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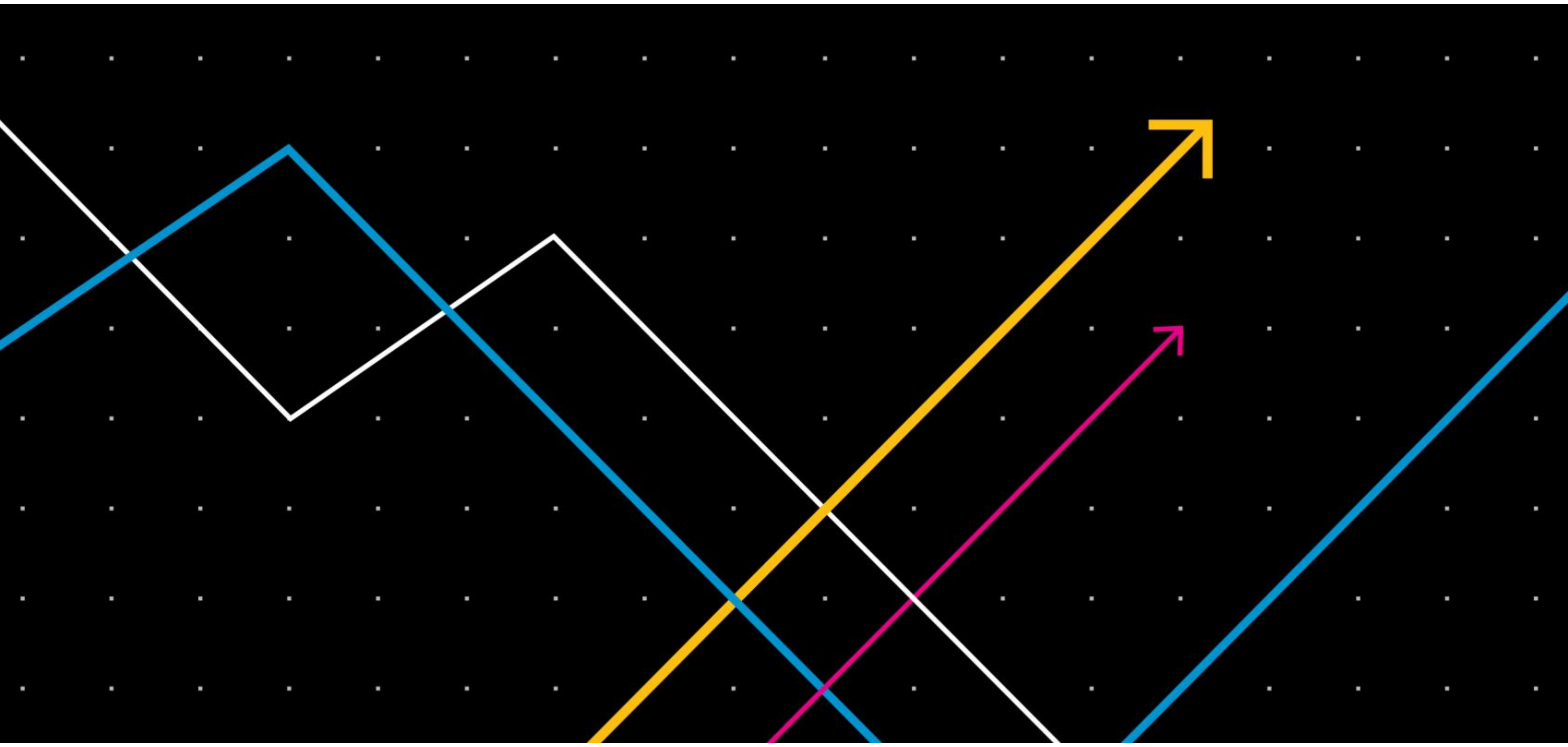

RESEARCH REPORT

# How Should We Measure and Interpret Racial and Ethnic Disparities in Health Care?

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URBAN INSTITUTE

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# Executive Summary

**Racial disparities in health and health care are well documented and long-standing in the US health care system. Efforts to eliminate disparities are a critical national priority, and quantifying and interpreting measures of disparities in health care are important but challenging parts of that effort. Broadly, a health care disparity can be defined as a difference in health care access or use between two population subgroups because of which the less socially advantaged group underperforms compared with its more advantaged counterpart for reasons deemed avoidable, unnecessary, or unjust (Whitehead 1992). In 2003, the Institute of Medicine (IOM) offered a more specific definition of disparity in health care access and use when evaluating health care system performance (Smedley, Stith, and Nelson 2003). Under the IOM definition, racial and ethnic differences in health care access and use for reasons outside of clinical need and patient preferences are considered unjustifiable and thus constitute a disparity. Under this definition, differences due to age, sex, health status, and patient preferences are considered acceptable or just. Though this is not the only valid definition, it provides a clear example of how to explicitly define a disparity and use a conceptual framework that defines just and unjust drivers of differences to guide interpretation.**

Many studies of health care disparities fail to use such a clear disparity definition and framework for analysis, however, and this can result in estimates that are ill defined and subject to misinterpretation. For example, when studies provide multiple estimates of disparities that control for different sets of covariates without a strong justification for each or an *a priori* statement of their preferred approach, interpretation is challenging. We provide an example of such a study that includes four estimation approaches resulting in disparity estimates that vary considerably. The most heavily adjusted estimates are reported in the abstract, without justification for the chosen approach or sufficient guidance for interpreting the other estimates. Furthermore, many studies fail to consider or incorporate the role of systemic racism within a conceptual framework used to interpret disparity estimates. Though systemic racism may be difficult to measure, it is critical to discuss its role in driving disparities in the context of any study and to what extent it is correlated with other measured drivers or captured in a residual.

This paper offers the following five recommendations for producing and interpreting estimates of racial and ethnic disparities in health care:

1. Include a clear and explicit definition of the health care disparity of interest and a corresponding method that applies the definition to estimate the magnitude and/or drivers of the disparity.
2. Provide a conceptual framework that considers or incorporates the role of systemic racism as an unjust driver of group differences to guide the interpretation of study results.
3. Discuss the data limitations in applying a given definition of disparity, including the measurement of race and ethnicity.
4. Estimate a model with a comprehensive set of covariates consistent with the conceptual framework and use the appropriate components of the estimated model to calculate the magnitude of the disparity as defined.
5. Investigate models that move beyond documenting disparities to analyzing contributing factors.

Each recommendation includes caveats and conditions for producing and interpreting disparity estimates, as described in the following sections. We highlight two published studies that we see as exemplars of work on disparities that align with many of our recommendations (Cook et al. 2017; Kirby, Taliaferro, and Zuvekas 2006).

To further illustrate the value of these recommendations, we provide several empirical examples using data from the National Health Interview Survey. These examples demonstrate some of the ways in which disparity estimates and interpretation can vary with different definitions and estimation methods, thereby affirming the importance of motivating these choices with an explicit definition, research question, and comprehensive conceptual framework. Key findings from our empirical analyses include the following:

- Disparity estimates can vary widely when using different disparity definitions. Clearly defining the disparity with a supporting conceptual framework—rather than defining it implicitly based on the included covariates in a regression model—is critical for interpretation. Whether and how to adjust for health status when estimating disparities in health care access and use is a complex decision that should be guided by a conceptual framework with careful attention to how different health status measures reflect need versus access to care. *When a study presents multiple disparity estimates with different sets of covariates and no explicit disparity definition (which is currently a common approach), the study is implicitly using multiple disparity definitions.*

- Estimates of a single disparity definition (e.g., the IOM definition) and its component parts vary modestly across estimation approaches, but our examples indicate that an approach using a more a comprehensive model with a rich set of covariates is less likely to understate the IOM disparity.
- If the data demonstrate a differential effect of socioeconomic status or other covariates on outcomes by racial group, estimates of the disparity and the drivers of disparity may be improved by estimating separate models by race.

Clear, high-quality measures of racial and ethnic disparities in health care use, with explicit definitions and interpretations, are critical to understanding disparities and exploring their causal factors. Well-defined and interpretable estimates are necessary to produce more actionable information to address disparities, guide and evaluate interventions to reduce and eliminate disparities in health care use, and set and prioritize equity policy agendas for policymakers and the public.

# How Should We Measure and Interpret Racial and Ethnic Disparities in Health Care?

**Racial disparities in health and health care are well documented and long-standing in the US health care system. Efforts to eliminate disparities are increasingly recognized as a critical national priority. More clarity in defining, measuring, and interpreting racial and ethnic health care disparities would facilitate progress toward that goal. Though the literature documenting such disparities is extensive, difficulties in measuring and interpreting them remain, particularly with respect to the role of systemic racism. Systemic racism includes not only discrimination within the health care system (Smedley, Stith, and Nelson 2003) but also socioeconomic disadvantages and systematic disinvestment in communities of color (Bailey et al. 2017).**

To demonstrate some of the challenges in interpreting results in this area, we consider a recent study in *JAMA Health Forum*, "Disparities in Health Care Spending and Utilization among Black and White Medicaid Enrollees" (Wallace et al. 2022). The authors present four estimates of disparities for various outcomes using a series of regression adjustments that do not follow from a specified disparities definition or conceptual model. The paper presents a series of disparities estimates in which the different adjustment approaches produce various conflicting results, including changes in the sign of a key outcome; the unadjusted difference in the share receiving any primary care in a year for Black versus white adults is –3.5 percentage points; the difference adjusted for sex, age, disability eligibility category, and zip code is –0.33 percentage points; the difference with additional health status adjustments is 0.87 percentage points; and the difference with additional adjustments for provider characteristics is –0.44 percentage points.

Thus, the conclusions of the paper are dependent on the covariates chosen and the set of estimates highlighted by the authors. The authors highlight their fully adjusted results, a choice not motivated by definitions, research questions, or justification of chosen covariates based on a conceptual framework. In this case, if the goal of the analysis is to assess Black-white differences in health care use among patients who have similar sexes, ages, and health statuses, live in similar areas, and visit similar providers, extensive adjustment for underlying health and economic factors may be appropriate. If the goal is to understand disparities or unjust differences in these outcomes by race more broadly, however, adjusting for factors like zip code and provider characteristics may minimize the role of structural factors that cause worse outcomes among the Black population. In this study and many like it, the definition of "disparity," the authors' research question, and related methodological choices are unclear; thus, the findings are subject to misinterpretation.

The objective of this paper is to offer recommendations that researchers should consider when producing and presenting estimates of racial disparities. We are not the first to provide guidance on this issue and our recommendations include many insights from prior work in this area (Cook, McGuire, and Zaslavsky 2012; McGuire et al. 2006), but we include some updated guidance for considering the role of systemic racism and structural factors, including social determinants of health such as socioeconomic status, in defining and measuring disparities. We also illustrate our points using both published studies and our own empirical analysis using data from the National Health Interview Survey (NHIS).

# Recommendations for Producing and Interpreting Estimates of Health Care Disparities

## 1. Include a Clear and Explicit Definition of the Health Care Disparity of Interest and a Corresponding Method to Estimate the Disparity

Definitions of health care disparities vary across studies, and sometimes the definition is neither explicitly stated nor even implied. A disparity can refer to any type of difference, but it is often used to connote differences considered to be unfair. As described by the scholar Paula Braveman, health research lacks consensus about the meaning of the terms *health differences*, *health disparities*, and *health* inequities (Braveman 2006). She defines a health disparity as a particular type of health difference in which disadvantaged social groups "systematically experience worse health or greater health risks than more advantaged social groups" (Braveman 2014). She also suggests using a more concise definition developed by Margaret Whitehead that defines health disparities as differences that are "avoidable, unnecessary, and unjust" (Whitehead 1992).

More recently, some scholars have used the term *inequity* to mean "an unfair difference between two groups that is driven by structural factors," while using the term *disparity* to refer to any difference regardless of cause.[1] Other scholars suggest, however, that inequity is a broader concept that identifies differences from an optimal level of health care access, use, or outcomes (Gaskin 2021). This optimal level may or may not be well approximated by the level achieved by a more advantaged social group; in this context, the definition of inequity relies on an aspirational concept that can capture unjust differences experienced by all groups rather than a single disadvantaged group. Given these varied, evolving, and sometimes conflicting definitions, it is critical for researchers to determine and state their definitions up front. The factors that determine the definition to be employed may include the research question or study goals, the intended use of the measure, or the sources of difference that the researchers intend the disparity measure to include or exclude.

Researchers can then choose an appropriate method to estimate the disparity of interest that is consistent with the stated definition. Thus, an approach to estimating disparities involves a definition paired with an estimation method. Common approaches to estimating disparities, as applied in practice, fall into one of three categories (Cook, McGuire, and Zaslavsky 2012). In some cases, the definition and estimation method pair are unique and simultaneously determined (e.g., in a total difference approach), while in other cases, more than one method may be used to estimate a disparity under a given definition (e.g., in the Institute of Medicine approach, described below). Finally, an estimation method may implicitly impose a definition if the researchers have not otherwise stated one (e.g., in a residual direct effect approach). We discuss each of these in more detail in the following sections.

### THE TOTAL DIFFERENCE APPROACH

The total difference approach, employed by the Agency for Healthcare Research and Quality in its annual *National Healthcare Disparities Report* and other empirical studies (Chaves et al. 2020; Cook et al. 2017), defines a disparity in a health care outcome as the entire difference in the outcome between groups. This definition leads to the difference in group means as the estimation method, and conversely, estimating the disparity as the difference in group means defines the disparity of interest as the total difference in outcomes. The total difference approach does not attempt to adjust for differences in outcomes that may be appropriate, such as those stemming from differences in age, sex, and need for health care. The total difference approach is easy to apply and is not dependent on covariate availability in a given dataset. Reporting total differences can be a powerful way to describe the actual state of the world, and it is useful as a first step to motivate further study. The total difference approach is best used to answer questions about the overall magnitude of a difference between groups when the portion of difference deemed to be just or unjust is not relevant to the research question or as a starting point in an investigation of multiple potential drivers of a difference.

### THE INSTITUTE OF MEDICINE APPROACH

A second approach is to use the definition of health care disparity developed by the IOM in its *Unequal Treatment* report (Smedley, Stith, and Nelson 2003), combined with a suitable estimation method. Though the IOM is now known as the National Academy of Medicine, we refer to an approach employing this definition as an IOM approach. The IOM defined a health care disparity as a difference in health care services received by two groups not due to differences in underlying health care needs (which may be measured by age, sex, health conditions, and other health status measures) or preferences of members of the groups. This definition indicates the IOM's determination that health care differences between groups stemming from differences in health and patient preferences may be considered fair or just differences. All other sources of differences are considered unjust, including differences driven by patient socioeconomic status and other social determinants of health and inequities in the operation of the health care system, which may reflect past and present effects of systemic racism.

The IOM definition of disparity is based on a clearly articulated principle—the definition stands independent of the data and methods used to estimate the magnitude of the disparity. Multiple methods may be used to estimate disparities under the IOM definition, as described in recommendation 4. Implementing an IOM approach still depends on which measures of health needs and preferences are available and used by researchers, and room for variation across studies is significant, particularly with respect to adjustments for health status. The IOM approach is best used (1) to answer questions about the magnitude of unjust differences between groups in cases where differences are not due to clinical need or patient preferences and (2) to further explore the underlying drivers of these unjust differences.

### THE RESIDUAL DIRECT EFFECT APPROACH

A third approach, the residual direct effect approach, is perhaps the most common and estimates a disparity as the remaining difference between groups after adjusting, via multivariate regression, for any of a wide range of covariates a researcher selects. Covariates may include health needs but also socioeconomic status–related measures such as education, employment (Cook et al. 2009), and other social determinants of health or area-level measures such as the Neighborhood Deprivation Index. Studies will often estimate a disparity as a residual direct effect without providing an explicit definition of disparity; in those cases, the disparity is implicitly defined as the residual direct effect. This approach is common; one example is the Medicaid enrollee study described in the executive summary (Wallace et al. 2022). Another recent study estimated racial disparities in the use of a common device implanted to prevent heart failure and presented a large number of models (Cascino et al. 2022).[2] In both cases, the authors present multiple residual direct effect specifications, and each model specification imposes its own implicit disparity definition. Although common, this approach leads to various potential disparity interpretations and often does not provide sufficient context to determine clear conclusions.

