

Dissecting the causal question underlying the association between cancer and ADRD

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Introduction

There is significant interest in understanding the association between cancer and dementia¹. Many observational studies have consistently determined that individuals with a history or incident cancer have a lower risk of developing of Alzheimer’s disease or related dementias (ADRD) when compared to individuals with no history of cancer. These findings have motivated the identification of molecular and genetic mechanisms that explain this association. Specific molecular mechanisms, such as Pin1 enzyme over-expression promotes tumorigenesis, while it’s down-regulation is attributed to mechanisms that contribute to Alzheimer’s Disease. These findings are suggest new opportunities to repurpose cancer chemotherapeutics for Alzheimer’s Disease.

Nevertheless, observational studies are susceptible to several bias, and researchers have raised concerns related to the competing event of death as well as unmeasured confounding and ascertainment error that could drive these results. Though, to understand sources of bias the causal question should be explicitly specified. This particular question has at least two core challenges. First, defining the underlying biological mechanism we would ideally intervene upon and defining cancer diagnosis as the proxy for that mechanism. For example, researchers refer to biological mechanisms which onset may be prior to cancer diagnosis and to the effect of cancer treatment, both relate to different causal questions and would be translated into different study design and analytic choices. Second, defining the causal question (or estimand) of interest when the competing event of death is interest. Previous studies alluded to choosing a particular summary measure or modeling approach to account for death (incidence rate ratios (ref), hazard ratios (ref), multistate models (ref)). However, when competing events are present, there is more than one causal question that can be framed and each question has different interpretations and relies on different assumptions.

With these considerations in mind, we outline a case study focused on the question: *What is the direct effect of Pin1 over-expression at late-midlife in the risk of ADRD after 20 years of follow-up compared to having regulated expression of Pin1.* To identify this effect with real-world data, we illustrate the complexity of the research question by building progressively a directed acyclic graph (DAG) which help us connect the causal question to the observable data and the assumptions we rely on to identify the effect. We exemplify different scenarios with data collected from the Rotterdam Study, a population-based cohort study. We describe the challenges and how they translate into the analytic decisions. Last, we discuss how information on mortality and cause of death can provides insight about the direction of some sources of bias.

Overview of the causal structure and assumptions related to cancer diagnosis as a proxy for Pin1

We begin by illustrating a hypothetical scenario through the causal graph of Figure 1, that depicts a situation where Pin1, represented as P , was measured in all participants in late midlife (for example between age 50 and 60). Based on the measurement of Pin1, participant’s exposure is classified as 1) Over-expressed Pin1 or 2) Regulated Pin1. Although Pin1 expression acts as a biomarker and is not a well-defined intervention, we can allude that sometime in the future people with over-expressed Pin1 could follow a specific treatment that lowers Pin1 under a specified threshold. Furthermore, through out the follow-up each participant followed

a strict monitoring for ADRD onset, represented as Y_{20} , and no participant was lost to follow-up nor died during the study period (no competing events present).

Since Pin1 expression was not a randomized intervention, we consider a set of measured covariates L for which conditional exchangeability holds. Previous studies have described age, sex, educational level and race/ethnicity as the minimal adjusting set of covariates (Ospina). However, environmental and behavioral factors such as smoking, alcohol intake, or chronic diseases which are known to cause microenvironmental changes such as inflammation and changes in tissue remodelling, may translate into Pin1 over-expression and are also related to the development of ADRD. Therefore, had we measured all L we could identify the effect of P in Y_{20} by conditioning on L .

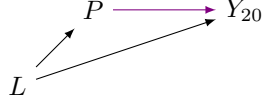


Figure 1: Direct effect of Pin1 in ADRD

Unfortunately, Pin1 expression measurement is not available at the moment in real-world settings, we can only rely on a proxy of this exposure. Since Pin1 over-expression is present in tumors and tumors are only measured through screening and diagnosis, we considered cancer diagnosis as the proxy for Pin1 over-expression, as most observational studies have defined (all refs). We depict this feature in Figure 2, where P^* represents *incident cancer diagnosis*, the measured proxy of P . In this DAG we colored the path $P^* \leftarrow P \rightarrow Y_{85}$ because although we are measuring the association between P^* and Y_{20} in the observed data, we are assuming that the captured effect is only through P .

