Supplementary Online Content

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This supplementary material has been provided by the authors to give readers additional information about their work.

eAppendix. Community Servings Home Delivered Meals Programs

eMethods. Technical Aspects

Covariates

We considered 5 categories of covariates for matching and adjustment in the study, as detailed in the table below.

eTable 1: Covariates

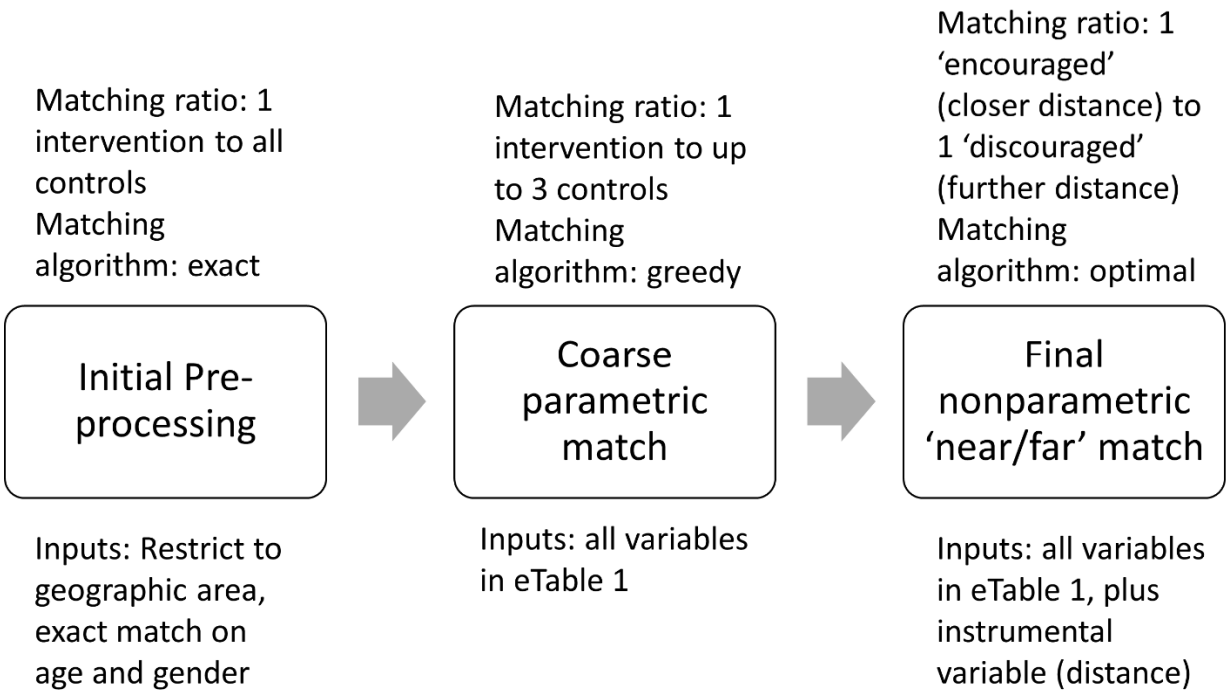
Variable	Category	Source
Age	Demographics	Member Eligibility files
Gender	Demographics	Member Eligibility files
Insurance Type	Demographics	Member Eligibility files
Race/ethnicity	Demographics	Member Eligibility files
Disability Status	Demographics	Member Eligibility files
Gagne Comorbidity Index	Comorbidity	Claims files, based on ICD-9 codes
Human Immunodeficiency Virus Status Indicator	Comorbidity	Claims files, based on ICD-9 codes
Cancer	Comorbidity	Claims files, based on ICD-9 codes
End Stage Renal Disease	Comorbidity	Claims files, based on ICD-9 codes
Diabetes Mellitus	Comorbidity	Claims files, based on ICD-9 codes
Congestive Heart Failure	Comorbidity	Claims files, based on ICD-9 codes
Presence of a 'Triggering Event' (Inpatient Admission within 6 Months of Index Date	Healthcare Use	Claims Files
Number of Inpatient Admissions	Healthcare Use	Claims Files
Number of Skilled Nursing Facility Admissions	Healthcare Use	Claims Files
Number of Home Health Visits	Healthcare Use	Claims Files
Total Medical Costs	Healthcare Use	Claims Files
Total Pharmaceutical Costs	Healthcare Use	Claims Files
High-Dimensional Propensity Score	High-Dimensional Propensity Score	Claims files, based on ICD-9 codes