A study can explicitly present a disparity definition and then estimate it as a residual direct effect, however. For example, one might estimate disparities under the IOM definition as a residual direct effect in a regression that controlled for age, sex, health status, and preference measures. We would refer to this particular case as an IOM approach because it applies the IOM definition but estimates it as a residual direct effect. The residual direct effect approach can also be applicable for a specific type of research question in which the primary concern is determining what portion of the difference between groups cannot be explained by any other observable factor (perhaps observable to the provider or the researcher); for example, a question such as, "At the point of care and given a set of known characteristics that may influence health care use, is there a difference by race in the rate of a particular type of care?" In this case, the researcher could control for all information known to the provider, rather than controlling for specific concepts within a conceptual framework. While such a measure is likely to control away differences that may be considered unfair, it would leave behind a difference that is uncorrelated with any observable factors and therefore represent a maximally conservative estimate of a difference by race.

In general, the residual direct effect approach is best used to answer questions about the remaining difference between groups after controlling for differences in specified observable characteristics. With a well-specified conceptual framework, this approach can estimate the magnitude of unjust differences in instances when the definition of "unjust" may differ from the IOM concept. It can also further identify specific drivers of the differences between groups. The key weakness of this approach is that it is often used to present several implicitly defined disparity estimates side by side without providing the necessary context to allow interpretation of the results.

No single approach to defining and estimating disparities is necessarily superior to others in all cases. In this report, however, we advocate for distinguishing between differences and disparities, where disparities reflect differences between groups driven by unjust factors. Just and unjust factors should be defined using a conceptual framework, and estimations should seek to calculate the magnitude of the disparity and further explore specific factors that contribute to it.

## 2. Provide a Conceptual Framework That Considers or Incorporates the Role of Systemic Racism as an Unjust Driver of Group Differences to Guide the Interpretation of Study Results

When researching health care disparities, a conceptual framework should be used to identify the mechanisms and processes that could lead to them. Over time, many frameworks have been developed to explain the drivers of health care disparities (Bowleg 2012; Crenshaw 1991; Ford and Airhihenbuwa 2010; WHO 2010).[3] These drivers include differences in individual demographic and socioeconomic characteristics and their interactions; political, social, and community contexts; neighborhoods and physical environments; health and the health care system; and attitudes, beliefs, and perceptions. Moreover, many of these differences have been driven by historic and current policies and practices that have systematically excluded people of color from opportunities for education, employment, homeownership, and health, along with more explicit discrimination and unfair treatment inside and outside the health care system. Thus, when developing a conceptual framework to explain current disparities, it is critical to consider how the legacy of systemic racism may drive differences between groups in the context of the study and to further consider or incorporate discriminatory policies and practices that may still operate today and lead to differences in outcomes. The more explicit researchers can be about the present policies, institutional actions, and practices that lead to disparities (especially where levers of change exist), the better we can interpret estimates of disparities, test hypotheses of mechanisms with data, and develop policies to reduce disparities.

A clear and comprehensive conceptual framework is helpful for interpreting any disparity estimate. For example, even a total difference estimate can benefit from a well-specified framework that details factors contributing to that difference. Under the IOM conceptual framework, the key distinction is that differences in health care use can be due to differences in clinical need and preferences or other factors, and that only those differences due to clinical need and preferences are justifiable. Though this is a strong starting point, the IOM approach has some limitations that may require conceptual modifications in implementation and interpretation. In particular, clinical need has typically been defined to include age, sex, and health status. Though age and sex are well defined, it is less clear what health status measures should and should not be incorporated. Health status measures may include self-reports of mental or physical health, self-reports of diagnosed conditions, provider reports of diagnosed conditions, and more. One potential concern with using diagnosed conditions as a measure of need is that such measures also capture the access to care needed to get a diagnosis. This difference in access may not be considered a justifiable reason for a difference in use and should therefore be captured in the disparity measure. Furthermore, several studies have found that the magnitudes of racial and ethnic disparities in health care use increase with worse health (Biener and Zukevas 2019, 2020) and advocate for stratifying samples by health status as an alternative approach to addressing the role of underlying heath status in estimating the magnitude of disparities.

Such decisions become more complex when considering the role of systemic disadvantages and structural racism as drivers of underlying health status and preferences (Bailey et al. 2017; Braveman et al. 2022; Gee and Ford 2011; Hardeman et al. 2022). If poor underlying health status reflects these structural factors, should differences in health care use driven by poor health status be considered justifiable? In this case, though the need may be driven by unjust factors, the need is real and therefore adjusting for health status is likely appropriate. For example, estimating the disparity in use of specialist care should account for the fact that Black patients often have poorer health and thus greater need for health care even if these higher health needs are driven by long-standing systemic factors. A series of studies has clarified that under the IOM approach, differences in underlying health care needs should be defined as the portion of differences due to age, sex, health conditions, and other health status measures uncorrelated with socioeconomic status and systemic inequities (McGuire et al. 2006).

In addition, recent literature has found that health care preferences are influenced by factors such as past discrimination in health care settings (Progovac et al. 2020; Sonik et al. 2020). Thus, adjusting for health preferences without addressing preferences that may be correlated with systemic inequalities may not be consistent with the IOM definition of disparity. If historic mistreatment by the health system has generated mistrust among Black patients, should that mistrust be considered a justifiable reason for seeking and receiving less care? These are challenging decisions for researchers, but ones that should be grappled with when developing a conceptual framework that strives to distinguish between justifiable and unjustifiable differences. Preferences for culturally and linguistically effective care may be justifiable reasons for differences in health care use, for example. Though studies motivated by the IOM definition and conceptual framework may benefit from stronger descriptions of which health status and preference measures are included and why, many studies provide little insight on why specific variables are included or excluded or what parts of the total difference in outcomes are justified or unjustified (Cascino et al. 2022; Kenney, Coyer, and Anderson 2013; Rethy et al. 2020; Waidmann 2009; Wallace et al. 2022). This can be particularly confusing when a study presents multiple adjustments without offering clear guidance for interpreting the resulting estimates. One recent example is a research letter published in *JAMA Internal Medicine* (Cai et al. 2021). It presents nationwide racial and ethnic disparities in outpatient visits to 29 physician specialists. Analysts controlled for age in the main results and further controlled for sex, self-reported health status, health insurance, education level, and income in a second specification. Though it was reported that the conclusions were similar, the lack of a clear disparity definition or a conceptual framework made it difficult to interpret each specification.

On the contrary, a well-specified question and framework can lead to meaningful interpretation of study results that go beyond the IOM approach to defining disparities. For example, a recent study presents within-hospital disparities in patient safety between Black and white patients (Gangopadhyaya 2021). The study clearly acknowledges that there are unjust differences in patient safety driven by differences in the quality of hospitals that disproportionately serve Black versus white patients that are not measured in the analysis, but it further motivates the need to understand differences within hospitals to assess whether resolving quality differences across hospitals will be sufficient to eliminate disparities. It includes a strong conceptual framework that details many of the unjust drivers of within-hospital differences, which include implicit and explicit bias and a lack of racial concordance between patients and providers. The model specified to isolate these differences estimates the average within-hospital difference in patient safety after controlling for differences in age and sex. The results therefore reflect the difference due to all the unfair drivers specified in the conceptual framework but cannot isolate the individual drivers. The study is forthcoming about this limitation, however, and provides the necessary information to interpret the results.

A strong conceptual framework can clarify analytic choices, contextualize research findings, incorporate the effects of structural racism and other upstream causal factors, and thereby prevent potential misinterpretation of results. In addition, a strong conceptual framework is necessary for moving beyond documenting disparities to explaining them and identifying solutions. This facilitates a more useful understanding of the specific causes and potential remedies of the disparities.

## 3. Discuss the Data Limitations in Implementing a Definition of Disparity, Including the Measurement of Race and Ethnicity

Limitations in the measurement of key variables used to estimate health care disparities should be acknowledged and considered. This includes limitations in the measurement and completeness of data showing the subgroups of interest (e.g., race and ethnicity) and important covariates (e.g., sex, gender, health status, and health care preferences). Studies can begin by noting any issues with data quality, validity, and completeness, including how the study data compare with data collected according to an ideal or best practice. For example, studies using administrative race and ethnicity data can acknowledge limitations relative to self-reporting, which is the gold standard for the collection of race and ethnicity data recognized by both the Office of Management and Budget and the US Department of

Health and Human Services (ASPE 2011).[4] Data completeness is a concern, as item nonresponse with respect to race and ethnicity can reflect how the questions were asked and can limit researchers' ability to disaggregate groups to a degree appropriate for their study goals. Recent scholarship on the heterogeneity within the aggregate Asian or Asian American category reveals important differences that could provide motivation for disaggregating groups as much as the data can support when examining health-related measures (Kanaya et al. 2022).

Beyond this, researchers might also acknowledge that multiple dimensions of race and ethnicity are not captured in traditional measures, including skin color, "street race" (López et al. 2018), racial identity centrality (Perry et al. 2016), tribal membership, ethnic ancestry, caste, country of birth, and language. To the extent that these and other unmeasured dimensions of key variables are associated with health care disparities, researchers should consider how these data limitations may affect their disparity estimates and interpretation (Bauer et al. 2021).

Additional limitations in measurement of key covariates should also be noted. For example, the IOM recommends controlling for health status, but available measures are often limited and vary across data sources; thus, each study may face different challenges in computing the same definition of disparities (with such consequences as making comparable definitions across studies difficult). Moreover, the measurement of health status may not reflect actual health status, as groups with more limited access to health care may have systematically underdiagnosed health conditions, which would introduce bias into estimates controlling only for diagnosed health problems. Several studies have documented race and gender disparities in the diagnosis of existing health conditions (Mamary et al. 2018; Seyyed-Kalantari et al. 2021; Singh et al. 2014).

Clearly acknowledging any limitations in the ability to measure desired subgroups or covariates in available data and discussing the implications (such as potential bias or masking of heterogeneity of interest) allows for meaningful interpretation of disparity estimates and provides guidance for future research and data collection.

## 4. Estimate a Model with a Comprehensive Set of Covariates Consistent with the Conceptual Framework and Use the Appropriate Components of the Model to Calculate the Magnitude of the Disparity

The magnitude of disparities under a given definition may be estimated in multiple ways, with important implications for interpretation. Consider an analysis that seeks to estimate the IOM disparity in health care access between non-Hispanic Black and non-Hispanic white adults. The IOM defines the disparity as the difference between groups not due to differences in clinical need or patient preferences. A corresponding conceptual framework might include age, sex, and health status as measures of clinical need and insurance coverage and other socioeconomic status measures as other drivers of access. The conceptual framework might also acknowledge potentially measurable factors that are not available (e.g., patient preferences) or factors that are not directly measurable and expected to be captured by residual differences (e.g., unequal treatment by the health care system).

The disparity of interest may be estimated with a linear regression model that adjusts for a limited set of covariates including only age, sex, and health status, along with an indicator for race. This approach uses a residual direct effect estimation method to estimate a disparity under the IOM disparity definition, acknowledging the limitation that measures of patient preference are not available. The coefficient on the race indicator would measure the disparity (box 1).

BOX 1

**Single Regression Model with Limited Covariates (Residual Direct Effect)**

$$Y = \beta_1 White + \beta_2 AgeSexHealth + \varepsilon$$

$$Total\ Difference = \bar{Y}_{white} - \bar{Y}_{Black}$$

$$Difference\ due\ to\ AgeSexHealth = \beta_2(\overline{AgeSexHealth}_{white} - \overline{AgeSexHealth}_{Black})$$

$$IOM\ Disparity = Total\ Difference - Difference\ due\ to\ AgeSexHealth$$

$$= Unexplained\ difference = \beta_1$$

Alternatively, a regression could be estimated with a more complete set of covariates that maps characteristics from the framework to measures available in the data source, such as education, income, marital status, insurance status, and other socioeconomic variables (e.g., measures of structural inequity at the community level such as residential segregation, neighborhood violence, and exposure to pollution and toxins). The IOM disparity in this case would be computed by subtracting the difference due to age, sex, and health status from the total, or by adding components explained by mean differences in the included socioeconomic variables (health insurance coverage and socioeconomic status variables) to the residual component measured by the coefficient on the indicator of race and ethnicity (box 2).

BOX 2

**Single Regression Model with Full Set of Covariates**

$$Y = \beta_1 White + \beta_2 AgeSexHealth + \beta_3 Coverage + \beta_4 SES + \varepsilon$$

$$Total\ Difference = \bar{Y}_{white} - \bar{Y}_{Black}$$

$$Difference\ due\ to\ AgeSexHealth = \beta_2(\overline{AgeSexHealth}_{white} - \overline{AgeSexHealth}_{Black})$$

$$Difference\ due\ to\ Coverage = \beta_3(\overline{Coverage}_{white} - \overline{Coverage}_{Black})$$

$$Difference\ due\ to\ SES = \beta_4(\overline{SES}_{white} - \overline{SES}_{Black})$$

$$Unexplained\ Difference = \beta_1$$

$$\begin{aligned} IOM\ Disparity &= Total\ Difference - Difference\ due\ to\ AgeSexHealth \\ &= Difference\ due\ to\ Coverage + Difference\ due\ to\ SES + Unexplained\ Difference \end{aligned}$$

These two estimation methods produce different estimates of the same disparity concept. The first method, which does not include all factors specified by the conceptual framework, suffers from omitted variable bias relative to the second method, which uses the more comprehensive set of explanatory variables (if we acknowledge the likely role of the additional variables in affecting the outcome). Specifically, the limited covariate regression model in the first approach subtracts out the independent effect of age, sex, and health status as intended, but it also subtracts out the effect of socioeconomic variables correlated with the included factors. That is, because age, sex, and health status are correlated with the socioeconomic measures, the model does, in fact, adjust for these socioeconomic variables to the extent they are correlated with age, sex, and health status. As a result, the first model incorrectly assigns some portion of unjust differences in an outcome to differences in age, sex, or health status.

In contrast, by controlling for socioeconomic factors explicitly, the full regression approach subtracts out the portion of the effects of age, sex, and health status that is uncorrelated with socioeconomic status or other included factors. The full model approach to estimating disparities also has the benefit of allowing a more complete understanding of the factors associated with the outcome of interest and their relative importance.

In addition, using the full model approach, with the effects of adjustment factors shown separately, can facilitate computation of alternative disparity measures. Appropriate measures of disparities may vary based on the research question, the outcome under study, and the context, thus providing the information necessary to compute disparity measures under alternative definitions from the model results and summary statistics that can be useful for wider applicability and use. We therefore recommend that studies provide the full regression output and sample means that would be needed to compute the magnitude of disparity using different definitions; that is, studies should report coefficients and group means for age, sex, health status, and other adjustment variables separately so that the IOM disparity can be computed.

## 5. Investigate Models That Move Beyond Documenting Disparities to Analyze Contributing Factors

Research efforts have often focused on documenting health disparities but given less attention to how to eliminate them. To reduce disparities, the mechanisms generating them need to be better understood. It is particularly useful to identify factors amenable to modification by public policy or other practical action.

Since the IOM report in 2003, the emphasis in research on health and health care disparities has shifted toward identifying the structural drivers of disparities. In a linear regression framework, an accounting can be performed by reporting how group differences in each explanatory variable or set of variables contribute to the disparity and what portion remains unexplained by the model. A useful application of the regression approach is the Kitagawa-Blinder-Oaxaca decomposition (Kitagawa 1955; Blinder 1973; Oaxaca 1973) and nonlinear variants of this long-standing method (Fairlie 2005). In these approaches, separate regression models are estimated by racial and ethnic group, and the mean group difference in the outcome measure is decomposed into parts attributable to differences in explanatory variable means (evaluated at the chosen group's coefficients) and differences in coefficients (evaluated at the chosen group's explanatory variable means; box 3). Though subject to the usual caveats of descriptive cross-sectional regression models with respect to identifying associations and not causal effects, such an approach can provide preliminary estimates of the reduction in the disparity that could be achieved if various explanatory characteristics were equalized across groups (e.g., income and insurance coverage). In this example, we use the coefficients for the Black population to evaluate the change in the outcome that could be achieved if the differences in the explanatory variables were eliminated. This approach is most common when decomposing differences where one group is assumed to be disadvantaged (Biener and Zuvekas 2019; Hargraves and Hadley 2003; Kirby et al. 2006; McMorrow et al. 2014) and effectively measures how the disadvantaged group would respond to a change in a particular characteristic (e.g., income or insurance coverage) based on their past response to such changes.