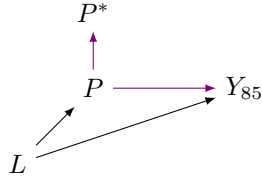


Figure 2: Direct effect of Pin1 in ADRD, with cancer diagnosis as proxy of Pin1

A major challenge related to assigning cancer diagnosis as the proxy of Pin1 is defining time zero. Not everyone who had Pin1 over-expression will be diagnosed with cancer by late-midlife, but in late-life. For this reason prospective cohort studies have considered cancer diagnosis as a time-varying or time-dependent exposure. This means that a participant within a prospective cohort study contributes to the “regulated Pin1” since study entry up to the time of cancer diagnosis and later on to the “Over-expressed Pin1” arm. Other studies have included participants with cancer diagnosis at the time of the diagnosis (for example from cancer registries such as SEER) and matched participants by age. In both cases we must remember that the main interest is on the unmeasured P and we should only adjust for covariates prior to P , and be careful to adjust for post-baseline covariates of P or mediators between P and P^* .

One of the mediators between P and P^* is death prior to cancer diagnosis. Since individuals are at risk of dying from other causes (such as cardiovascular death), we can only measure P^* in the subset of individuals who have survived long enough to have a cancer diagnosis. We zoom in the relationship of $P \rightarrow P^*$ in Figure 3. In this DAG we include $D = 0$ between P and P^* , this illustrates that to observe P^* , we condition (box around $D = 0$) on surviving long enough to have a cancer diagnosis. Several risk factors that increase the risk of cancer might also cause death prior to cancer diagnosis, for example smoking may cause lung cancer and chronic obstructive pulmonary disease (a leading cause of death in this age group). Therefore, to isolate the effect of P in P^* (the violet arrow), we need to block all shared causes between P and D and between D and P^* both represented in Figure 3 as C_1 and C_2 . In this way we assume a hypothetical scenario in which we could prevent death prior to cancer diagnosis, by conditioning on a rich set of covariates. This

assumption must hold regardless of whether we use incident cancer as a time-varying exposure, or match cancer patients to participants free of cancer by age.

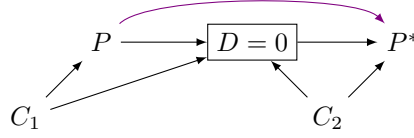


Figure 3: Zoom in the association between P and P star

Up to this point we have outlined the underlying assumptions of using incident cancer diagnosis as a proxy for Pin1 expression. We continue outlining the additional assumptions related to death as a competing event of ADRD.

Overview of the causal question and assumptions related to death as a competing event of ADRD

As illustrated in previous figures, we are interested in the effect of Pin1 in late midlife in ADRD risk after 20 years of follow-up. Nevertheless, mortality is substantial and increases steeply in late life, and a leading cause of death is cancer. This means that in order to measure ADRD over follow-up individuals have to had survive to cancer and to other leading causes of death. For this reason death is a competing event of ADRD because if a participant dies prior to ADRD diagnosis, death prevents from observing ADRD at future time-points. We can visualize this data feature in the causal diagram in Figure 4. In this DAG we include D_{15} as a representation of death at 15 years of follow-up, an arrow between P and D_{15} since P may increase the mortality risk irrespective of cancer diagnosis. We also include an arrow between P^* and D_{15} since cancer diagnosis, and subsequent treatment (or lack of treatment) may have an effect on death. Last, the arrow from D_{15} and Y_{20} represents the key feature of a competing events data structure: an individual who dies at 15 years of follow-up cannot subsequently develop ADRD, and since D_{15} and Y_{20} are events related to aging, C represent the shared causes of both events.

