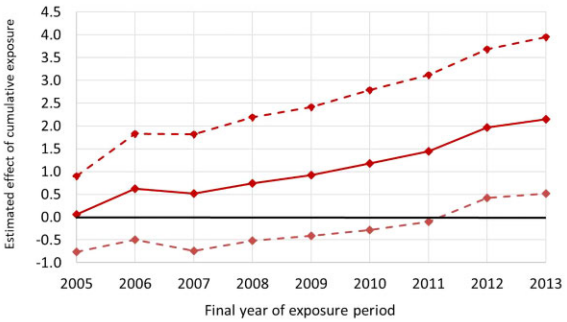

FIGURE 5
Estimated effects of $PM_{2.5}$ on dementia by exposure duration.

cumulative AME against the results from the more parsimonious model in Column (5) indicates that the three notable differences between the two approaches yields only a small difference in the economic magnitude of their estimated effects (0.23 pp).

To provide context for the AME of 2.15 pp, a 1 $\mu g/m^3$ change is equivalent to 9.1% of the average person's exposure between 2004 and 2013 and 59% of a standard deviation. A 2.15 pp change in the dementia diagnosis rate is a 11% increase relative to the diagnosis rate among people in our sample who survive to the end of 2013. To provide an age-based comparison to this statistic, the dementia diagnosis rate in 2013 was 2.2 pp higher among 80-year-old women compared with 79-year-old women (Figure 1).[49]

Figure 5 shows how our estimates of the cumulative AME evolve over time, along with 95% bootstrapped confidence intervals. The underlying year-specific AMEs are presented in Supplementary Material, Appendix Table I5. While the year-specific AMEs are imprecisely estimated, the AME for 2005 is close to zero and, starting in 2008, the year-specific AMEs are positive in each year, which is reflected in the increasing cumulative AME shown in Figure 5. In addition, the year-specific AMEs are generally increasing in the duration of exposure. When we weight the year-specific AMEs by the surviving share of the baseline population to account for attrition, as shown in equation (4.10), the resulting weighted year-specific AMEs become similar in magnitude. This similarity is reflected in the approximately linear trend in cumulative AME point estimates shown in Figure 5, although visual inspection of the confidence intervals suggests that we lack the statistical power to rule out a non-linear function.

6.3. *Heterogeneity in effects*

The results shown in Column (6) of Table 1 average over considerable heterogeneity in the marginal effects of $PM_{2.5}$ exposure. Interestingly, the cumulative AMEs tend to be larger among individuals who experienced lower levels of $PM_{2.5}$. To illustrate this, we divide individuals into terciles by their baseline residential exposures during 2001–3. Individuals in the top tercile of

49. To compare these results to earlier medical literature, Gatz *et al*. (1997) find that approximately 74% of Alzheimer's disease cases are heritable using twin pairs. We impose the additive separability assumption underlying that statistic and perform a back-of-the-envelope calculation to see how much variation in new dementia diagnoses could be explained by decadal $PM_{2.5}$ exposures after age sixty-five in our sample. Specifically, we use a linearized and additively separable version of our decadal model to calculate $(AME^2\ Var(durPM))/(Var(\Delta y)) \approx 1\%$, where $AME = 0.0238$ (this number is multiplied by 100 when discussed in the text), $Var(durPM) = 2.8812$, and $Var(\Delta y) = 0.1572$. We thank an anonymous referee for this suggestion.

baseline exposure (above 14.2 $\mu g/m^3$) experienced a cumulative AME of 1.91 pp. In comparison, individuals in the middle tercile (whose baseline exposures were between 12.4 and 14.2 $\mu g/m^3$) experienced an AME of 2.10 pp. Individuals with baseline exposures below 12.4 $\mu g/m^3$ experienced an AME of 2.45 pp. For those in the top, middle, and bottom terciles who survived through 2013, average exposures from 2004 to 2013 were 12.46, 11.14, and 9.24 $\mu g/m^3$, respectively. These results highlight that the effects of $PM_{2.5}$ on dementia persist well below the current U.S. regulatory threshold of 12 $\mu g/m^3$ of annual average concentrations.

The estimates also show heterogeneity across individual characteristics. For example, the cumulative AME is larger for individuals whose exposures we observe at older ages (*e.g.* 1.13 pp for people born in 1938 for whom we observe quasi-random variation in exposure from age sixty-six up to age seventy-five, compared with 2.34 pp for people born in 1928 for whom we observe variation in exposure from age seventy-six up to age eighty-five). Conditional on age at exposure, the AME is higher for women compared with men (*e.g.* 2.57 pp for women born in 1928 compared with 2.03 pp for men born in 1928). Conditional on age and sex, the AME is higher for individuals with more clinical risk factors for dementia at the start of the decade (*e.g.* 2.24 pp for women born in 1928 with no baseline clinical risk factors compared with 2.61 pp for women born in 1928 who had been diagnosed with ischaemic heart disease and hypertension at baseline). Finally, when we condition on $PM_{2.5}$ exposure, the AME is higher among individuals denoted by CMS as "Black or African-American" compared with "non-Hispanic White" (*e.g.* 0.21 pp higher among women born in 1928 whose baseline exposure to $PM_{2.5}$ was within a one-unit window of the sample median of 13.4 $\mu g/m^3$).[50]