BOX 3

**Separate Group-Specific Regression Models with Full Sets of Covariates**

$$Y_{Black} = \beta_{2Black} AgeSexHealth + \beta_{3Black} Coverage + \beta_{4Black} SES + \varepsilon$$
$$Y_{white} = \beta_{2white} AgeSexHealth + \beta_{3white} Coverage + \beta_{4white} SES + \varepsilon$$

$$Total\ Difference = \bar{Y}_{white} - \bar{Y}_{Black}$$

$$Difference\ due\ to\ AgeSexHealth = \beta_{2Black}(\overline{AgeSexHealth}_{white} - \overline{AgeSexHealth}_{Black})$$

$$Difference\ due\ to\ Coverage = \beta_{3Black}(\overline{Coverage}_{white} - \overline{Coverage}_{Black})$$

$$Difference\ due\ to\ SES = \beta_{4Black}\ (\overline{SES}_{white} - \overline{SES}_{Black})$$

$$\begin{aligned} &Difference\ due\ to\ Coefficients \\ &\qquad = \overline{AgeSexHealth}_{Black}(\beta_{2white} - \beta_{2Black}) + \overline{Coverage}_{Black}(\beta_{3white} - \beta_{3Black}) \\ &\qquad + \overline{SES}_{Black}(\beta_{4white} - \beta_{4Black}) \end{aligned}$$

$$\begin{aligned} &Difference\ due\ to\ Interaction\ between\ Coefficents\ and\ Covariates \\ &\qquad = \left((\overline{AgeSexHealth}_{white} - \overline{AgeSexHealth}_{Black})(\beta_{2white} - \beta_{2Black})\right) \\ &\qquad + \left((\overline{Coverage}_{white} - \overline{Coverage}_{Black})(\beta_{3white} - \beta_{3Black})\right) \\ &\qquad + \left((\overline{SES}_{white} - \overline{SES}_{Black})(\beta_{4white} - \beta_{4Black})\right) \end{aligned}$$

$$
\begin{aligned}
\mathit{Unexplained\ Difference} &\\
&= \mathit{Difference\ due\ to\ Coefficents}\\
&+ \mathit{Difference\ due\ to\ Interaction\ between\ Coefficients\ and\ Covariates}
\end{aligned}
$$

$$
\begin{aligned}
\mathit{IOM\ Disparity} &= \mathit{Total\ Difference} - \mathit{Difference\ due\ to\ AgeSexHealth}\\
&= \mathit{Difference\ due\ to\ Coverage} + \mathit{Difference\ due\ to\ SES} + \mathit{Unexplained\ Difference}
\end{aligned}
$$

# Evaluating Disparities Research in Practice

In this section, we highlight two studies that align with many of our recommendations: "Explaining Racial and Ethnic Disparities in Health Care" (Kirby, Taliaferro, and Zuvekas 2006) and "Assessing the Individual, Neighborhood, and Policy Predictors of Disparities in Mental Health Care" (Cook et al. 2017). By using the Kitagawa-Blinder-Oaxaca decomposition method to explain how group differences in a wide range of observable characteristics contribute to differences in health care access and use between groups, both papers take a critical step toward analyzing the drivers of disparities and considering policy solutions.

The first paper examines differences in three health care access measures across seven racial and ethnic groups, where total differences were decomposed into parts attributable to group differences in insurance status, sociodemographic variables, language, attitudes about risk and health care, neighborhood racial and ethnic composition, neighborhood socioeconomic status, health system capacity, and factors not attributable to group differences in observable characteristics (Kirby, Taliaferro, and Zuvekas 2006). Using data from the Medical Expenditure Panel Survey, the study found a 6.1 percentage-point difference in the share with a usual source of care, with white adults reporting better access than Black adults. Neighborhood racial and ethnic composition variables explain 41 percent of the difference, insurance coverage explains 34 percent, and sociodemographic characteristics explain 21 percent. Neighborhood socioeconomic status explains a smaller portion of the gap, while health system capacity variables and patients' health attitudes and preferences and languages explain even less and have negative contributions, implying that removing these differences widens the disparity. Only 2 percent of the total difference is unexplained by the model. These findings suggest that effective policy responses to residential segregation present an important opportunity for eliminating disparities in health care access for the Black population.[5]

Despite the inclusion of a full set of covariates and efforts to decompose drivers of disparities and draw policy conclusions, this study does not provide a clear definition of disparity or a conceptual framework that outlines how the covariates were chosen and which factors were considered just or unjust drivers of disparities, although it does acknowledge data limitations. In addition, as with most studies in this area, the study does not provide the full underlying regression output that would be needed to compute an alternative disparity definition. For example, we could not compute the IOM disparity from these estimates because the study included measures beyond age and sex in the sociodemographic category.

The second paper examines differences in initiation and length of mental health care across three racial and ethnic groups; total differences are decomposed into parts attributable to individual-level factors (e.g., age, sex, health needs, education, income, marital status, and region), neighborhood and county factors (e.g., at the census-block level, the percentage of residents who are college graduates, Black, Latino, or unemployed; and at the county level, the density of general health care providers and mental health specialists), state-level factors (e.g., differences in Medicaid eligibility), and unexplained differences (Cook et al. 2017). The study employs two conceptual frameworks to guide the different levels of factors—individual, contextual (e.g., health care provider supply), and compositional (e.g., neighborhood segregation)—in the regression models. Relative to Black people, white people were 16 percentage points more likely to initiate mental health care among those with probable mental illness. Differences in observable characteristics explain 31 percent of the total difference. Differences in individual-level characteristics account for 59 percent of the explained difference, while community characteristics contribute 36 percent and state factors contribute 6 percent. In particular, the study found that the differences in the supply of mental health specialists in an area were not a significant driver of disparities in mental health care initiation. Thus, the authors conclude that interventions to increase provider supply are unlikely to reduce disparities, although the inclusion of other neighborhood and county factors may affect this interpretation. The authors do not explicitly discuss decomposing the total differences reported into those driven by just differences (e.g., age, gender, and health needs) and unjust differences (e.g., education and income), but they do provide sufficient information to compute the IOM disparity or alternative definitions.

Both studies reflect strong efforts to measure and interpret disparities in health care access and use in ways that can inform policy efforts to reduce disparities, but the comprehensive conceptual framework and provision of analytic details make the second paper a particularly good example of our principles in action.

# Illustrative Empirical Examples

To further illustrate some of these points, we use data from the NHIS to estimate disparities between Black and white adults in several measures of health care access and use. The NHIS is the primary source of nationally representative data on the nation's health, and we use publicly available data from 2016–18 accessed through IPUMS Health Surveys.[6] We limit our analysis to nonelderly adults ages 19 to 64 and use self-reported race and ethnicity data to identify non-Hispanic Black and non-Hispanic white individuals (hereafter race and ethnicity are referred to as Black and white). We analyze five measures of health care access and use based on respondents' reports of having the following: a usual source of care, any visit to a medical provider in the past 12 months, any visit to a specialist physician in the past 12 months, receipt of a flu vaccine in the past 12 months, and a blood pressure check in the past 12 months.

We present analyses in this section to illustrate how disparity estimates differ across definitions and estimation methods. We do not aim to put forth a complete disparity analysis that would incorporate all of the recommendations we have discussed; rather, we aim to highlight the importance of these choices. First, we show how the magnitude of a disparity estimate can vary across definitions. Then, using the IOM disparity definition, we show how the magnitude of the disparity estimate varies with different estimation methods. Finally, we decompose the IOM disparity estimate into its component parts and consider variation across estimation methods and implications for interpretation. Our findings include the following:

- Disparity estimates can vary widely when using different disparity definitions. Clearly defining the disparity with a supporting conceptual framework—rather than defining it implicitly based on the included covariates in a regression model—is critical for interpretation. Whether and how to adjust for health status when estimating disparities in health care access and use is a complex decision that should be guided by a conceptual framework with careful attention to how different health status measures reflect need versus access to care. *When a study presents multiple disparity estimates with different sets of covariates (which is currently a common approach), the study is implicitly using multiple disparity definitions.*
- Estimates of disparities using the IOM definition and their component parts vary modestly across estimation methods. Using a residual direct effect method may understate the magnitude of the IOM-defined disparity by adjusting for unjust drivers of the disparity correlated with age, sex, and health, compared with a full model method (either using a pooled model or separate models by racial group) that adjusts for only the portion of differences due to

age, sex, and health uncorrelated with measures such as socioeconomic status or insurance coverage.

- If the data demonstrate a differential effect of socioeconomic status or other factors on outcomes by racial group, estimates of the disparity and the drivers of disparity may be improved by estimating separate models by race.

## Comparing the Magnitude of Disparity Estimates Using Different Disparity Definitions

We begin by estimating the total difference in outcomes between Black and white adults for each measure (table 1). For all access measures, unadjusted rates for white adults are higher than for Black adults, indicating better access, though disparities in any provider visit and blood pressure check are not statistically different at the 5 percent level. We then present four disparity estimates using the residual direct effect approach that sequentially add categories of covariates. These estimates are calculated by estimating linear probability models and interpreting the coefficient on a binary indicator for white race as the disparity estimate. As discussed above, such estimates implicitly define the disparity as the difference in the outcome not due to differences in the included covariates. Without further guidance, this is often interpreted as the difference considered to be unfair.

The first model adjusts only for age groups and a binary indicator for sex (woman or man), and the disparity between white and Black adults is smaller across all measures, likely reflecting that white adults are older, on average, than Black adults in the sample (see appendix table A.1). Statistical significance was set at a $p$-value < 0.05.

TABLE 1

**Disparities in Health Care Access and Use under Different Definitions Estimated as Residual Direct Effects**

| Access measure | White mean | Black mean | Total difference | Disparity not due to age and sex (model 1) | Disparity not due to age, sex, and health (model 2) | Disparity not due to age, sex, health, and insurance (model 3) | Disparity not due to age, sex, health, insurance, and SES (model 4) |
|---|---|---|---|---|---|---|---|
| Usual source of care | 0.860 | 0.825 | 0.035* | 0.028* | 0.034* | 0.016* | -0.008 |
| Any provider visit (past 12 months) | 0.849 | 0.837 | 0.012 | 0.010 | 0.018* | 0.003 | -0.013 |
| Specialist visit (past 12 months) | 0.279 | 0.190 | 0.089* | 0.077* | 0.082* | 0.072* | 0.046* |
| Flu vaccine (past 12 months) | 0.393 | 0.305 | 0.088* | 0.079* | 0.085* | 0.070* | 0.044* |
| Blood pressure check (past 12 months) | 0.856 | 0.845 | 0.011 | 0.007 | 0.013 | -0.003 | -0.020* |
| **Sample size range** | **42,762–42,857 (variation due to missing values in the outcome variable)** | | | | | | |

**Source:** Authors' analysis of the 2016–18 National Health Interview Survey.

**Notes:** SES = socioeconomic status. Analysis sample includes non-Hispanic Black and non-Hispanic white adults ages 19 to 64 who are not missing values for any of the full range of covariates. Usual source of care is measured at the time of the survey. Health is health status measures; insurance is health insurance types including uninsured. The disparity estimates in model 2 that control for age, sex, and health status represent Institute of Medicine disparities, under the caveat that measures of patient preferences would also have been used as controls if they were available. Model 3 adds adjustments for health insurance, and model 4 adds adjustments for education; employment; home ownership; income relative to poverty; receipt of Supplemental Security Income, welfare, or Supplemental Nutrition Assistance Program assistance; marital status; census region; and citizenship. See text for more details. Descriptive statistics for the variables included in each of the adjustment categories are shown in appendix table A.1.

* *p*-value < 0.05 on a two-tailed test.

After adjusting for health status (including self-reported general health status), psychological distress, obesity, smoking status, and several diagnosed chronic conditions, however, the disparity between Black and white adults is generally larger than when adjusting for age and sex only (model 2 compared with model 1). Black adults are more likely to be in fair or poor health, to be obese, and to be diagnosed with hypertension, diabetes, and stroke than white adults (appendix table A.1). Because these health measures are positively associated with many measures of health care access and use, adding controls for health status results in larger disparity estimates. However, different health measures have somewhat different effects on the disparities estimates. The disparity estimates are larger if adjusted only for self-reported health status measures (general health status, psychological distress, obesity, and smoking status) than if also adjusted for diagnosed conditions (appendix tables A.2 through A.6). For example, adjusting for age, sex, and self-reported health status only, the disparity in receipt of a specialist visit is 9.7 percentage points, compared with 8.2 percentage points when also adjusting for diagnosed conditions (appendix table A.4). This likely owes to the fact that diagnosed conditions reflect both health needs and access to care, with white adults exhibiting higher rates of many diagnosed conditions (appendix table A.1). A growing body of literature explores disparities by race and gender in underdiagnosis, as discussed previously, which can be used to help interpret the effects of adjusting for diagnosed conditions (Mamary et al. 2018; Seyyed-Kalantari et al. 2021; Singh et al. 2014).

The final two models include further adjustments: adjustments for insurance coverage (Medicare, Medicaid/the Children Health Insurance Program, Marketplace, direct purchase, other public, other private, and uninsured) in model 3 and other demographic and socioeconomic characteristics (education; employment; homeownership; income relative to poverty; receipt of Supplemental Security Income, welfare, or Supplemental Nutrition Assistance Program assistance; marital status; census region; and citizenship) in model 4. After adding adjustments for insurance coverage (model 3), disparities narrow for all measures and are no longer statistically significant for measures of any provider visit in the past 12 months. This reflects higher rates of Medicaid coverage and uninsurance among Black adults and lower health care access and use among those with Medicaid and no coverage. Further controlling for a wide range of demographic and socioeconomic characteristics reduces estimated disparities for all measures and results in two measures that are not statistically significant and one measure in which the disparity is significant but reverses direction (i.e., indicates worse access for white adults).

This exercise reinforces the importance of recommendations 1, 2, and 3 above. First, it demonstrates the importance of **explicitly defining the disparity of interest** rather than doing so implicitly and leaving the interpretation to the reader. When a study presents multiple disparity estimates using a residual direct effect approach and different sets of covariates and does not otherwise define disparity, the study is implicitly using multiple disparity definitions. As shown, the selected covariates can substantially change the estimated disparity across these definitions. Thus, without appropriate context, it can be challenging to interpret multiple disparity estimates in a single analysis, as was the case in the Medicaid enrollee study (Wallace et al. 2022).

Second, this exercise supports the value of **providing a comprehensive conceptual framework**. For example, a study may present a single implied disparity definition, such as that from model 4. This implied definition excludes differences in outcomes due to differences in insurance coverage and income and thereby minimizes the role of structural factors that limit both economic opportunity and health care access for the Black population. In this case, without an explicit definition or conceptual framework, the results might be misinterpreted as indicating only modest disparities in access and use between Black and white adults when the estimates from other definitions are considerably larger. However, the results might be more clearly interpreted if a study presented these estimates, acknowledged the role of structural factors in driving disparities in the conceptual framework, and explained that the disparity of interest excluded the differences due to insurance coverage and socioeconomic status to assess the disparity that might be addressed through interventions other than coverage or income supports.

This exercise also further supports the value of using a conceptual framework by illustrating the complexity of deciding whether and how to adjust for health status. Differences in health care use due to differences in health needs are probably reasonable and fair and thus should be excluded from an estimated disparity, but differences in health care use due to differences in health care access are likely unfair and should be included in a disparity estimate. Considering these issues when developing a conceptual framework would help guide which health status measures to include when some measures reflect both health need and health care access.

Finally, the exercise reveals the importance of **acknowledging data limitations.** Each group of added covariates aims to capture a specific concept, but data limitations exist within each group. For example, models 1 and 2 add measures of age, sex, and health status to approximate the estimation of the IOM disparity definition. However, no measures of patient preferences are included, which may be acknowledged as a limitation. Similarly, the added indicators for insurance coverage are only able to capture the presence and source of insurance coverage but not the quality of that coverage. Finally, the measures of socioeconomic status do not include measures of wealth, which may have important implications for the results. These types of data limitations can have meaningful impacts on disparity estimates, so it is critically important for researchers to discuss any limitations and the potential implications for their results.