In this DAG we observe that, had we measured and adjusted for L , we could estimate the total effect of P in Y_{20} without further assumptions. However the total effect includes all pathways between P and Y_{20} . This means that if Pin1 has an effect on mortality through cancer or other mechanisms, we will observe a protective effect of Pin1 over-expression in ADRD, partially or fully mediated through death. For this reason estimators that are used to approximate a total effect (JY2020), such as the Aalen-Johansen estimator (underlying estimator for multistate models) or subdistribution hazard models, may not be useful in this setting. Since the question of interest is focused on the direct effect of P in Y_{20} as in Figure 4 (violet arrows), there are several causal questions (estimands) to represent this direct effect, such as: the controlled direct effect (CDE), the survivor average causal effect (SACE) and the natural separable direct effect. In this section we discuss the controlled direct effect as the causal question of interest since it translates to frequently used estimators in this literature (such as Kaplan-Meier estimator and Cox-proportional hazard model).

The CDE represents the effect of Pin1 in ADRD in a setting where we could have prevented death over the study period. It relies on the assumption that we have measured all C to block the pathway $Y_{85} \leftarrow C \rightarrow D_{80} \rightarrow P \rightarrow P^*$. This assumption is defined as the independent censoring assumption conditional on covariates. In this setting death is treated as a censoring event and it can be interpreted as those who died would have the same risk of developing dementia if prevented from dying than those who remained alive and free of dementia at a given time point, conditional on shared causes of dementia and death.

Since the conditional independent censoring assumption is not testable, when there are no other mechanisms of censoring, we can explore the extreme scenarios and estimate the bounds.

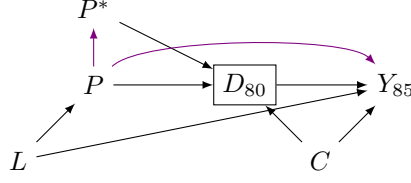


Figure 4: Effect of Pin1 in the risk of ADRD by age 85, with cancer diagnosis as proxy of Pin1

Motivating example with the RS data

In this section we illustrate how previous DAGS may reflect on the analysis performed to unveil the effect of Pin1 in all cause-dementia. We use data collected in the Rotterdam Study, a population-based prospective cohort study among persons living in the Ommoord district in Rotterdam, the Netherlands. Recruitment and initial assessments were held between 1990 and 1993, participants had follow-up visits between 1993-1995, 1997-1999, 2002-2005, and 2008-2010. All participants had data on incident cancer diagnosis and incident dementia diagnosis through follow-up, collected from medical records of general practitioners (including hospital discharge letters) and through linkage with national registries. Date and cause of death was collected on a weekly basis via municipal population registries. Data on clinical outcomes was available until 2015.

To match the analysis to our initial question, we considered as inclusion criteria being between 60 and 70 years old at study entry, without history of cancer diagnosis, free of cognitive decline or with previous history of dementia. Out of 7983 persons who participated at baseline, 3642 were considered eligible. Time to cancer diagnosis, to dementia diagnosis and death status was measured for all participants. All participants were followed from study entry until dementia diagnosis, death or 20 years after their individual baseline date, whichever occurred first.

Participants had a mean age of 64.46 (SD: 2.86), and 54% ($n = 1981$) were women. Further details on participants are presented in Table 1. Over follow up, 24% ($n = 878$) developed cancer and 76% ($n = 2764$) remain free of cancer diagnosis, the median age of cancer diagnosis was 73 (IQR: 69-77). From the total sample, 12% ($n = 431$) had dementia over follow-up and median time to dementia was 79 (IQR: 75-83). Among participants with incident cancer, 6% ($n = 50$) had dementia diagnosis and 63% ($n = 549$) died over follow-up, 32% ($n = 279$) remain alive at 20 years since study entry. In contrast, among participants free of cancer diagnosis over follow-up, 14% ($n = 385$) were diagnosed with dementia and 23% ($n = 624$) died over follow-up, 63% ($n = 1755$) were alive at the end of follow-up.