7. MAIN VALIDATION TESTS AND ADDITIONAL SENSITIVITY ANALYSIS

7.1. *Main validation tests*

Table 2 presents three validation tests of our estimator. First, we assess the assumption that our nonattainment instrument is independent of earlier-in-life measures of $PM_{2.5}$, conditional on baseline $PM_{2.5}$ exposure and the other covariates. Specifically, we examine whether the AME shown in Table 1, Column (6), changes when we add measures of earlier-in-life exposures, specifically average annual $PM_{2.5}$ in 1999 and 2000.[51] These are the first two years that the U.S. EPA had a national network of $PM_{2.5}$ monitors and the first two years that researchers can obtain administrative data describing the Medicare population. Thus, this validation test exhausts the available data. For the 23% of our sample that were under age sixty-five in those years and not yet enrolled in Medicare, we assign 1999 and 2000 $PM_{2.5}$ exposures based on the location where we first observe these individuals living upon enrolling in Medicare. While this assignment is imperfect, low short-term migration rates among this age group limit the scope for error. For example, the year 2000 Census of Population reports that 77% of people aged 65–69 lived in the same residence as they did five years ago.

If the exclusion restrictions on Z_i are valid, then adding controls for earlier-in-life $PM_{2.5}$ should not change the estimated AME of cumulative exposure over the decade. Column (2) shows that this augmented specification yields an AME of 2.25 pp. This is similar to the AME of 2.15 pp from our main specification (repeated in Column (1) for convenience). This similarity reinforces the validity of the instrument and is consistent with

50. Average decadal $PM_{2.5}$ exposures in our estimation sample were 6% higher for Black or African-American individuals compared with non-Hispanic White individuals who survived through 2013.

51. In the years of 1999 and 2000, 86% of our balanced panel of monitors were in operation.

TABLE 2
Validation tests

	(1)	(2)	(3)	(4)
Probit model AME (1 μg/m^3 increase in decadal $PM_{2.5}$)	2.151***	2.246***	1.754**	−0.167
	(0.846)	(0.929)	(0.704)	(0.283)
Modification to main specification				
Control for $PM_{2.5}$ in 1999 and 2000		x		
Control for other regulated air pollutants			x	
Placebo outcome = dementia in 2004				x
F-statistic on $PM_{2.5}$ instruments	165–489	147–492	146–350	620
Number of individuals: dementia function	989,751–2,293,270	989,751–2,293,270	989,751–2,293,270	2,734,032
Chi-square statistic on survival instruments	1,166–2,274	1,166–2,274	1,168–2,277	
Number of individuals: survival function	2,439,904	2,439,904	2,439,904	

Notes: The first column repeats our main result from Table 1, Column (6) for comparison. The next three columns report results from alternative specifications that are designed to test the identifying assumptions that underlie our main specification. Asterisks indicate statistical significance at the 10% (*), 5% (**), and 1% (***) levels based on robust standard errors clustered at the Census block group. See the note to Table 1 and main text for further details.

the EPA's nonattainment designation criteria, which relied solely on $PM_{2.5}$ concentrations in 2001–3.

The specification in Column (3) tests whether our results are confounded by the model's omission of air pollutants that may be co-generated with $PM_{2.5}$. Specifically, we add measures of exposure to PM_{10}, ozone, nitrogen dioxide, sulphur dioxide, and carbon monoxide. Each measure is constructed following the same procedures that we used to construct measures of cumulative $PM_{2.5}$. When we control for these ancillary pollutants, the cumulative AME for $PM_{2.5}$ remains large and precisely estimated.

Additionally, we test for sorting based on unobserved risk factors, such as genetics, that may contribute to dementia and be correlated with $PM_{2.5}$. In principle, sorting on unobserved risk factors could bias the estimator if, prior to our study period, people at a lower unobserved risk for dementia sorted themselves into neighbourhoods that were more or less likely to be designated as nonattainment in the future, even conditional on baseline neighbourhood $PM_{2.5}$ and the other controls. While we cannot directly test this sorting hypothesis in our main estimation sample, we can test it indirectly by extending the sample to include the people who were excluded because they were diagnosed with dementia prior to 2005. In other words, if individuals sorted themselves into future nonattainment areas based on unobserved dementia risk, then we would expect to see a conditional relationship between dementia rates in 2004 and $PM_{2.5}$ exposure over the subsequent decade.[52] We test this hypothesis using a placebo specification that replaces the outcome in equation (4.4) with an indicator for a dementia diagnosis in 2004. Including everyone alive in 2004, with or without dementia, increases our sample size to 2.7 million. Column (4) shows that the estimated AME is negative, close to zero, and estimated relatively precisely.