## Comparing Different Methods of Estimating the IOM Disparity

Once the preferred disparity definition has been selected, the disparity can be estimated in multiple ways. For the analysis in this section, we use the IOM disparity definition as the disparity of interest to compare estimation approaches. As a reminder, the IOM disparity is implemented here as the difference in outcomes not due to differences in age, sex, or health status. A more complete implementation of the IOM definition would also adjust for patient preferences, but these are difficult to measure, and such measures are not available in our data.

#### IOM DISPARITY = TOTAL DIFFERENCE – DIFFERENCE DUE TO AGE, SEX, AND HEALTH

We examine three methods for estimating the IOM disparity: (1) a single regression model with limited covariates, that is, a residual direct effect model (box 1); (2) a single regression model with a full set of covariates (box 2); or (3) separate group-specific regression models with full sets of covariates (box 3). All models are estimated on a sample of only Black and white adults. The residual direct effect method uses a linear probability model that adjusts for only age, sex, and health status and interprets the coefficient on the white race indicator as the IOM disparity. The single full regression method includes a full set of covariates including age, sex, health status, health insurance coverage, socioeconomic status variables, and a binary indicator variable for race and computes the disparity as described in box 2. The separate group-specific models with a full regression method uses Kitagawa-Blinder-Oaxaca decomposition and estimate separate models for nonelderly Black and white adults. The disparity due to differences in specific characteristics is constructed as described in box 3, using the coefficients from the model estimated using the sample of Black adults. A disparity estimate could also be constructed using the coefficients from the model estimated using the sample of white adults, but most studies using decomposition methods to analyze disparities use the coefficients of the more disadvantaged population. The resulting disparity estimates are commonly interpreted as the expected change in the outcome if the Black population had the same characteristics as the white population but had the Black population's responses to (i.e., regression coefficients for) those characteristics.[7]

Table 2 shows the IOM disparity estimates using each of the above estimation approaches. The IOM disparity estimate from the single full regression (method 2) is slightly larger than the estimate using the residual direct effect (method 1) for each outcome. Though these alternate estimation methods do not lead to substantially different conclusions in our examples, the exercise does demonstrate that using the residual direct effect method typically results in IOM disparity estimates smaller in magnitude than those from the full regression methods. This difference likely reflects a reduction in omitted variable bias when using the full regression method. Using the residual direct effect method may result in a biased estimate because it effectively adjusts for the socioeconomic correlates of age, sex, and health not included in the model. For example, because older age among nonelderly adults is correlated with higher income, adjusting only for age and sex will also capture some of the effects of having higher income.

TABLE 2

**IOM Disparity Using Different Estimation Methods**

| Outcome measure | IOM disparity using single regression model with age, sex, and health covariates (method 1) | IOM disparity using single regression model with full set of covariates (method 2) | IOM disparity using separate group-specific regression models with full set of covariates (method 3) |
|---|---|---|---|
| Usual source of care | 0.034* | 0.037* | 0.031* |
| Any provider visit (past 12 months) | 0.018* | 0.021* | 0.024* |
| Specialist (past 12 months) | 0.082* | 0.085* | 0.091* |
| Flu vaccine (past 12 months) | 0.085* | 0.089* | 0.087* |
| Blood pressure check (past 12 months) | 0.013 | 0.017* | 0.019* |
| **Sample size range** | **42,762–42,857** | **42,762–42,857** | **White: 36,607–36,696**<br>**Black: 6,155–6,161** |

**Source:** Authors' analysis of the 2016–18 National Health Interview Survey.

**Notes:** IOM = Institute of Medicine. Analysis sample includes non-Hispanic Black and non-Hispanic white adults ages 19 to 64 who are not missing values for any of the covariates included in the full model. Usual source of care is measured at the time of the survey. Method 1 includes health status (including self-reported general health status), psychological distress, obesity, smoking status, and several diagnosed chronic conditions, and estimates the disparity as the residual direct effect (box 1). Method 2 adds adjustments for health insurance coverage and socioeconomic status, including education; employment; homeownership; income relative to poverty; receipt of Supplemental Security Income, welfare, or Supplemental Nutrition Assistance Program assistance; marital status; census region; and citizenship, and estimates the disparity as in box 2. Method 3 applies the Kitagawa-Blinder-Oaxaca decomposition method. It uses the coefficients from a model using the full set of covariates estimated on the sample of Black adults to compute disparity estimates, and estimates the disparity as in box 3.

* *p*-value < 0.05 on a two-tailed test.

Though the IOM disparity estimates from the separate group-specific full regression models (method 3) are broadly similar to those in method 2, there are small variations with some important implications for choosing a method. For example, the IOM disparity estimate for usual source of care is smaller (3.1 percentage points) when using separate models than when using a single regression model (3.7 percentage points). For specialist care, however, using separate models produces a larger IOM disparity estimate (9.1 versus 8.5 percentage points). In both cases, the variation is driven by differences between Black and white adults in the relationships between health status and health care access and use (appendix table A.7). For usual source of care, there is a strong negative relationship between fair or poor health and health care access among Black adults ($\beta$ = –.048) but not white adults ($\beta$ = .001). Because Black adults are more likely to be in fair or poor health than white adults, using separate models thereby attributes more of the gap in having a usual source of care to differences in health status and reduces the IOM disparity estimate. For specialist visits, there is a stronger positive relationship among Black adults than among white adults between several chronic conditions and health care use, including diabetes ($\beta_{black}$ = .129, $\beta_{white}$ = .061) and stroke ($\beta_{black}$ = .102, $\beta_{white}$ = .021). Because these conditions are more common among Black adults, using separate models attributes less of the gap in specialist visits to health status and increases the IOM disparity estimate.

The estimates presented in table 2 show far less variation across estimation methods for a single disparity definition in our examples than across definitions for a single estimation method (table 1). Given its potential to reduce omitted variable bias, however, this exercise supports our **preference for a full regression method with a comprehensive set of covariates rather than a residual direct effect method,** as noted in recommendation 4. When considering pooled versus separate models by race for the full regression approach, we recommend always considering the use of separate models by group and then stating why the simpler model or the more detailed model might be preferred. When sample sizes are sufficient to not present major concerns for estimating full models for smaller groups and there is evidence of *meaningful* (not merely statistically significant) differences in corresponding coefficient values across groups, we recommend using separate models and basing disparity estimates on the model using the coefficients of the more disadvantaged population.

## Estimating the Contribution of Socioeconomic Status and Other Factors to the IOM Disparity

In addition to reducing omitted variable bias, a strength of estimating a full model is that it allows estimation of the role of socioeconomic disadvantage or other factors in the disparity and estimation of the potential reduction in the disparity if the group differences in such factors are reduced or eliminated. In table 3, we decompose the total difference into differences due to age, sex, and health status; insurance coverage, socioeconomic status, and other demographic characteristics; and factors unexplained by the variables in the regression models. Each component is calculated as described in box 3, and the IOM disparity is then calculated as follows:

*IOM disparity = total difference – difference due to age, sex, and health*

or, equivalently,

*IOM disparity = difference due to coverage + difference due to socioeconomic status + unexplained difference*

TABLE 3

**Decomposition of IOM Disparity Estimates**

| Outcome measure | Total difference | Difference due to age, sex, and health | IOM disparity | Difference due to insurance coverage | Difference due to SES | Unexplained difference |
|---|---|---|---|---|---|---|
| Usual source of care | 0.035* | 0.005 | 0.031* | 0.011* | 0.029* | -0.010 |
| Any provider visit (past 12 months) | 0.012 | -0.012* | 0.024* | 0.008* | 0.027* | -0.011 |
| Specialist visit (past 12 months) | 0.089* | -0.002 | 0.091* | 0.002 | 0.026* | 0.062* |
| Flu vaccine (past 12 months) | 0.088* | 0.001 | 0.087* | 0.005 | 0.023* | 0.058* |
| Blood pressure check (past 12 months) | 0.011 | -0.007 | 0.019* | 0.012* | 0.042* | -0.035* |

**Source:** Authors' analysis of the 2016–18 National Health Interview Survey.

**Notes:** IOM = Institute of Medicine. SES = socioeconomic status. Analysis sample includes non-Hispanic Black and non-Hispanic white adults ages 19 to 64. Health is health status. Insurance is health insurance types including uninsured. SES is education; employment; homeownership; income relative to poverty; receipt of Supplemental Security Income, welfare, or Supplemental Nutrition Assistance Program assistance; marital status; census region; and citizenship. The IOM disparity estimates reported and decomposed here are those reported in table 2 using method 3. These estimates are produced using separate, race-specific full regression models, and all estimates use the coefficients from the regression model estimated on the sample of Black adults to compute disparity estimates. The IOM disparity may differ from the sum of the reported components because of rounding.

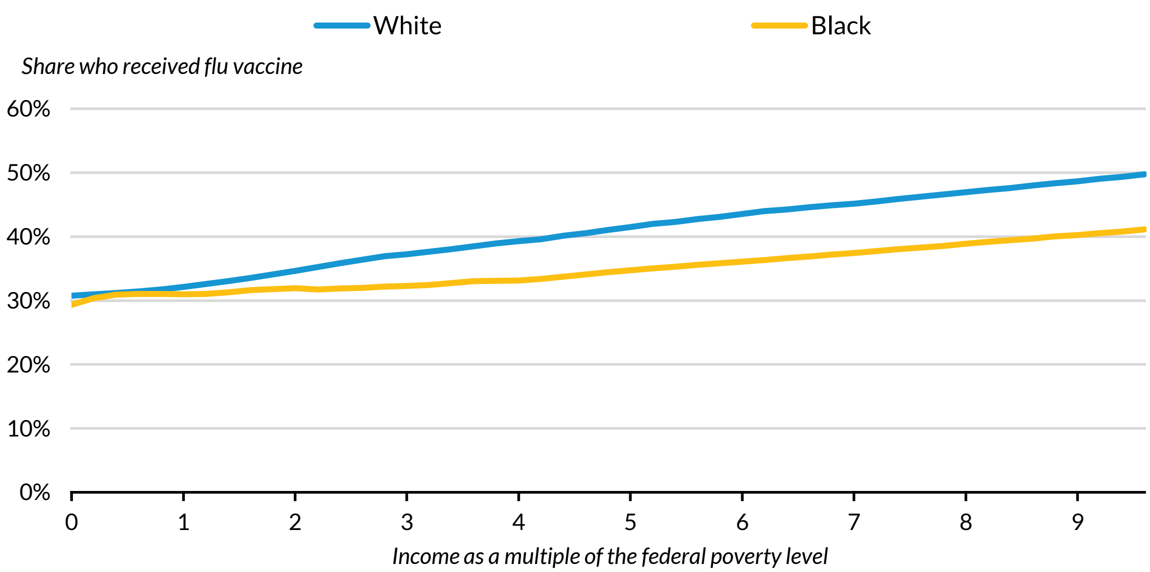
* *p*-value < 0.05 on a two-tailed test.

For every measure assessed in this study, the results suggest that replacing the means for insurance coverage, socioeconomic status, and other demographic characteristics for Black adults with the means for white adults would have a positive impact on the estimates of health care access and use for Black adults. The part of the total disparity explained by differences in socioeconomic status and other characteristics is larger than that explained by differences in insurance coverage for all outcomes. For specialist visits and flu vaccine receipt, the part of the disparity unexplained by differences in any of the variables in the model is even larger than that explained by socioeconomic status and other characteristics. This unexplained portion of the disparity captures differences in the outcome of interest due to any unmeasured factors, including personal preferences, and captures differences due to individual, institutional, and structural racism not already reflected through their impacts on insurance coverage and socioeconomic status.

Using pooled versus separate models also has implications for estimates of the part of the disparity explained by socioeconomic status and other factors. For example, while the IOM disparity estimate for flu vaccine receipt (table 2) is quite similar regardless of model choice (8.9 percentage points using a single model versus 8.7 percentage points using separate models), the estimated effects of equalizing socioeconomic status vary more (appendix table A.8). For flu vaccines, the underlying models indicate that increasing income relative to poverty has a much stronger effect on vaccine receipt for white adults than for Black adults (appendix table A.7 and figure 1). So, using the model estimated on the sample of Black adults, the estimated effects of equalizing socioeconomic status characteristics are smaller: a reduction in the disparity of 2.3 percentage points, compared with 3.3 percentage points using a pooled model (appendix table A.8). This finding is consistent with literature suggesting weaker effects of socioeconomic status in generating positive health outcomes among Black populations compared with white populations (Assari 2020; Bell et al. 2020; Ciciurkaite 2021). This provides further evidence to support the use of separate models by group if the data demonstrate differential effects of socioeconomic status or other factors on outcomes by racial group.

FIGURE 1

**Estimated Relationship between Income Relative to Poverty and Flu Vaccine Receipt among Nonelderly Adults, by Race**




**Source:** Authors' analysis of the 2016–18 National Health Interview Survey.
**Notes:** The relationship between income as a multiple of the poverty rate and the share that received flu vaccine are estimated for each group using locally weighted regression (lowess command in Stata using default options).

This exercise reinforces recommendation 5. Though establishing the magnitude of a specific disparity is important, **analyzing the factors that contribute to the disparity** provides more information that can ultimately be used to reduce disparities. For example, there have been considerable efforts in recent years to improve the rate of health insurance coverage for all populations. However, Black adults remain less likely to be insured than white adults, and this likely contributes to lower rates of health care access and utilization for Black adults. But by decomposing the disparities in health care access and use, we can see that differences in socioeconomic status and unmeasured factors that may include systemic and structural barriers often explain a much larger share of the disparity. Thus, while increasing health insurance coverage is still a worthy goal, it will not be sufficient to eliminate disparities in health care access and use.

# Conclusion

Since the IOM's landmark 2003 report proposing a clear definition of disparity in health care access and use for evaluating health care system performance, numerous studies have built on that foundation, creating various applications and approaches, some of which we have described and modeled here. Yet as the field moves slowly toward a standard set of approaches, many studies of health care disparities continue to present estimates that are ill defined and subject to misinterpretation.

From our assessment of the literature to date, we offer five recommendations to improve the quality, interpretation, and applicability of estimates of racial disparities. First, we discuss the importance of having a clear definition of disparity appropriate for the research question and a corresponding method to estimate the disparity of interest. Second, we describe the importance of choosing a comprehensive conceptual framework that details the factors driving disparities, as the framework identifies which parts of estimated group differences are deemed just versus unjust. Third, we provide rationale for describing the strengths and weaknesses of the key available data to identify population groups and analyze the drivers of differences and disparities. Fourth, we further advocate for researchers to choose an analytic approach that incorporates a comprehensive set of covariates consistent with the chosen conceptual framework. With this, researchers can estimate a disparity as the difference in outcomes that can be attributed to differences between the two populations that are deemed unjust in the context of the study (including those driven by systemic racism or structural factors such as social determinants of health). Finally, we suggest going beyond documenting disparities to analyze contributing factors, such as by decomposing the resulting disparity estimate into its associated factors to consider the potential to reduce the disparity by addressing specific factors.

To illustrate the value of these recommendations for both estimating and interpreting disparity estimates, we provide several empirical examples using data from the NHIS. These examples demonstrate some of the ways in which disparity estimates and interpretation can vary with different empirical choices, thereby affirming the importance of motivating these choices with a clear research question and a comprehensive conceptual framework. In particular, we find that the residual direct effect approach may understate the magnitude of racial disparities (using the IOM definition) by adjusting for some of the unjust drivers of the disparity.