We present the following analysis:

The difference in dementia risk treating cancer as ever vs. never (As if Pin1 and cancer dx. happened at the same time if looked retrospectively).

We begin illustrating the most simple scenario (as Figure 1), where Pin1 overexpression is defined as “ever vs. never” and we consider death as an independent censoring mechanism. We consider baseline covariates for adjustment such as age at study entry, sex, educational attainment, smoking status, alcohol intake status.

The difference in dementia risk considering cancer as a time-varying exposure independent of death. For example. at five years from follow-up the risk of cancer was 6.5 (95%CI: 5.8, 7.4) and the risk of death was 2.6 (95%CI: 2.2, 3.2), thus we assume that those who died would have the same risk of cancer had they not died, unconditionally.

Discussion

We need to figure out what the key aging-dependent changes are and how to modulate these factors safely. Causes such as aging, smoking, irradiation etc are causing micro environmental changes, like inflammation and changes in tissue remodelling. This promotes selection for adaptive mutations.

Efforts to prevent and treat cancer should converge with similar efforts to prevent other aging- associated diseases.

We need better means to

Youthful tissues

Knowing the cause of death provides information about the direction of missclassification. In patients with cancer we observed a slight % that died with dementia as a primary cause. On the other hand, among individuals free of cancer, we observed % of individuals who died with cancer as a primary cause.

In cancer you are afraid of missing dementia in cancer free you are afraid of missing cancer dx

Make a table of definition of weights based on cancer def. and

Explicitly outlining the estimands and the assumptions that connect the causal question to the observed data provide an opportunity to improve the design of observational studies and the interpretation of their findings, plus better insight of potential sources of bias. This is a crucial since these studies are providing insights that are guiding other fields of research in the area, from bench science to biostatistics and epidemiological methods¹⁵. To guide future observational and methodologic studies, our aim is to outline the potential estimands of interest that represent the underlying causal questions behind the association between cancer and dementia

Table 1

	0	1
n	2764	878
sex = 1 (%)	1623 (58.7)	358 (40.8)
age_0 (mean (SD))	64.42 (2.86)	64.61 (2.87)
education (%)		
0	1251 (45.3)	347 (39.5)
1	1225 (44.3)	412 (46.9)
2	269 (9.7)	114 (13.0)
Unknown	19 (0.7)	5 (0.6)
apoe4 (%)		
0	1874 (67.8)	622 (70.8)
1	685 (24.8)	203 (23.1)
2	78 (2.8)	18 (2.1)
Unknown	127 (4.6)	35 (4.0)
smoke1 (%)		
0	799 (28.9)	193 (22.0)
1	1301 (47.1)	425 (48.4)
2	664 (24.0)	260 (29.6)
bmi1 (mean (SD))	26.65 (3.80)	26.39 (3.50)
oh1 (mean (SD))	10.13 (15.50)	13.25 (17.97)
sbp1 (mean (SD))	138.67 (20.80)	138.74 (20.98)
ht1 = 1 (%)	1623 (58.7)	509 (58.0)
hd_prev (%)		
0	2503 (90.6)	800 (91.1)
1	203 (7.3)	64 (7.3)
2	58 (2.1)	14 (1.6)
hd_v = 1 (%)	797 (28.8)	213 (24.3)
diabetes_prev (%)		
0	1981 (71.7)	553 (63.0)
1	284 (10.3)	91 (10.4)
2	499 (18.1)	234 (26.7)
diab_v = 1 (%)	549 (19.9)	165 (18.8)
stroke_prev = 1 (%)	47 (1.7)	15 (1.7)
stroke_v = 1 (%)	361 (13.1)	101 (11.5)
cancer_v = 1 (%)	0 (0.0)	878 (100.0)