*All data taken from 360 days prior to index date

Data Preprocessing and Matching

Because near/far matching is computationally intensive[1], and the Massachusetts All-Payer Claims Database is very large (over 7 million individual adults) we proceeded in several steps to conduct the matching, following the flowchart below.

eFigure 1: Flowchart of Data Preprocessing and Matching Procedures



Statistical Analysis

Our major concern when analyzing the data for this study was to address the potential for confounding introduced by non-random assignment to the intervention. To do this we used an instrumental variable analysis approach called 'near/far' matching.[1,2] This method seeks

to construct a matched cohort that is as similar as possible on relevant sociodemographic and clinical characteristics, but that differs in whether an individual was 'encouraged' or 'discouraged' to receive the intervention based on their value of an instrumental variable. One can view a standard matching algorithm as trying to minimize the multi-dimensional 'distance' between a set of variables (for example, by viewing a 61-year old Hispanic female as being 'closer' to a 62-year old Hispanic female than a 55-year old non-Hispanic Black female). Near/far matching extends this approach by trying to minimize the distance between all covariates except one—the instrumental variable, for which it tries to maximize the distance between 2 pairs. This results in a set of matched pairs that are as similar as possible on all included covariates, while simultaneously being as far apart as possible on the value of their instrumental variable. This effectively emulates a 'matched-pairs' randomized trial design where two individuals who are similar on baseline characteristics are randomly assigned to receive different treatments. This has the effect of tending to strengthen the instrumental variable as when baseline characteristics are more similar between individuals, the instrumental variable is more likely to make the difference as to whether the individual receives the treatment or not. Since a formal statistical justification for near/far matching is beyond the scope of this paper, we recommend as an introduction to the method the paper "Near/far matching: a study design approach to instrumental variables" by Baiocchi et al.[2]

In this study, the instrumental variable was the distance an individual lived from Community Servings, which should subtly 'encourage' those living closer to enroll. In short, we attempt to address the potential for confounding by observed variables by matching and by unobserved variables by using the instrument. The function of distance as an

instrument in this study was justified on the basis of 1) being likely to affect receipt of the intervention (with individuals living closer more likely to receive the intervention), 2) not being associated with the outcome except by receipt of the intervention (as, within the study area, there is no reason to think that proximity to Community Servings as an organization, as opposed to proximity to a clinic or hospital, would otherwise affect healthcare utilization), and 3) there being no expected confounding between the instrument and the intervention, as individuals are unlikely to choose where they live based on proximity to Community Servings. We formally test the first assumption by examining the strength of the instrument using the F-statistic for a regression of distance on meal program participation (generally, an F-statistic > 13 is considered a sufficiently 'strong' instrument[3]), and by examining how the odds of participation decreased as one lived farther away from Community Servings.

The second and third assumptions are not formally testable, as there could always be unmeasured confounding. Instead, to support the role of distance as an instrument in this case, we present a balance table showing that, when categorized by distance rather than receipt of the intervention, balance on important confounding variables improves, even without matching. This suggests that there is an element of 'randomization' introduced by the instrument, at least for the observed covariates.

In testing the assumptions of our instruments, we several analyses demonstrated that living closer to Community Servings was associated with higher likelihood of receiving the intervention. The first stage F-statistic for prediction of intervention receipt by distance was 218.62, and the odds ratio for receipt by distance was 0.96 (95% confidence interval

[95%CI] 0.95 to 0.97, p<.0001), meaning that for every 1km farther from Community Servings an individual lived, the odds of intervention receipt decreased by 4%. Further, categorizing individuals by distance from Community Servings led to all substantially better covariate balance (**eTable 2**), which supports the role of distance as an instrumental variable. **eTable 2** presents results of a dichotomized version of the instrumental variable, which is necessary to calculate a standardized mean difference. This was done for illustrative purposes in testing the instrument, and made use of an arbitrary dichotomization. However, since distance is a continuous variable, we also conducted additional tests of association between the candidate instrument and measured covariates to examine whether there was an association between the candidate instrument and these factors that may affect the outcome. We found no evidence of such an association, with very weak F-statistics (3.15 was the highest, most values less than 1) and low amounts of variation in the covariates explained by the candidate instrument (generally less than 1%). These results are presented in **eTable 3**.