This report aims to establish broad principles for the analysis and interpretation of disparities in health care access and use, but numerous conceptual and empirical issues not explicitly addressed here have been explored in other studies. Here we only specifically consider defining disparities as the absolute differences between two groups, but disparity measures can also be computed in relative terms or compared with a fixed benchmark (McMorrow et al. 2015),[8] and methodological guidelines have been developed for such analyses (Keppel et al. 2005). Further, we do not discuss issues of intersectionality across marginalized groups, but a growing body of literature has explored the ways in which an individual's multiple identities (e.g., based on race, gender, class, and sexual orientation, etc.) can interact to affect inequities in health (Bauer 2014; Bauer and Scheim 2019). In addition, we only discuss the application of our recommendations using linear regression models and associated decomposition approaches. There are also nonlinear options for model estimation and decomposition and propensity-score and rank-and-replace methods for producing disparity estimates adjusted for selected characteristics (Cook et al. 2009). Moreover, we focus only on estimating disparities within a single time period and do not discuss issues with isolating the causal impact of a specific intervention on disparities in health care access. These and other methodological issues will have important implications for specific analyses, but we suggest that following the broader methodological recommendations presented here as a first step will lead to higher-quality and more actionable evidence on health care disparities.

Clear, high-quality measures of racial and ethnic disparities in health care use, with explicit definitions and interpretations, are critical to understanding disparities and exploring their causal factors. Well-defined and interpretable estimates are necessary to produce more actionable information to address disparities, guide and evaluate interventions to reduce and eliminate disparities in health outcomes, and set and prioritize equity policy agendas for policymakers and the public.

# Appendix A. Tables for Empirical Examples

APPENDIX TABLE A.1

**Covariate Means**

| | White | Black | Diff. | SE | *p*-value | |
|---|---|---|---|---|---|---|
| Age 19–25 | 0.139 | 0.166 | -0.026 | 0.008 | 0.001 | * |
| Age 26–34 | 0.193 | 0.229 | -0.036 | 0.008 | 0.000 | * |
| Age 35–44 | 0.196 | 0.208 | -0.012 | 0.007 | 0.087 | |
| Age 45–54 | 0.225 | 0.209 | 0.016 | 0.007 | 0.024 | * |
| Age 55–64 | 0.247 | 0.188 | 0.059 | 0.006 | 0.000 | * |
| Female | 0.496 | 0.535 | -0.039 | 0.009 | 0.000 | * |
| Male | 0.504 | 0.465 | 0.039 | 0.009 | 0.000 | * |
| None or mild psychological distress | 0.882 | 0.882 | 0.000 | 0.006 | 0.993 | |
| Moderate psychological distress | 0.079 | 0.082 | -0.003 | 0.005 | 0.596 | |
| Severe psychological distress | 0.039 | 0.036 | 0.003 | 0.003 | 0.396 | |
| Excellent/very good health, self-reported | 0.677 | 0.568 | 0.108 | 0.009 | 0.000 | * |
| Good health, self-reported | 0.230 | 0.288 | -0.058 | 0.008 | 0.000 | * |
| Fair/poor health, self-reported | 0.093 | 0.143 | -0.050 | 0.006 | 0.000 | * |
| Obese | 0.307 | 0.407 | -0.100 | 0.009 | 0.000 | * |
| Current smoker | 0.180 | 0.165 | 0.015 | 0.006 | 0.013 | * |
| Former smoker | 0.222 | 0.100 | 0.122 | 0.005 | 0.000 | * |
| Never smoker | 0.598 | 0.735 | -0.138 | 0.008 | 0.000 | * |
| Diagnosed hypertension (past 12 months) | 0.167 | 0.237 | -0.070 | 0.007 | 0.000 | * |
| Diagnosed diabetes (ever) | 0.063 | 0.090 | -0.027 | 0.005 | 0.000 | * |
| Diagnosed weak/failing kidneys (past 12 months) | 0.012 | 0.016 | -0.004 | 0.002 | 0.031 | * |
| Asthma (time of survey) | 0.085 | 0.093 | -0.008 | 0.005 | 0.086 | |
| Diagnosed emphysema (ever) | 0.011 | 0.006 | 0.005 | 0.001 | 0.000 | * |
| Diagnosed chronic bronchitis (past 12 months) | 0.036 | 0.034 | 0.001 | 0.003 | 0.602 | |
| Coronary heart disease (ever) | 0.021 | 0.024 | -0.003 | 0.002 | 0.192 | |
| Angina pectoris (ever) | 0.011 | 0.010 | 0.001 | 0.002 | 0.375 | |
| Heart attack (ever) | 0.017 | 0.017 | 0.000 | 0.002 | 0.857 | |
| Other heart condition (ever) | 0.065 | 0.050 | 0.015 | 0.004 | 0.000 | * |
| High cholesterol (past 12 months) | 0.160 | 0.123 | 0.037 | 0.006 | 0.000 | * |
| Stroke (ever) | 0.016 | 0.029 | -0.012 | 0.003 | 0.000 | * |
| Chronic liver condition (ever) | 0.014 | 0.008 | 0.006 | 0.001 | 0.000 | * |
| Diagnosed hepatitis (ever) | 0.021 | 0.016 | 0.006 | 0.002 | 0.004 | * |
| Diagnosed liver condition (past 12 months) | 0.018 | 0.011 | 0.008 | 0.002 | 0.000 | * |
| Diagnosed ulcer (ever) | 0.058 | 0.042 | 0.016 | 0.003 | 0.000 | * |
| Diagnosed cancer (ever) | 0.072 | 0.028 | 0.044 | 0.003 | 0.000 | * |
| Diagnosed arthritis (ever) | 0.201 | 0.169 | 0.032 | 0.006 | 0.000 | * |
| Medicare: public hierarchy | 0.038 | 0.056 | -0.018 | 0.004 | 0.000 | * |
| Medicaid/CHIP: public hierarchy | 0.082 | 0.183 | -0.102 | 0.006 | 0.000 | * |
| Other public: public hierarchy | 0.006 | 0.013 | -0.007 | 0.002 | 0.001 | * |
| Exchange: public hierarchy | 0.045 | 0.038 | 0.007 | 0.003 | 0.031 | * |
| Employer: public hierarchy | 0.694 | 0.544 | 0.150 | 0.009 | 0.000 | * |
| Direct purchase: public hierarchy | 0.033 | 0.011 | 0.023 | 0.003 | 0.000 | * |
| Other private: public hierarchy | 0.017 | 0.017 | 0.000 | 0.002 | 0.864 | |
| Uninsured | 0.084 | 0.138 | -0.054 | 0.006 | 0.000 | * |

| | White | Black | Diff. | SE | *p*-value | |
|---|---|---|---|---|---|---|
| Lesbian, gay, or bisexual | 0.034 | 0.032 | 0.003 | 0.003 | 0.395 | |
| Not lesbian, gay, or bisexual | 0.966 | 0.968 | -0.003 | 0.003 | 0.395 | |
| Citizen | 0.982 | 0.940 | 0.042 | 0.005 | 0.000 | * |
| Noncitizen | 0.018 | 0.060 | -0.042 | 0.005 | 0.000 | * |
| Married | 0.574 | 0.329 | 0.245 | 0.009 | 0.000 | * |
| Lives with partner | 0.095 | 0.084 | 0.011 | 0.005 | 0.028 | * |
| Widowed, separated, or divorced | 0.119 | 0.172 | -0.053 | 0.006 | 0.000 | * |
| Never married | 0.211 | 0.415 | -0.204 | 0.009 | 0.000 | * |
| Northeast | 0.189 | 0.159 | 0.030 | 0.007 | 0.000 | * |
| Midwest | 0.281 | 0.153 | 0.128 | 0.006 | 0.000 | * |
| South | 0.326 | 0.614 | -0.288 | 0.009 | 0.000 | * |
| West | 0.204 | 0.074 | 0.130 | 0.005 | 0.000 | * |
| Received SSI (past 12 months) | 0.020 | 0.052 | -0.032 | 0.003 | 0.000 | * |
| Received public assistance (past 12 months) | 0.006 | 0.018 | -0.012 | 0.002 | 0.000 | * |
| Received SNAP benefits (past 12 months) | 0.086 | 0.258 | -0.172 | 0.007 | 0.000 | * |
| Education: less than high school | 0.056 | 0.111 | -0.056 | 0.005 | 0.000 | * |
| Education: high school graduate | 0.220 | 0.285 | -0.065 | 0.008 | 0.000 | * |
| Education: some college | 0.325 | 0.355 | -0.030 | 0.009 | 0.000 | * |
| Education: college graduate | 0.399 | 0.248 | 0.151 | 0.008 | 0.000 | * |
| Works full time | 0.646 | 0.600 | 0.046 | 0.009 | 0.000 | * |
| Works part time | 0.123 | 0.107 | 0.016 | 0.006 | 0.004 | * |
| Not working | 0.230 | 0.293 | -0.062 | 0.008 | 0.000 | * |
| Owns home | 0.706 | 0.425 | 0.281 | 0.009 | 0.000 | * |
| Rents home | 0.271 | 0.549 | -0.278 | 0.009 | 0.000 | * |
| Does not rent or own home | 0.022 | 0.026 | -0.004 | 0.002 | 0.141 | |
| Family income < 100% of FPL | 0.084 | 0.215 | -0.131 | 0.007 | 0.000 | * |
| Family income 100–200% of FPL | 0.121 | 0.212 | -0.091 | 0.007 | 0.000 | * |
| Family income 200–400% of FPL | 0.273 | 0.296 | -0.023 | 0.008 | 0.005 | * |
| Family income > 400% of FPL | 0.522 | 0.277 | 0.245 | 0.008 | 0.000 | * |

**Source:** Authors' analysis of the 2016–18 National Health Interview Survey.

**Notes:** Diff. = difference; SE = standard error of difference; CHIP = Children's Health Insurance Program; SSI = Supplemental Security Income; SNAP = Supplemental Nutrition Assistance Program; FPL = federal poverty level.

* *p*-value < 0.05 on a two-tailed test.

APPENDIX TABLE A.2

**Usual Source of Care—Residual Direct Effect Regression Models**

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| **White, non-Hispanic** | **0.028** | **0.007** | * | **0.036** | **0.007** | * | **0.034** | **0.007** | * | **0.016** | **0.007** | * | **-0.008** | **0.007** | |
| Age 26–34 | 0.013 | 0.010 | | 0.019 | 0.010 | | 0.015 | 0.010 | | 0.013 | 0.010 | | -0.002 | 0.010 | |
| Age 35–44 | 0.106 | 0.009 | * | 0.111 | 0.009 | * | 0.099 | 0.009 | * | 0.091 | 0.009 | * | 0.062 | 0.010 | * |
| Age 45–54 | 0.137 | 0.009 | * | 0.140 | 0.009 | * | 0.111 | 0.009 | * | 0.101 | 0.009 | * | 0.065 | 0.010 | * |
| Age 55–64 | 0.165 | 0.009 | * | 0.169 | 0.009 | * | 0.118 | 0.009 | * | 0.106 | 0.009 | * | 0.066 | 0.010 | * |
| Female | 0.080 | 0.004 | * | 0.077 | 0.004 | * | 0.076 | 0.004 | * | 0.069 | 0.004 | * | 0.068 | 0.004 | * |
| Moderate psychological distress | | | | -0.029 | 0.009 | * | -0.037 | 0.009 | * | -0.026 | 0.009 | * | -0.020 | 0.009 | * |
| Severe psychological distress | | | | -0.016 | 0.012 | | -0.031 | 0.012 | * | -0.009 | 0.011 | | -0.002 | 0.011 | |
| Good health, self-reported | | | | -0.002 | 0.005 | | -0.017 | 0.005 | * | -0.005 | 0.005 | | 0.000 | 0.005 | |
| Fair/poor health, self-reported | | | | 0.015 | 0.008 | | -0.031 | 0.008 | * | -0.016 | 0.008 | | -0.009 | 0.009 | |
| Obese | | | | 0.023 | 0.005 | * | 0.007 | 0.005 | | 0.008 | 0.005 | | 0.007 | 0.005 | |
| Current smoker | | | | -0.110 | 0.007 | * | -0.112 | 0.007 | * | -0.078 | 0.006 | * | -0.061 | 0.007 | * |
| Former smoker | | | | -0.023 | 0.005 | * | -0.028 | 0.005 | * | -0.024 | 0.005 | * | -0.019 | 0.005 | * |
| Diagnosed hypertension (past 12 months) | | | | | | | 0.051 | 0.005 | * | 0.047 | 0.005 | * | 0.048 | 0.005 | * |
| Diagnosed diabetes | | | | | | | 0.027 | 0.006 | * | 0.028 | 0.006 | * | 0.029 | 0.006 | * |
| Diagnosed weak/failing kidneys (past 12 months) | | | | | | | 0.018 | 0.012 | | 0.016 | 0.012 | | 0.018 | 0.012 | |
| Asthma (time of survey) | | | | | | | 0.031 | 0.007 | * | 0.022 | 0.006 | * | 0.021 | 0.006 | * |
| Diagnosed emphysema (ever) | | | | | | | 0.032 | 0.017 | | 0.026 | 0.017 | | 0.032 | 0.017 | |
| Diagnosed chronic bronchitis (past 12 months) | | | | | | | -0.003 | 0.010 | | -0.004 | 0.010 | | -0.002 | 0.010 | |
| Coronary heart disease (ever) | | | | | | | 0.017 | 0.013 | | 0.011 | 0.013 | | 0.009 | 0.012 | |
| Angina pectoris (ever) | | | | | | | -0.051 | 0.022 | * | -0.047 | 0.019 | * | -0.043 | 0.019 | * |
| Heart attack (ever) | | | | | | | 0.010 | 0.015 | | 0.010 | 0.014 | | 0.014 | 0.014 | |
| Other heart condition (ever) | | | | | | | 0.012 | 0.008 | | 0.012 | 0.007 | | 0.009 | 0.007 | |
| High cholesterol (past 12 months) | | | | | | | 0.062 | 0.004 | * | 0.048 | 0.004 | * | 0.045 | 0.004 | * |

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| Diagnosed stroke (ever) | | | | | | | 0.000 | 0.013 | | -0.006 | 0.012 | | -0.005 | 0.012 | |
| Chronic liver condition (ever) | | | | | | | 0.045 | 0.017 | * | 0.037 | 0.016 | * | 0.039 | 0.016 | * |
| Diagnosed hepatitis (ever) | | | | | | | 0.022 | 0.012 | | 0.020 | 0.011 | | 0.024 | 0.011 | * |
| Diagnosed liver condition (past 12 months) | | | | | | | -0.018 | 0.017 | | -0.014 | 0.016 | | -0.015 | 0.015 | |
| Diagnosed ulcer (ever) | | | | | | | 0.013 | 0.008 | | 0.016 | 0.007 | * | 0.017 | 0.007 | * |
| Diagnosed cancer (ever) | | | | | | | 0.028 | 0.006 | * | 0.022 | 0.006 | * | 0.019 | 0.006 | * |
| Diagnosed arthritis (ever) | | | | | | | 0.038 | 0.005 | * | 0.030 | 0.005 | * | 0.028 | 0.005 | * |
| Medicare: public hierarchy | | | | | | | | | | -0.011 | 0.009 | | 0.007 | 0.010 | |
| Medicaid/CHIP: public hierarchy | | | | | | | | | | -0.019 | 0.008 | * | 0.011 | 0.010 | |
| Other public: public hierarchy | | | | | | | | | | 0.025 | 0.018 | | 0.043 | 0.018 | * |
| Exchange: public hierarchy | | | | | | | | | | -0.037 | 0.009 | * | -0.020 | 0.010 | * |
| Direct purchase: public hierarchy | | | | | | | | | | -0.022 | 0.013 | | -0.012 | 0.013 | |
| Other private: public hierarchy | | | | | | | | | | -0.017 | 0.016 | | -0.005 | 0.016 | |
| Uninsured | | | | | | | | | | -0.333 | 0.011 | * | -0.299 | 0.011 | * |
| Lesbian, gay, or bisexual | | | | | | | | | | | | | -0.002 | 0.012 | |
| Noncitizen | | | | | | | | | | | | | -0.077 | 0.021 | * |
| Lives with partner | | | | | | | | | | | | | -0.064 | 0.009 | * |
| Widowed, separated, or divorced | | | | | | | | | | | | | -0.022 | 0.006 | * |
| Never married | | | | | | | | | | | | | -0.021 | 0.007 | * |
| Midwest | | | | | | | | | | | | | -0.028 | 0.006 | * |
| South | | | | | | | | | | | | | -0.041 | 0.006 | * |
| West | | | | | | | | | | | | | -0.051 | 0.007 | * |
| Education: less than high school | | | | | | | | | | | | | -0.037 | 0.011 | * |
| Education: high school graduate | | | | | | | | | | | | | -0.017 | 0.006 | * |
| Education: some college | | | | | | | | | | | | | 0.004 | 0.005 | |
| Works part time | | | | | | | | | | | | | 0.010 | 0.007 | |
| Not working | | | | | | | | | | | | | 0.008 | 0.006 | |
| Rents home | | | | | | | | | | | | | -0.047 | 0.006 | * |

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **β** | **SE** | | **β** | **SE** | | **β** | **SE** | | **β** | **SE** | | **β** | **SE** | |
| Does not rent or own home | | | | | | | | | | | | | 0.005 | 0.013 | |
| Family income < 100% of FPL | | | | | | | | | | | | | -0.045 | 0.010 | * |
| Family income 100–200% of FPL | | | | | | | | | | | | | -0.039 | 0.009 | * |
| Family income 200–400% of FPL | | | | | | | | | | | | | -0.009 | 0.005 | |
| Received SSI (past 12 months) | | | | | | | | | | | | | 0.051 | 0.011 | * |
| Received public assistance (past 12 months) | | | | | | | | | | | | | 0.061 | 0.020 | * |
| Received SNAP benefits (past 12 months) | | | | | | | | | | | | | 0.009 | 0.010 | |
| Constant | 0.697 | 0.011 | * | 0.708 | 0.011 | * | 0.712 | 0.011 | * | 0.765 | 0.011 | * | 0.873 | 0.014 | * |
| **Sample size** | **42,857** | | | **42,857** | | | **42,857** | | | **42,857** | | | **42,857** | | |

**Source:** Authors' analysis of the 2016–18 National Health Interview Survey.

**Notes:** β = regression coefficient; SE = standard error of regression coefficient; SES = socioeconomic status; CHIP = Children's Health Insurance Program; FPL = federal poverty level; SSI = Supplemental Security Income; SNAP = Supplemental Nutrition Assistance Program.