52. Intuitively, under the hypothesis that people sorted into future nonattainment areas based on unobserved dementia risk, some people would have been diagnosed with dementia prior to 2005 and been dropped from our estimation sample, while others would have been diagnosed after 2005 and been included in our estimation sample.

This provides supporting evidence that the exclusion restriction is unlikely to be violated by initial differences in unobserved dementia risk, including unobserved genetic factors.

7.2. *Additional sensitivity analysis*

The effect of $PM_{2.5}$ on dementia persists when we use (1) different measures of dementia such as the use of prescription drugs for the symptoms of Alzheimer's disease rather than claims-based diagnosis codes; (2) different samples that include people who select into managed care plans known as Medicare Advantage (MA); (3) monitor-level attainment indicators rather than county-level indicators; (4) different approaches to measuring $PM_{2.5}$ exposure including expanding the set of monitors to include those not present for the entire study period; (5) a limited sample of individuals who live close to a monitor; and (6) controls for baseline pollution exposure that are even more flexible than the fourth-order polynomial function described above. We present and discuss these results in Supplementary Material, Appendix J.

Finally, we estimate models for placebo health outcomes. We examine five chronic conditions that are not known or suspected to be caused by air pollution but share similarities with dementia in terms of how they affect the body, how they are diagnosed, and how diagnosis rates are correlated with age, race, and gender. These are glaucoma, fibromyalgia, breast cancer, prostate cancer, and peripheral vascular disease.[53] Supplementary Material, Appendix Table J5 shows that we fail to reject the null hypothesis of zero effect at the 10% significance level for each of these placebos. We elaborate on these models and results in the Supplementary Material, Appendix.

Our criteria for selecting placebos excluded illnesses that have previously been linked to air pollution. When we instead ignore these criteria and repeat estimation for each of the 15 most common chronic conditions among the Medicare population including those linked to pollution exposure, we find positive effects of $PM_{2.5}$ at the 5% level for two diseases besides dementia: chronic obstructive pulmonary disease (COPD) (AME $= 1.79$, $p = 0.002$) and chronic kidney disease (AME $= 1.15$, $p = 0.038$).[54] These results could be interpreted as "reverse placebo tests" in the sense that positive findings may be expected based on prior cohort studies that found that long-term exposure to $PM_{2.5}$ is associated with these diseases (*e.g.* Guo *et al.*, 2018).

8. CONCLUSION

Dementia's global social costs continue to grow with the aging populations of many countries, causing the World Health Organization to label it a "public health priority" and the U.S. Centers for Disease Control to describe it as a "public health crisis." Because no medical preventions or cures exist, policy discussions have focused on investment in research and health infrastructure and modifying behaviours related to smoking, diet, and exercise. Our findings reveal that air quality regulations provide another lever to policy makers to reduce the prevalence of dementia.

Beyond these policy implications, our results provide guidance for additional research on the causes and consequences of dementia. Our study establishes a causal link between long-term,

53. Glaucoma is a progressive disorder with nerve degeneration that is strongly associated with age; fibromyalgia affects mood and behaviour and can be difficult to diagnose; breast cancer and prostate cancer can be slow to progress and have gender-specific diagnosis rates; and peripheral vascular disease is associated with reduced blood circulation.

54. According to the Centers for Medicare and Medicaid Services (2012), the top 15 conditions ranked from most prevalent to least prevalent are high blood pressure, high cholesterol, ischemic heart disease, arthritis, diabetes, heart failure, chronic kidney disease, depression, COPD, Alzheimer's disease, atrial fibrillation, cancer, osteoporosis, asthma, and stroke.

later-in-life exposure to $PM_{2.5}$ and dementia, yet the precise mechanisms and causal pathways remain unknown. Research can investigate how the presence of small particulates in the brain alters cognitive function and relates to Alzheimer's disease specifically, and whether the effects differ across chemical composition, genotypes, comorbidities, stages of life, or other factors. Likewise, our results can help guide efforts to study the broader link between air pollution, cognitive decline, and financial decision making. Such insights can shed light on the economic costs of impaired cognition as well as the value of various approaches to mitigate these costs, whether through the provision of long-term care and long-term care insurance, support for family caregivers, financial decision support, and medical technologies.

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Supplementary Data

Supplementary data are available at the *Review of Economic Studies* online.

Data Availability Statement

The Medicare data used in this study are available through the CMS but restrictions apply to the availability of these data, which were used under licence for the current study, and so are not publicly available. The code and other data underlying this research is available on Zenodo at https://doi.org/10.5281/zenodo.7196076.

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