After conducting the matches and instrument testing as above, we conducted statistical analyses using the two-stage residual inclusion (2SRI) approach to instrumental variable analyses.[4] We used this approach because the outcomes we were modeling, healthcare utilization and cost, are not well modelled using ordinary least squares regression, which is required for the alternative, two-stage least squares, approach to instrumental variable analysis, and because our treatment was binary rather than continuous. Using the 2SRI approach we fit a first-stage logistic regression model that predicts receipt of the intervention using distance, the instrumental variable, and adjusting for the above mentioned covariates (**eTable 1**). Next, the residuals, defined as the

difference between the observed and predicted values from the first stage model are calculated. Finally, a second-stage model, which can be a generalized linear model, is fit by regressing the outcome on receipt of the intervention, along with the residuals from the first-stage model and the other covariates. For the event data (inpatient admission, skilled nursing facility admissions, and emergency department admissions) we fit Poisson regression models with a log link in the second stage. For the spending data, we fit gamma regression models with a log link, selecting the gamma distribution after conducting modified Park tests as suggested by Manning and Mullahy.[5] All models adjusted for covariates to account for any residual imbalance after matching. Analyses also adjusted for the index date to account for secular trends. Our analyses followed the intention-to-treat approach whereby individuals who enrolled in the intervention continued to be analyzed as part of the intervention even if they stopped participating.

To express the results of these models on the absolute (risk difference) and relative (risk ratio) scale, we used the method of recycled predictions, also known as the parametric g-formula, which, after fitting the model in the original dataset, generates predicted values for the outcome in datasets where all participants are artificially set to have received and not received the intervention.[6] This standardizes the predictions over the observed distribution of the relevant covariates. To obtain confidence intervals, we used a nonparametric bootstrap of the entire process (both the first- and second-stage models), with 1000 replications.[4]

As sensitivity analyses, we also sought to quantify the amount of unmeasured confounding that would be required to render the observed treatment-outcome association

null (risk ratio of 1). We did this using the E-Value approach.[7,8] This approach quantifies the minimum strength of association an unobserved confounder would have to have with both the treatment and the outcome in order to adjust the observed treatment-outcome association to 1. We present both bias plots (which plot the frontier of the necessary confounder-treatment and cofounder-outcome association) (**eFigures 2a-2c**) and a table (**eTable 5**) presenting the E-Values (minimum strength of association needed) for each of the three main outcomes (inpatient admissions, skilled nursing admissions, and healthcare expenditures). These show that it would take very strong unobserved confounding, stronger than that of any observed variable, to render the observed treatment-outcome association null.

eTable 2: Balance Table Comparing Standardized Mean Differences in Demographic and Clinical Characteristics by Community Servings Participation Status and by Distance to Community Servings

	Standardized mean difference between those who did and did not participate in Community Servings	Standardized mean difference between those who did and did not live within 30 kilometers of Community Servings
Age, years	0.0849	0.0204
Female	0.1709	0.0241
Race/ethnicity	0.7976	0.2136
Insurance	0.8900	0.1531
Participant on disability	0.6725	0.1140
Experienced 'triggering event'	0.8713	0.0350
Number of inpatient visits in past 12 months	0.3101	0.0254
Number of skilled nursing facility visits in past 12 months	0.0601	0.0094
Number of home health visits in past 12 months	0.3400	0.0134
Total healthcare costs in past 12 months, $	0.2851	0.0063
Comorbidity index	1.2831	0.0474
Human immunodeficiency virus positive	0.7112	0.0488
History of cancer	0.6488	0.0078
History of end stage renal disease	0.8167	0.0369
History of diabetes mellitus	0.7039	0.0485
History of congestive heart failure	0.7392	0.0363
Percent living in poverty in zip-code tabulation area	1.2086	0.1195[a]
Emergency department visits in healthcare service area	0.2909	0.2006
[a]adjusted for repeated measurements within zip-code tabulation area		

eTable 3: Association Between Instrumental Variable (Distance to Community Servings) and Covariates

	F-statistic	R-squared
Age, years	0.06	0.00003
Female	0.01	0.0003
Race/ethnicity	0.06	0.005
Insurance	1.24	0.004
Participant on disability	1.34	0.004
Experienced 'triggering event'	0.34	0.0001
Number of inpatient visits in past 12 months	3.15	0.004
Number of skilled nursing facility visits in past 12 months	0.29	0.0005
Number of home health visits in past 12 months	2.04	0.006
Total healthcare costs in past 12 months, $	0.24	0.002
Comorbidity index	0.05	0.0002
Human immunodeficiency virus positive	0.04	0.0008
History of cancer	0.49	0.0002
History of end stage renal disease	1.27	0.001
History of diabetes mellitus	0.32	0.001
History of congestive heart failure	2.61	0.00002
Percent living in poverty in zip-code tabulation area	0.75	0.029
Emergency department visits in healthcare service area	0.36	0.002