* *p*-value < 0.05 on a two-tailed test.

APPENDIX TABLE A.3

**Any Provider Visit—Residual Direct Effect Regression Models**

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| **White, non-Hispanic** | **0.010** | **0.007** | | **0.019** | **0.007** | * | **0.018** | **0.007** | * | **0.003** | **0.007** | | **-0.013** | **0.007** | |
| Age 26–34 | 0.005 | 0.009 | | 0.005 | 0.009 | | 0.001 | 0.009 | | -0.001 | 0.009 | | -0.025 | 0.010 | * |
| Age 35–44 | 0.044 | 0.009 | * | 0.040 | 0.009 | * | 0.024 | 0.009 | * | 0.016 | 0.009 | | -0.017 | 0.010 | |
| Age 45–54 | 0.074 | 0.009 | * | 0.064 | 0.009 | * | 0.022 | 0.009 | * | 0.014 | 0.009 | | -0.022 | 0.010 | * |
| Age 55–64 | 0.109 | 0.008 | * | 0.094 | 0.009 | * | 0.022 | 0.009 | * | 0.013 | 0.009 | | -0.026 | 0.010 | * |
| Female | 0.117 | 0.004 | * | 0.113 | 0.004 | * | 0.112 | 0.004 | * | 0.107 | 0.004 | * | 0.103 | 0.004 | * |
| Moderate psychological distress | | | | 0.020 | 0.008 | * | 0.008 | 0.008 | | 0.017 | 0.008 | * | 0.023 | 0.008 | * |
| Severe psychological distress | | | | 0.032 | 0.009 | * | 0.010 | 0.009 | | 0.029 | 0.009 | * | 0.031 | 0.009 | * |
| Good health, self-reported | | | | 0.020 | 0.005 | * | -0.002 | 0.005 | | 0.008 | 0.005 | | 0.016 | 0.005 | * |
| Fair/poor health, self-reported | | | | 0.081 | 0.006 | * | 0.014 | 0.007 | * | 0.027 | 0.007 | * | 0.035 | 0.007 | * |
| Obese | | | | 0.031 | 0.005 | * | 0.008 | 0.005 | | 0.008 | 0.005 | | 0.012 | 0.005 | * |
| Current smoker | | | | -0.079 | 0.006 | * | -0.082 | 0.006 | * | -0.055 | 0.006 | * | -0.035 | 0.007 | * |
| Former smoker | | | | 0.006 | 0.005 | | 0.000 | 0.005 | | 0.003 | 0.005 | | 0.010 | 0.005 | |
| Diagnosed hypertension (past 12 months) | | | | | | | 0.081 | 0.004 | * | 0.078 | 0.004 | * | 0.080 | 0.004 | * |
| Diagnosed diabetes (ever) | | | | | | | 0.043 | 0.005 | * | 0.043 | 0.005 | * | 0.044 | 0.005 | * |
| Diagnosed weak/failing kidneys (past 12 months) | | | | | | | 0.002 | 0.011 | | 0.000 | 0.010 | | 0.001 | 0.010 | |
| Asthma (time of survey) | | | | | | | 0.046 | 0.006 | * | 0.040 | 0.006 | * | 0.037 | 0.006 | * |
| Diagnosed emphysema | | | | | | | -0.003 | 0.014 | | -0.007 | 0.015 | | -0.001 | 0.015 | |
| Diagnosed chronic bronchitis (past 12 months) | | | | | | | 0.019 | 0.008 | * | 0.019 | 0.008 | * | 0.019 | 0.008 | * |
| Coronary heart disease (ever) | | | | | | | 0.032 | 0.008 | * | 0.028 | 0.008 | * | 0.027 | 0.008 | * |
| Angina pectoris (ever) | | | | | | | -0.035 | 0.016 | * | -0.034 | 0.016 | * | -0.032 | 0.016 | * |
| Heart attack (ever) | | | | | | | 0.023 | 0.011 | * | 0.023 | 0.010 | * | 0.025 | 0.010 | * |
| Other heart condition (ever) | | | | | | | 0.025 | 0.007 | * | 0.025 | 0.007 | * | 0.022 | 0.007 | * |
| High cholesterol (past 12 months) | | | | | | | 0.072 | 0.004 | * | 0.062 | 0.004 | * | 0.058 | 0.004 | * |

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| Diagnosed stroke (ever) | | | | | | | 0.005 | 0.011 | | 0.001 | 0.011 | | -0.002 | 0.011 | |
| Chronic liver condition (ever) | | | | | | | 0.012 | 0.019 | | 0.008 | 0.019 | | 0.009 | 0.019 | |
| Diagnosed hepatitis (ever) | | | | | | | 0.022 | 0.015 | | 0.020 | 0.015 | | 0.019 | 0.015 | |
| Diagnosed liver condition (past 12 months) | | | | | | | 0.005 | 0.016 | | 0.007 | 0.016 | | 0.005 | 0.016 | |
| Diagnosed ulcer (ever) | | | | | | | 0.010 | 0.008 | | 0.012 | 0.008 | | 0.014 | 0.008 | |
| Diagnosed cancer (ever) | | | | | | | 0.046 | 0.006 | * | 0.041 | 0.006 | * | 0.037 | 0.006 | * |
| Diagnosed arthritis (ever) | | | | | | | 0.054 | 0.005 | * | 0.048 | 0.005 | * | 0.046 | 0.004 | * |
| Medicare: public hierarchy | | | | | | | | | | -0.006 | 0.007 | | 0.006 | 0.008 | |
| Medicaid/CHIP: public hierarchy | | | | | | | | | | -0.026 | 0.008 | * | -0.002 | 0.009 | |
| Other public: public hierarchy | | | | | | | | | | -0.005 | 0.020 | | 0.008 | 0.021 | |
| Exchange: public hierarchy | | | | | | | | | | -0.042 | 0.011 | * | -0.026 | 0.011 | * |
| Direct purchase: public hierarchy | | | | | | | | | | -0.019 | 0.013 | | -0.012 | 0.013 | |
| Other private: public hierarchy | | | | | | | | | | -0.021 | 0.016 | | -0.017 | 0.016 | |
| Uninsured | | | | | | | | | | -0.255 | 0.010 | * | -0.226 | 0.010 | * |
| Lesbian, gay, or bisexual | | | | | | | | | | | | | 0.014 | 0.011 | |
| Noncitizen | | | | | | | | | | | | | -0.034 | 0.021 | |
| Lives with partner | | | | | | | | | | | | | -0.036 | 0.009 | * |
| Widowed, separated, or divorced | | | | | | | | | | | | | -0.020 | 0.006 | * |
| Never married | | | | | | | | | | | | | -0.026 | 0.007 | * |
| Midwest | | | | | | | | | | | | | -0.033 | 0.006 | * |
| South | | | | | | | | | | | | | -0.023 | 0.006 | * |
| West | | | | | | | | | | | | | -0.035 | 0.007 | * |
| Education: less than high school | | | | | | | | | | | | | -0.068 | 0.011 | * |
| Education: high school graduate | | | | | | | | | | | | | -0.049 | 0.006 | * |
| Education: some college | | | | | | | | | | | | | -0.024 | 0.005 | * |
| Works part time | | | | | | | | | | | | | 0.005 | 0.007 | |
| Not working | | | | | | | | | | | | | 0.026 | 0.006 | * |
| Rents home | | | | | | | | | | | | | -0.015 | 0.006 | * |

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **β** | **SE** | | **β** | **SE** | | **β** | **SE** | | **β** | **SE** | | **β** | **SE** | |
| Does not rent or own home | | | | | | | | | | | | | -0.009 | 0.014 | |
| Family income < 100% of FPL | | | | | | | | | | | | | -0.048 | 0.010 | * |
| Family income 100–200% of FPL | | | | | | | | | | | | | -0.044 | 0.008 | * |
| Family income 200–400% of FPL | | | | | | | | | | | | | -0.027 | 0.006 | * |
| Received SSI (past 12 months) | | | | | | | | | | | | | 0.029 | 0.011 | * |
| Received public assistance (past 12 months) | | | | | | | | | | | | | 0.036 | 0.018 | * |
| Received SNAP benefits (past 12 months) | | | | | | | | | | | | | 0.019 | 0.009 | * |
| Constant | 0.728 | 0.010 | * | 0.716 | 0.011 | * | 0.721 | 0.011 | * | 0.764 | 0.011 | * | 0.866 | 0.014 | * |
| **Sample size** | **42,822** | | | **42,822** | | | **42,822** | | | **42,822** | | | **42,822** | | |

**Source:** Authors' analysis of the 2016–18 National Health Interview Survey.

**Notes:** β = regression coefficient; SE = standard error of regression coefficient; SES = socioeconomic status; CHIP = Children's Health Insurance Program; FPL = federal poverty level; SSI = Supplemental Security Income; SNAP = Supplemental Nutrition Assistance Program.

* *p*-value < 0.05 on a two-tailed test.

APPENDIX TABLE A.4

**Specialist Visit—Residual Direct Effect Regression Models**

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| **White, non-Hispanic** | **0.077** | **0.007** | * | **0.097** | **0.007** | * | **0.082** | **0.007** | * | **0.072** | **0.007** | * | **0.046** | **0.007** | * |
| Age 26–34 | 0.048 | 0.008 | * | 0.046 | 0.008 | * | 0.037 | 0.008 | * | 0.036 | 0.008 | * | 0.012 | 0.009 | |
| Age 35–44 | 0.100 | 0.008 | * | 0.086 | 0.008 | * | 0.061 | 0.008 | * | 0.056 | 0.008 | * | 0.027 | 0.009 | * |
| Age 45–54 | 0.175 | 0.009 | * | 0.145 | 0.009 | * | 0.082 | 0.009 | * | 0.075 | 0.009 | * | 0.046 | 0.010 | * |
| Age 55–64 | 0.241 | 0.009 | * | 0.196 | 0.009 | * | 0.084 | 0.009 | * | 0.075 | 0.009 | * | 0.045 | 0.010 | * |
| Female | 0.051 | 0.005 | * | 0.045 | 0.005 | * | 0.034 | 0.005 | * | 0.034 | 0.005 | * | 0.029 | 0.005 | * |
| Moderate psychological distress | | | | 0.040 | 0.010 | * | 0.014 | 0.010 | | 0.020 | 0.010 | * | 0.026 | 0.010 | * |
| Severe psychological distress | | | | 0.044 | 0.015 | * | 0.002 | 0.015 | | 0.013 | 0.015 | | 0.020 | 0.014 | |
| Good health, self-reported | | | | 0.091 | 0.007 | * | 0.055 | 0.006 | * | 0.061 | 0.006 | * | 0.074 | 0.006 | * |
| Fair/poor health, self-reported | | | | 0.264 | 0.011 | * | 0.139 | 0.011 | * | 0.144 | 0.011 | * | 0.162 | 0.011 | * |
| Obese | | | | 0.004 | 0.006 | | -0.019 | 0.006 | * | -0.018 | 0.006 | * | -0.010 | 0.006 | |
| Current smoker | | | | -0.068 | 0.007 | * | -0.073 | 0.007 | * | -0.057 | 0.007 | * | -0.025 | 0.007 | * |
| Former smoker | | | | 0.020 | 0.007 | * | 0.007 | 0.007 | | 0.010 | 0.007 | | 0.021 | 0.007 | * |
| Diagnosed hypertension (past 12 months) | | | | | | | 0.026 | 0.008 | * | 0.024 | 0.008 | * | 0.029 | 0.008 | * |
| Diagnosed diabetes (ever) | | | | | | | 0.071 | 0.012 | * | 0.071 | 0.012 | * | 0.073 | 0.012 | * |
| Diagnosed weak/failing kidneys (past 12 months) | | | | | | | 0.163 | 0.025 | * | 0.159 | 0.025 | * | 0.158 | 0.025 | * |
| Asthma (time of survey) | | | | | | | 0.060 | 0.010 | * | 0.057 | 0.010 | * | 0.053 | 0.010 | * |
| Diagnosed emphysema (ever) | | | | | | | -0.053 | 0.029 | | -0.058 | 0.029 | * | -0.043 | 0.028 | |
| Diagnosed chronic bronchitis (past 12 months) | | | | | | | 0.022 | 0.016 | | 0.022 | 0.016 | | 0.023 | 0.016 | |
| Coronary heart disease (ever) | | | | | | | 0.102 | 0.024 | * | 0.099 | 0.024 | * | 0.098 | 0.024 | * |
| Angina pectoris (even) | | | | | | | 0.033 | 0.029 | | 0.033 | 0.029 | | 0.036 | 0.028 | |
| Heart attack (even) | | | | | | | 0.054 | 0.024 | * | 0.054 | 0.024 | * | 0.056 | 0.024 | * |
| Other heart condition (even) | | | | | | | 0.138 | 0.012 | * | 0.137 | 0.012 | * | 0.132 | 0.012 | * |
| High cholesterol (past 12 months) | | | | | | | 0.052 | 0.008 | * | 0.045 | 0.008 | * | 0.042 | 0.008 | * |

| | Adjusted for Age/Sex | | Adjusted for Age/Sex/ Self-Reported Health | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | β | SE | β | SE | β | SE | | β | SE | | β | SE | |
| Diagnosed stroke (ever) | | | | | 0.042 | 0.023 | | 0.036 | 0.022 | | 0.038 | 0.022 | |
| Chronic liver condition (ever) | | | | | 0.105 | 0.029 | * | 0.102 | 0.029 | * | 0.101 | 0.029 | * |
| Diagnosed hepatitis (ever) | | | | | 0.029 | 0.020 | | 0.029 | 0.020 | | 0.028 | 0.019 | |
| Diagnosed liver condition (past 12 months) | | | | | 0.046 | 0.025 | | 0.048 | 0.025 | | 0.049 | 0.025 | * |
| Diagnosed ulcer (ever) | | | | | 0.036 | 0.012 | * | 0.036 | 0.011 | * | 0.038 | 0.011 | * |
| Diagnosed cancer (ever) | | | | | 0.205 | 0.012 | * | 0.202 | 0.012 | * | 0.197 | 0.012 | * |
| Diagnosed arthritis (ever) | | | | | 0.136 | 0.008 | * | 0.132 | 0.008 | * | 0.132 | 0.008 | * |
| Medicare: public hierarchy | | | | | | | | 0.031 | 0.015 | * | 0.066 | 0.015 | * |
| Medicaid/CHIP: public hierarchy | | | | | | | | -0.042 | 0.009 | * | 0.010 | 0.010 | |
| Other public: public hierarchy | | | | | | | | -0.008 | 0.029 | | 0.021 | 0.029 | |
| Exchange: public hierarchy | | | | | | | | -0.033 | 0.012 | * | -0.012 | 0.012 | |
| Direct purchase: public hierarchy | | | | | | | | -0.019 | 0.014 | | -0.011 | 0.014 | |
| Other private: public hierarchy | | | | | | | | 0.013 | 0.019 | | 0.020 | 0.019 | |
| Uninsured | | | | | | | | -0.137 | 0.007 | * | -0.093 | 0.007 | * |
| Lesbian, gay, or bisexual | | | | | | | | | | | 0.030 | 0.013 | * |
| Noncitizen | | | | | | | | | | | -0.018 | 0.014 | |
| Lives with partner | | | | | | | | | | | -0.003 | 0.009 | |
| Widowed, separated, or divorced | | | | | | | | | | | -0.018 | 0.007 | * |
| Never married | | | | | | | | | | | -0.009 | 0.007 | |
| Midwest | | | | | | | | | | | -0.020 | 0.008 | * |
| South | | | | | | | | | | | -0.019 | 0.007 | * |
| West | | | | | | | | | | | -0.017 | 0.008 | * |
| Education: less than high school | | | | | | | | | | | -0.124 | 0.010 | * |
| Education: high school graduate | | | | | | | | | | | -0.081 | 0.007 | * |
| Education: some college | | | | | | | | | | | -0.037 | 0.006 | * |
| Works part time | | | | | | | | | | | 0.018 | 0.008 | * |
| Not working | | | | | | | | | | | 0.022 | 0.007 | * |
| Rents home | | | | | | | | | | | -0.009 | 0.006 | |