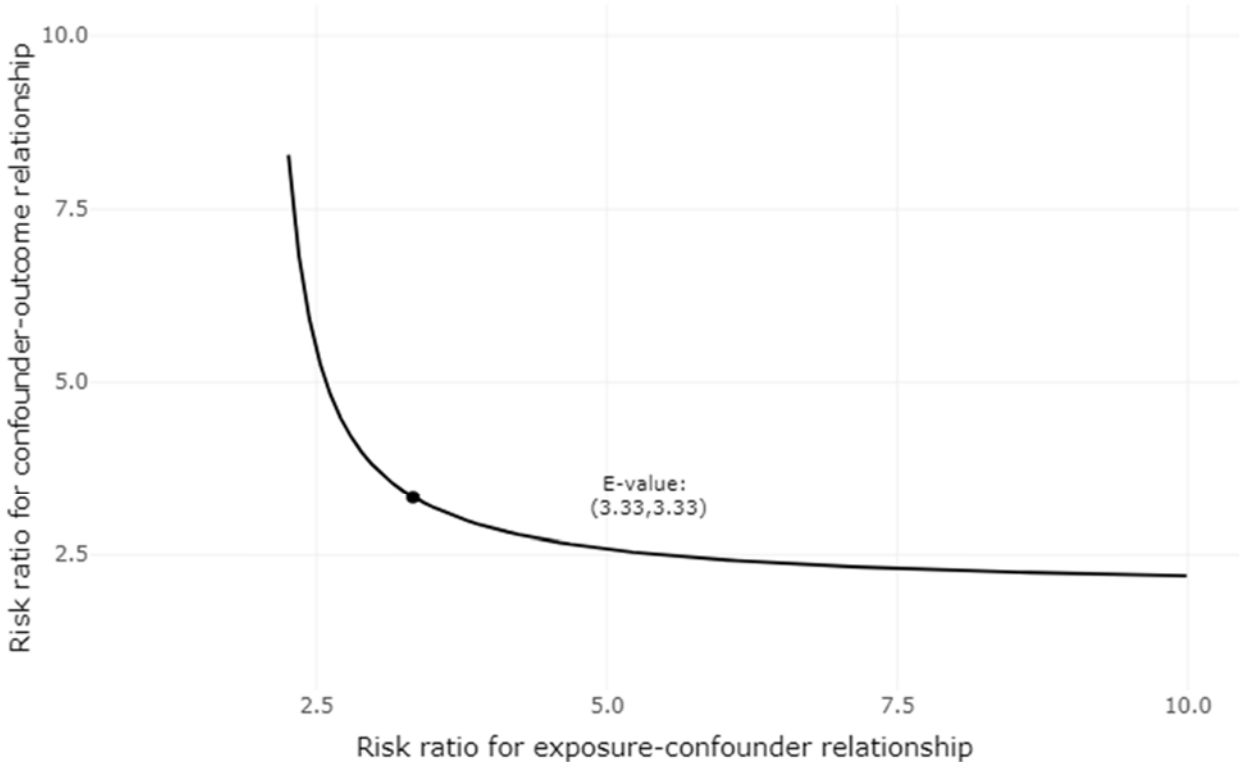
eTable 4: Sensitivity Analyses for Primary and Secondary Outcomes After Near/Far Matching Analysis, Using Non-Winsorized Outcomes

	Incidence rate ratio (95% CI)	Risk Difference per 1000 person years (95% CI)
Inpatient Admissions	0.49 (0.04 to 0.94)	-624 (-993 to -254)
Skilled Nursing Facility Admissions	0.35 (0.01 to 0.72)	-967 (-1934 to -496)
	Relative risk of mean per person per month expenditures (95% CI)	Difference in mean per person per month expenditures (95% CI)
Healthcare Costs	0.76 (0.57 to 0.94)	-$1858.53 (-$2880.15 to -$836.91)