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **β** | **SE** | | **β** | **SE** | | **β** | **SE** | | **β** | **SE** | | **β** | **SE** | |
| Does not rent or own home | | | | | | | | | | | | | 0.009 | 0.014 | |
| Family income < 100% of FPL | | | | | | | | | | | | | -0.066 | 0.010 | * |
| Family income 100–200% of FPL | | | | | | | | | | | | | -0.060 | 0.008 | * |
| Family income 200–400% of FPL | | | | | | | | | | | | | -0.038 | 0.006 | * |
| Received SSI (past 12 months) | | | | | | | | | | | | | 0.021 | 0.018 | |
| Received public assistance (past 12 months) | | | | | | | | | | | | | 0.027 | 0.029 | |
| Received SNAP benefits (past 12 months) | | | | | | | | | | | | | -0.018 | 0.009 | |
| Constant | 0.049 | 0.009 | * | 0.009 | 0.009 | | 0.028 | 0.009 | * | 0.055 | 0.009 | * | 0.156 | 0.014 | * |
| **Sample size** | **42,849** | | | **42,849** | | | **42,849** | | | **42,849** | | | **42,849** | | |

**Source:** Authors' analysis of the 2016–18 National Health Interview Survey.

**Notes:** β = regression coefficient; SE = standard error of regression coefficient tus; CHIP = Children's Health Insurance Program; FPL = federal poverty level; SSI = Supplemental Security Income; SNAP = Supplemental Nutrition Assistance Program.

* *p*-value < 0.05 on a two-tailed test.

APPENDIX TABLE A.5

**Flu Vaccine—Residual Direct Effect Regression Models**

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| **White, non-Hispanic** | **0.079** | **0.008** | * | **0.089** | **0.008** | * | **0.085** | **0.008** | * | **0.070** | **0.008** | * | **0.044** | **0.009** | * |
| Age 26–34 | 0.052 | 0.010 | * | 0.061 | 0.010 | * | 0.057 | 0.010 | * | 0.057 | 0.010 | * | 0.012 | 0.011 | |
| Age 35–44 | 0.084 | 0.010 | * | 0.093 | 0.010 | * | 0.078 | 0.010 | * | 0.070 | 0.010 | * | 0.012 | 0.012 | |
| Age 45–54 | 0.107 | 0.010 | * | 0.114 | 0.010 | * | 0.075 | 0.011 | * | 0.064 | 0.011 | * | 0.009 | 0.012 | |
| Age 55–64 | 0.214 | 0.010 | * | 0.219 | 0.010 | * | 0.150 | 0.011 | * | 0.140 | 0.011 | * | 0.087 | 0.012 | * |
| Female | 0.079 | 0.006 | * | 0.074 | 0.006 | * | 0.072 | 0.006 | * | 0.072 | 0.006 | * | 0.065 | 0.006 | * |
| Moderate psychological distress | | | | -0.003 | 0.011 | | -0.017 | 0.011 | | -0.005 | 0.011 | | 0.001 | 0.011 | |
| Severe psychological distress | | | | -0.024 | 0.015 | | -0.049 | 0.015 | * | -0.027 | 0.015 | | -0.023 | 0.015 | |
| Good health, self-reported | | | | 0.003 | 0.007 | | -0.020 | 0.007 | * | -0.010 | 0.007 | | 0.004 | 0.007 | |
| Fair/poor health, self-reported | | | | 0.044 | 0.010 | * | -0.034 | 0.011 | * | -0.019 | 0.011 | | -0.004 | 0.012 | |
| Obese | | | | 0.009 | 0.006 | | -0.013 | 0.006 | * | -0.013 | 0.006 | * | -0.004 | 0.006 | |
| Current smoker | | | | -0.149 | 0.007 | * | -0.151 | 0.007 | * | -0.124 | 0.007 | * | -0.088 | 0.008 | * |
| Former smoker | | | | -0.030 | 0.008 | * | -0.036 | 0.007 | * | -0.031 | 0.007 | * | -0.015 | 0.007 | * |
| Diagnosed hypertension (past 12 months) | | | | | | | 0.041 | 0.009 | * | 0.040 | 0.008 | * | 0.044 | 0.008 | * |
| Diagnosed diabetes (ever) | | | | | | | 0.086 | 0.012 | * | 0.086 | 0.012 | * | 0.087 | 0.012 | * |
| Diagnosed weak/failing kidneys (past 12 months) | | | | | | | 0.095 | 0.025 | * | 0.093 | 0.025 | * | 0.096 | 0.025 | * |
| Asthma (time of survey) | | | | | | | 0.068 | 0.011 | * | 0.064 | 0.011 | * | 0.059 | 0.011 | * |
| Diagnosed emphysema (ever) | | | | | | | 0.030 | 0.029 | | 0.027 | 0.029 | | 0.030 | 0.029 | |
| Diagnosed chronic bronchitis (past 12 months) | | | | | | | 0.018 | 0.016 | | 0.017 | 0.016 | | 0.016 | 0.016 | |
| Coronary heart disease (ever) | | | | | | | 0.048 | 0.024 | * | 0.043 | 0.024 | | 0.044 | 0.024 | |
| Angina pectoris (ever) | | | | | | | -0.001 | 0.029 | | 0.001 | 0.029 | | 0.004 | 0.029 | |
| Heart attack (ever) | | | | | | | 0.012 | 0.024 | | 0.013 | 0.023 | | 0.015 | 0.023 | |
| Other heart condition (ever) | | | | | | | 0.040 | 0.012 | * | 0.040 | 0.012 | * | 0.035 | 0.012 | * |
| High cholesterol (past 12 months) | | | | | | | 0.085 | 0.009 | * | 0.075 | 0.009 | * | 0.071 | 0.009 | * |

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| Diagnosed stroke (ever) | | | | | | | 0.047 | 0.022 | * | 0.045 | 0.022 | * | 0.041 | 0.022 | |
| Chronic liver condition (ever) | | | | | | | 0.100 | 0.031 | * | 0.095 | 0.031 | * | 0.094 | 0.031 | * |
| Hepatitis diagnosis (ever) | | | | | | | 0.000 | 0.020 | | 0.002 | 0.020 | | 0.000 | 0.020 | |
| Diagnosed liver condition (past 12 months) | | | | | | | -0.060 | 0.027 | * | -0.057 | 0.027 | * | -0.056 | 0.026 | * |
| Diagnosed ulcer (ever) | | | | | | | -0.006 | 0.012 | | -0.004 | 0.012 | | -0.002 | 0.012 | |
| Diagnosed cancer (ever) | | | | | | | 0.052 | 0.012 | * | 0.047 | 0.012 | * | 0.042 | 0.012 | * |
| Diagnosed arthritis (ever) | | | | | | | 0.046 | 0.008 | * | 0.041 | 0.008 | * | 0.041 | 0.008 | * |
| Medicare: public hierarchy | | | | | | | | | | -0.002 | 0.015 | | 0.023 | 0.016 | |
| Medicaid/CHIP: public hierarchy | | | | | | | | | | -0.082 | 0.010 | * | -0.052 | 0.012 | * |
| Other public: public hierarchy | | | | | | | | | | 0.011 | 0.036 | | 0.028 | 0.037 | |
| Exchange: public hierarchy | | | | | | | | | | -0.127 | 0.013 | * | -0.104 | 0.013 | * |
| Direct purchase: public hierarchy | | | | | | | | | | -0.102 | 0.017 | * | -0.090 | 0.017 | * |
| Other private: public hierarchy | | | | | | | | | | 0.003 | 0.022 | | 0.006 | 0.022 | |
| Uninsured | | | | | | | | | | -0.217 | 0.008 | * | -0.176 | 0.009 | * |
| Lesbian, gay, or bisexual | | | | | | | | | | | | | 0.037 | 0.015 | * |
| Noncitizen | | | | | | | | | | | | | -0.010 | 0.019 | |
| Lives with partner | | | | | | | | | | | | | -0.045 | 0.011 | * |
| Widowed, separated, or divorced | | | | | | | | | | | | | -0.020 | 0.008 | * |
| Never married | | | | | | | | | | | | | -0.036 | 0.008 | * |
| Midwest | | | | | | | | | | | | | -0.008 | 0.009 | |
| South | | | | | | | | | | | | | -0.027 | 0.009 | * |
| West | | | | | | | | | | | | | -0.030 | 0.010 | * |
| Education: less than high school | | | | | | | | | | | | | -0.125 | 0.012 | * |
| Education: high school graduate | | | | | | | | | | | | | -0.131 | 0.008 | * |
| Education: some college | | | | | | | | | | | | | -0.086 | 0.007 | * |
| Works part time | | | | | | | | | | | | | -0.001 | 0.010 | |
| Not working | | | | | | | | | | | | | 0.010 | 0.008 | |
| Rents home | | | | | | | | | | | | | -0.012 | 0.007 | |

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **β** | **SE** | | **β** | **SE** | | **β** | **SE** | | **β** | **SE** | | **β** | **SE** | |
| Does not rent or own home | | | | | | | | | | | | | 0.010 | 0.017 | |
| Family income < 100% of FPL | | | | | | | | | | | | | -0.018 | 0.012 | |
| Family income 100–200% of FPL | | | | | | | | | | | | | -0.034 | 0.010 | * |
| Family income 200–400% of FPL | | | | | | | | | | | | | -0.026 | 0.007 | * |
| Received SSI (past 12 months) | | | | | | | | | | | | | 0.046 | 0.020 | * |
| Received public assistance (past 12 months) | | | | | | | | | | | | | 0.067 | 0.033 | * |
| Received SNAP benefits (past 12 months) | | | | | | | | | | | | | 0.018 | 0.011 | |
| Constant | 0.171 | 0.011 | * | 0.185 | 0.011 | * | 0.190 | 0.011 | * | 0.239 | 0.012 | * | 0.393 | 0.017 | * |
| **Sample size** | **42,762** | | | **42,762** | | | **42,762** | | | **42,762** | | | **42,762** | | |

**Source:** Authors' analysis of the 2016–18 National Health Interview Survey.

**Notes:** β = regression coefficient; SE = standard error of regression coefficient; CHIP = Children's Health Insurance Program; FPL = federal poverty level; SSI = Supplemental Security Income; SNAP = Supplemental Nutrition Assistance Program.

* *p*-value < 0.05 on a two-tailed test.

APPENDIX TABLE A.6

**Blood Pressure Check—Residual Direct Effect Regression Models**

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| **White, non-Hispanic** | **0.007** | **0.007** | | **0.014** | **0.007** | * | **0.013** | **0.007** | | **-0.003** | **0.007** | | **-0.020** | **0.007** | * |
| Age 26–34 | 0.037 | 0.010 | * | 0.038 | 0.010 | * | 0.033 | 0.010 | * | 0.032 | 0.010 | * | 0.007 | 0.010 | |
| Age 35–44 | 0.082 | 0.009 | * | 0.080 | 0.009 | * | 0.063 | 0.009 | * | 0.055 | 0.009 | * | 0.022 | 0.010 | * |
| Age 45–54 | 0.112 | 0.009 | * | 0.105 | 0.009 | * | 0.064 | 0.009 | * | 0.055 | 0.009 | * | 0.019 | 0.010 | |
| Age 55–64 | 0.146 | 0.009 | * | 0.137 | 0.009 | * | 0.066 | 0.009 | * | 0.055 | 0.009 | * | 0.020 | 0.010 | |
| Female | 0.098 | 0.004 | * | 0.094 | 0.004 | * | 0.093 | 0.004 | * | 0.089 | 0.004 | * | 0.086 | 0.004 | * |
| Moderate psychological distress | | | | 0.021 | 0.008 | * | 0.009 | 0.008 | | 0.019 | 0.008 | * | 0.024 | 0.008 | * |
| Severe psychological distress | | | | 0.039 | 0.009 | * | 0.016 | 0.009 | | 0.037 | 0.009 | * | 0.042 | 0.009 | * |
| Good health, self-reported | | | | 0.009 | 0.005 | | -0.012 | 0.005 | * | -0.002 | 0.005 | | 0.007 | 0.005 | |
| Fair/poor health, self-reported | | | | 0.046 | 0.007 | * | -0.018 | 0.007 | * | -0.002 | 0.007 | | 0.009 | 0.007 | |
| Obese | | | | 0.032 | 0.005 | * | 0.009 | 0.005 | | 0.009 | 0.005 | * | 0.012 | 0.005 | * |
| Current smoker | | | | -0.075 | 0.006 | * | -0.078 | 0.006 | * | -0.049 | 0.006 | * | -0.031 | 0.007 | * |
| Former smoker | | | | 0.004 | 0.005 | | -0.002 | 0.005 | | 0.002 | 0.005 | | 0.008 | 0.005 | |
| Diagnosed hypertension (past 12 months) | | | | | | | 0.083 | 0.004 | * | 0.080 | 0.004 | * | 0.082 | 0.004 | * |
| Diagnosed diabetes (ever) | | | | | | | 0.036 | 0.005 | * | 0.037 | 0.005 | * | 0.038 | 0.005 | * |
| Diagnosed weak/failing kidneys (past 12 months) | | | | | | | -0.005 | 0.011 | | -0.006 | 0.011 | | -0.005 | 0.011 | |
| Asthma (time of survey) | | | | | | | 0.038 | 0.006 | * | 0.032 | 0.006 | * | 0.030 | 0.006 | * |
| Diagnosed emphysema (ever) | | | | | | | -0.007 | 0.015 | | -0.010 | 0.015 | | -0.003 | 0.015 | |
| Diagnosed chronic bronchitis (past 12 months) | | | | | | | 0.033 | 0.008 | * | 0.033 | 0.008 | * | 0.033 | 0.008 | * |
| Coronary heart disease (ever) | | | | | | | 0.035 | 0.008 | * | 0.031 | 0.008 | * | 0.029 | 0.008 | * |
| Angina pectoris (ever) | | | | | | | -0.027 | 0.016 | | -0.024 | 0.016 | | -0.020 | 0.017 | |
| Heart attack (ever) | | | | | | | 0.008 | 0.013 | | 0.010 | 0.012 | | 0.011 | 0.012 | |
| Other heart condition (ever) | | | | | | | 0.014 | 0.007 | * | 0.014 | 0.007 | * | 0.011 | 0.007 | |
| High cholesterol (past 12 months) | | | | | | | 0.071 | 0.004 | * | 0.060 | 0.004 | * | 0.057 | 0.004 | * |