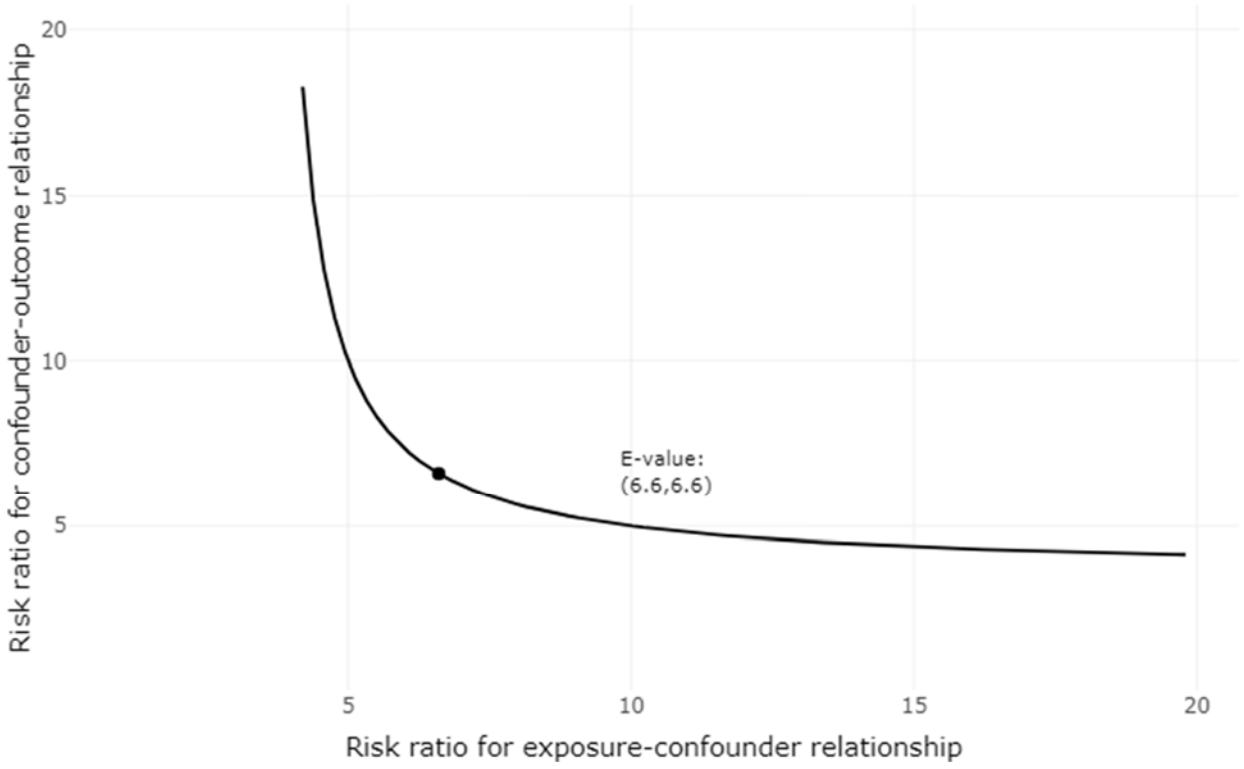
eFigure 2. Bias Plots

Bias plots represent varying levels of unobserved confounder-treatment association and unobserved confounder-outcome association necessary to render observed treatment-outcome association null for inpatient admission (2a), Skilled Nursing Facility Admissios (2b) and healthcare expenditures (2c).[7,8]

eFigure 2a:



eFigure 2b:



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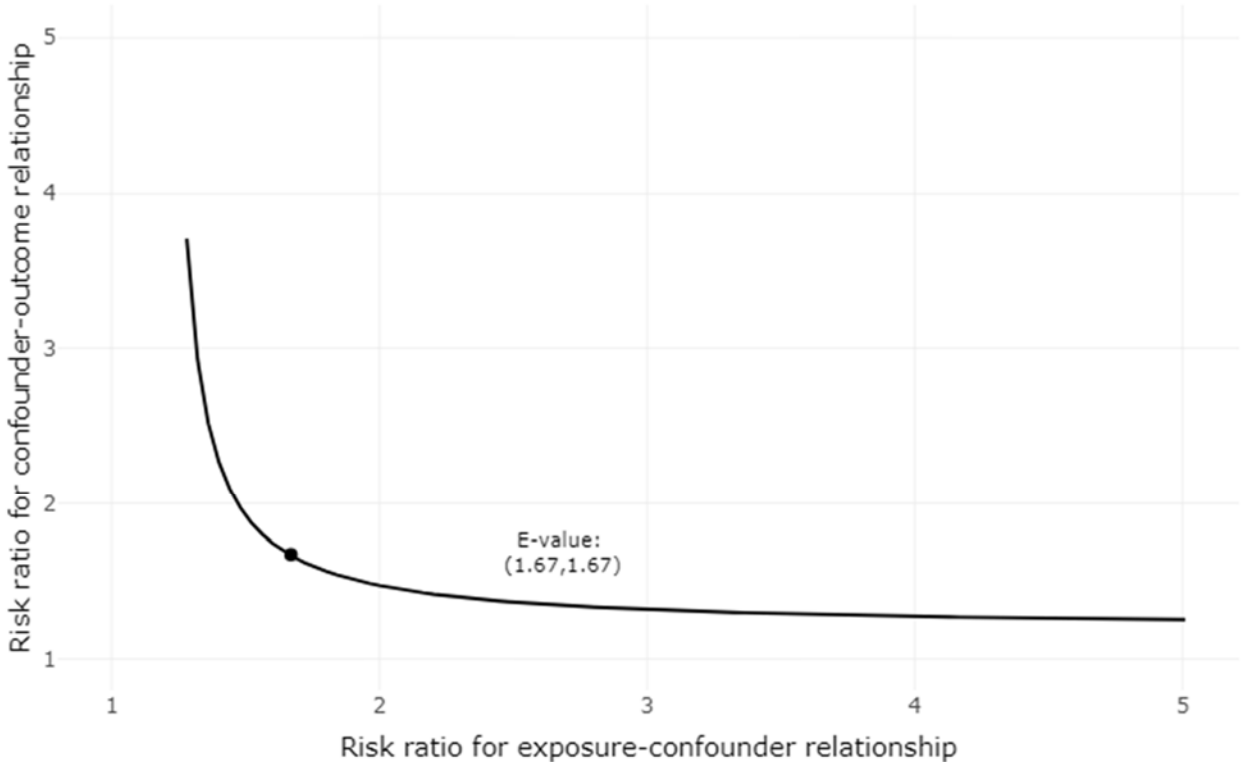

eTable 5: Sensitivity Analyses to Quantify Magnitude of Residual Confounding Necessary to Shift Estimate of Association to 1 (No Association)

	Relative Risk of Association of Variable with Receipt of Intervention*	Relative Risk of Variable with Inpatient Admissions	Relative Risk of Variable with Skilled Nursing Facility Admissions	Relative Risk of Variable with Healthcare Costs
E-Value	**n/a**	**3.33**	**6.60**	**1.67**
Age, years	1.02	1.00	1.05	1.00
Female	1.06	1.04	1.19	1.06
Race/ethnicity	1.33	1.69	1.85	1.25
Insurance	1.79	1.72	1.61	1.39
Participant on disability	1.82	1.18	1.02	1.16
Experienced 'triggering event'	2.18	1.36	2.60	1.41
Number of inpatient visits in past 12 months	1.04	1.09	1.19	1.14
Number of skilled nursing facility visits in past 12 months	1.01	1.02	1.05	1.01
Number of home health visits in past 12 months	1.00	1.00	1.00	1.00
Total healthcare costs in past 12 months, $	1.00	1.00	1.00	1.00
Comorbidity index	1.01	1.03	1.02	1.05
Human immunodeficiency virus positive	1.01	1.20	1.82	1.90
History of cancer	1.13	1.00	1.10	1.11

History of end stage renal disease	1.36	1.48	1.27	1.20
History of diabetes mellitus	1.15	1.37	2.36	1.10
History of congestive heart failure	1.22	1.04	1.32	1.02
Percent living in poverty in zip-code tabulation area	1.00	1.00	1.00	1.01

Tables present 'E-values', the minimum strength of association an unmeasured confounder would have to possess between both the treatment and the outcome in order to reduce the observed association between the treatment and outcome to 1 (no association) on the relative scale. For example, for an E-Value of 2, an unmeasured confounder would have to have relative risk of association of ≥2 for the outcome *and* a relative risk of association of ≥2 to make the treatment-outcome association null. For comparison, the associations of measured covariates between either the treatment or outcome, expressed as relative risks, are also presented.

*To facilitate comparison to the E-Value, all relative risks less than 1 have been inverted so that associations expressed are greater than 1. For example, if relative risk was 0.5 this would be inverted to 1/0.5 = 2.[7,8]

eReferences

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