| | Adjusted for Age/Sex | | Adjusted for Age/Sex/ Self-Reported Health | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | β | SE | β | SE | β | SE | | β | SE | | β | SE | |
| Diagnosed stroke (ever) | | | | | 0.018 | 0.010 | | 0.016 | 0.010 | | 0.015 | 0.010 | |
| Chronic liver condition (ever) | | | | | 0.017 | 0.015 | | 0.013 | 0.016 | | 0.013 | 0.016 | |
| Diagnosed hepatitis diagnosis (ever) | | | | | 0.009 | 0.014 | | 0.009 | 0.015 | | 0.010 | 0.015 | |
| Diagnosed liver condition (past 12 months) | | | | | 0.020 | 0.013 | | 0.024 | 0.014 | | 0.025 | 0.014 | |
| Diagnosed ulcer (ever) | | | | | 0.014 | 0.008 | | 0.016 | 0.007 | * | 0.017 | 0.007 | * |
| Diagnosed cancer (ever) | | | | | 0.040 | 0.006 | * | 0.036 | 0.006 | * | 0.032 | 0.006 | * |
| Diagnosed arthritis (ever) | | | | | 0.054 | 0.005 | * | 0.049 | 0.004 | * | 0.047 | 0.004 | * |
| Medicare: public hierarchy | | | | | | | | -0.019 | 0.008 | * | 0.002 | 0.009 | |
| Medicaid/CHIP: public hierarchy | | | | | | | | -0.044 | 0.008 | * | -0.015 | 0.010 | |
| Other public: public hierarchy | | | | | | | | 0.018 | 0.018 | | 0.034 | 0.019 | |
| Exchange: public hierarchy | | | | | | | | -0.045 | 0.010 | * | -0.026 | 0.011 | * |
| Direct purchase: public hierarchy | | | | | | | | -0.027 | 0.013 | * | -0.017 | 0.013 | |
| Other private: public hierarchy | | | | | | | | 0.000 | 0.015 | | 0.008 | 0.015 | |
| Uninsured | | | | | | | | -0.253 | 0.010 | * | -0.220 | 0.010 | * |
| Lesbian, gay, or bisexual | | | | | | | | | | | 0.022 | 0.011 | * |
| Noncitizen | | | | | | | | | | | -0.046 | 0.019 | * |
| Lives with partner | | | | | | | | | | | -0.034 | 0.008 | * |
| Widowed, separated, or divorced | | | | | | | | | | | -0.020 | 0.006 | * |
| Never married | | | | | | | | | | | -0.030 | 0.006 | * |
| Midwest | | | | | | | | | | | -0.015 | 0.006 | * |
| South | | | | | | | | | | | -0.022 | 0.006 | * |
| West | | | | | | | | | | | -0.040 | 0.007 | * |
| Education: less than high school | | | | | | | | | | | -0.081 | 0.011 | * |
| Education: high school graduate | | | | | | | | | | | -0.046 | 0.006 | * |
| Education: some college | | | | | | | | | | | -0.017 | 0.005 | * |
| Works part time | | | | | | | | | | | 0.004 | 0.007 | |
| Not working | | | | | | | | | | | 0.008 | 0.006 | |

| | Adjusted for Age/Sex | | | Adjusted for Age/Sex/ Self-Reported Health | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/ Insurance Coverage | | | Adjusted for Age/Sex/ Self-Reported Health/ Diagnosed Conditions/Insurance Coverage/ Other SES | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| Rents home | | | | | | | | | | | | | -0.009 | 0.006 | |
| Does not rent or own home | | | | | | | | | | | | | -0.008 | 0.014 | |
| Family income < 100% of FPL | | | | | | | | | | | | | -0.041 | 0.010 | * |
| Family income 100–200% of FPL | | | | | | | | | | | | | -0.039 | 0.008 | * |
| Family income 200–400% of FPL | | | | | | | | | | | | | -0.027 | 0.006 | * |
| Received SSI (past 12 months) | | | | | | | | | | | | | 0.032 | 0.012 | * |
| Received public assistance (past 12 months) | | | | | | | | | | | | | 0.048 | 0.017 | * |
| Received SNAP benefits (past 12 months) | | | | | | | | | | | | | 0.011 | 0.009 | |
| Constant | 0.716 | 0.011 | * | 0.708 | 0.011 | * | 0.713 | 0.011 | * | 0.759 | 0.011 | * | 0.856 | 0.014 | * |
| **Sample size** | **42,848** | | | **42,848** | | | **42,848** | | | **42,848** | | | **42,848** | | |

**Source:** Authors' analysis of the 2016–18 National Health Interview Survey.

**Notes:** β = regression coefficient; SE = standard error of regression coefficient; SES = socioeconomic status; CHIP = Children's Health Insurance Program; SSI = Supplemental Security Income; SNAP = Supplemental Nutrition Assistance Program; FPL = federal poverty level.

* *p*-value < 0.05 on a two-tailed test.

APPENDIX TABLE A.7

**Separate Regressions**

*Usual source of care, any provider visit, and specialist visit*

| | Usual Source of Care | | | | | | Any Provider Visit | | | | | | Specialist Visit | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | White | | | Black | | | White | | | Black | | | White | | | Black | | |
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| Age 26–34 | -0.002 | 0.011 | | -0.001 | 0.025 | | -0.021 | 0.010 | * | -0.041 | 0.025 | | 0.012 | 0.010 | | 0.003 | 0.018 | |
| Age 35–44 | 0.062 | 0.011 | * | 0.058 | 0.025 | * | -0.020 | 0.011 | | -0.004 | 0.025 | | 0.023 | 0.011 | * | 0.033 | 0.019 | |
| Age 45–54 | 0.067 | 0.011 | * | 0.051 | 0.026 | | -0.020 | 0.011 | | -0.031 | 0.026 | | 0.044 | 0.011 | * | 0.046 | 0.022 | * |
| Age 55–64 | 0.066 | 0.011 | * | 0.057 | 0.027 | * | -0.031 | 0.011 | * | 0.001 | 0.026 | | 0.048 | 0.012 | * | 0.008 | 0.025 | |
| Female | 0.065 | 0.004 | * | 0.081 | 0.012 | * | 0.103 | 0.005 | * | 0.103 | 0.013 | * | 0.031 | 0.006 | * | 0.017 | 0.012 | |
| Lesbian, gay, or bisexual | 0.011 | 0.012 | | -0.080 | 0.041 | | 0.006 | 0.012 | | 0.049 | 0.030 | | 0.031 | 0.015 | * | 0.023 | 0.031 | |
| Noncitizen | -0.065 | 0.020 | * | -0.089 | 0.043 | * | -0.017 | 0.020 | | -0.049 | 0.042 | | -0.025 | 0.019 | | -0.010 | 0.022 | |
| Lives with partner | -0.065 | 0.010 | * | -0.054 | 0.026 | * | -0.034 | 0.009 | * | -0.051 | 0.027 | | -0.005 | 0.010 | | 0.016 | 0.022 | |
| Widowed, separated, or divorced | -0.021 | 0.006 | * | -0.023 | 0.015 | | -0.022 | 0.006 | * | -0.008 | 0.015 | | -0.019 | 0.008 | * | 0.000 | 0.018 | |
| Never married | -0.025 | 0.007 | * | -0.012 | 0.016 | | -0.026 | 0.007 | * | -0.029 | 0.017 | | -0.014 | 0.007 | | 0.012 | 0.015 | |
| Midwest | -0.027 | 0.006 | * | -0.037 | 0.021 | | -0.040 | 0.006 | * | 0.009 | 0.023 | | -0.024 | 0.008 | * | 0.015 | 0.023 | |
| South | -0.041 | 0.006 | * | -0.040 | 0.016 | * | -0.029 | 0.006 | * | 0.009 | 0.020 | | -0.022 | 0.008 | * | -0.004 | 0.019 | |
| West | -0.050 | 0.007 | * | -0.060 | 0.027 | * | -0.041 | 0.007 | * | 0.006 | 0.030 | | -0.016 | 0.009 | | -0.032 | 0.026 | |
| Moderate psychological distress | -0.017 | 0.009 | | -0.036 | 0.025 | | 0.021 | 0.009 | * | 0.032 | 0.021 | | 0.032 | 0.011 | * | -0.003 | 0.019 | |
| Severe psychological distress | 0.000 | 0.012 | | -0.022 | 0.025 | | 0.032 | 0.010 | * | 0.029 | 0.022 | | 0.018 | 0.016 | | 0.037 | 0.036 | |
| Good health, self-reported | 0.007 | 0.005 | | -0.031 | 0.014 | * | 0.017 | 0.005 | * | 0.009 | 0.014 | | 0.079 | 0.007 | * | 0.051 | 0.014 | * |
| Fair/poor health, self-reported | 0.001 | 0.009 | | -0.048 | 0.024 | * | 0.032 | 0.008 | * | 0.041 | 0.017 | * | 0.168 | 0.012 | * | 0.141 | 0.026 | * |
| Obese | 0.006 | 0.005 | | 0.008 | 0.012 | | 0.010 | 0.005 | | 0.030 | 0.012 | * | -0.013 | 0.006 | * | 0.004 | 0.013 | |
| Current smoker | -0.064 | 0.007 | * | -0.045 | 0.017 | * | -0.041 | 0.007 | * | -0.007 | 0.017 | | -0.025 | 0.008 | * | -0.019 | 0.016 | |
| Former smoker | -0.022 | 0.005 | * | 0.007 | 0.017 | | 0.011 | 0.005 | * | -0.008 | 0.019 | | 0.021 | 0.007 | * | 0.015 | 0.020 | |
| Diagnosed hypertension (past 12 months) | 0.046 | 0.005 | * | 0.056 | 0.012 | * | 0.080 | 0.004 | * | 0.074 | 0.011 | * | 0.030 | 0.009 | * | 0.034 | 0.018 | |

| | Usual Source of Care | | | | | Any Provider Visit | | | | | Specialist Visit | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | White | | | Black | | White | | | Black | | White | | | Black | |
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| Diagnosed diabetes (ever) | 0.032 | 0.006 | * | 0.019 | 0.016 | | 0.046 | 0.006 | * | 0.043 | 0.012 | * | 0.061 | 0.013 | * | 0.129 | 0.029 | * |
| Diagnosed weak/failing kidneys (past 12 months) | 0.011 | 0.013 | | 0.029 | 0.025 | | -0.003 | 0.012 | | 0.017 | 0.020 | | 0.168 | 0.027 | * | 0.126 | 0.054 | * |
| Asthma (time of survey) | 0.027 | 0.006 | * | -0.008 | 0.019 | | 0.041 | 0.006 | * | 0.020 | 0.017 | | 0.051 | 0.011 | * | 0.055 | 0.024 | * |
| Diagnosed emphysema (ever) | 0.026 | 0.018 | | 0.066 | 0.051 | | 0.011 | 0.013 | | -0.115 | 0.081 | | -0.033 | 0.030 | | -0.176 | 0.078 | * |
| Diagnosed chronic bronchitis (past 12 months) | 0.002 | 0.010 | | -0.014 | 0.026 | | 0.019 | 0.008 | * | 0.024 | 0.022 | | 0.015 | 0.017 | | 0.054 | 0.039 | |
| Coronary heart disease (ever) | 0.005 | 0.014 | | 0.033 | 0.026 | | 0.028 | 0.009 | * | 0.027 | 0.021 | | 0.104 | 0.026 | * | 0.078 | 0.052 | |
| Angina pectoris (ever) | -0.029 | 0.020 | | -0.126 | 0.058 | * | -0.016 | 0.016 | | -0.117 | 0.051 | * | 0.047 | 0.031 | | -0.021 | 0.072 | |
| Heart attack (ever) | 0.010 | 0.016 | | 0.033 | 0.027 | | 0.031 | 0.011 | * | -0.005 | 0.029 | | 0.047 | 0.026 | | 0.089 | 0.053 | |
| Other heart condition (ever) | 0.007 | 0.008 | | 0.012 | 0.024 | | 0.020 | 0.007 | * | 0.042 | 0.020 | * | 0.132 | 0.013 | * | 0.134 | 0.032 | * |
| High cholesterol (past 12 months) | 0.046 | 0.004 | * | 0.044 | 0.013 | * | 0.062 | 0.004 | * | 0.037 | 0.013 | * | 0.043 | 0.009 | * | 0.033 | 0.022 | |
| Diagnosed stroke (ever) | -0.009 | 0.014 | | -0.001 | 0.022 | | 0.002 | 0.012 | | -0.009 | 0.026 | | 0.021 | 0.025 | | 0.102 | 0.045 | * |
| Chronic liver condition (ever) | 0.033 | 0.017 | * | 0.111 | 0.053 | * | 0.005 | 0.020 | | 0.056 | 0.045 | | 0.101 | 0.030 | * | 0.081 | 0.085 | |
| Diagnosed hepatitis (ever) | 0.027 | 0.012 | * | -0.013 | 0.034 | | 0.019 | 0.017 | | 0.012 | 0.030 | | 0.019 | 0.021 | | 0.114 | 0.050 | * |
| Diagnosed liver condition (past 12 months) | -0.022 | 0.016 | | 0.032 | 0.052 | | 0.003 | 0.017 | | 0.011 | 0.037 | | 0.048 | 0.026 | | 0.058 | 0.069 | |
| Diagnosed ulcer (ever) | 0.015 | 0.008 | | 0.032 | 0.019 | | 0.010 | 0.008 | | 0.035 | 0.020 | | 0.040 | 0.012 | * | 0.014 | 0.029 | |
| Diagnosed cancer (ever) | 0.019 | 0.006 | * | 0.016 | 0.022 | | 0.037 | 0.006 | * | 0.043 | 0.016 | * | 0.193 | 0.012 | * | 0.201 | 0.047 | * |
| Diagnosed arthritis (ever) | 0.025 | 0.005 | * | 0.043 | 0.013 | * | 0.052 | 0.005 | * | 0.007 | 0.013 | | 0.140 | 0.008 | * | 0.078 | 0.021 | * |

| | Usual Source of Care | | | | | | Any Provider Visit | | | | | | Specialist Visit | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | White | | | Black | | | White | | | Black | | | White | | | Black | | |
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| Education: less than high school | -0.043 | 0.012 | * | -0.019 | 0.027 | | -0.068 | 0.013 | * | -0.067 | 0.026 | * | -0.115 | 0.012 | * | -0.150 | 0.022 | * |
| Education: high school graduate | -0.016 | 0.006 | * | -0.023 | 0.017 | | -0.047 | 0.007 | * | -0.048 | 0.018 | * | -0.081 | 0.008 | * | -0.085 | 0.018 | * |
| Education: some college | 0.006 | 0.005 | | -0.003 | 0.015 | | -0.023 | 0.005 | * | -0.025 | 0.016 | | -0.037 | 0.007 | * | -0.044 | 0.017 | * |
| Works part time | 0.014 | 0.007 | | -0.009 | 0.021 | | 0.007 | 0.008 | | -0.005 | 0.023 | | 0.015 | 0.009 | | 0.033 | 0.020 | |
| Not working | 0.006 | 0.006 | | 0.018 | 0.018 | | 0.026 | 0.006 | * | 0.023 | 0.017 | | 0.022 | 0.008 | * | 0.015 | 0.016 | |
| Rents home | -0.050 | 0.006 | * | -0.038 | 0.015 | * | -0.012 | 0.006 | * | -0.029 | 0.015 | | -0.011 | 0.006 | | -0.004 | 0.013 | |
| Does not rent or own home | 0.000 | 0.013 | | 0.026 | 0.036 | | -0.017 | 0.015 | | 0.019 | 0.031 | | 0.016 | 0.016 | | -0.010 | 0.033 | |
| Family income < 100 FPL | -0.035 | 0.011 | * | -0.083 | 0.024 | * | -0.041 | 0.011 | * | -0.066 | 0.024 | * | -0.066 | 0.011 | * | -0.064 | 0.023 | * |
| Family income 100–200 FPL | -0.036 | 0.010 | * | -0.057 | 0.021 | * | -0.043 | 0.009 | * | -0.039 | 0.021 | | -0.065 | 0.009 | * | -0.040 | 0.020 | * |
| Family income 200–400 FPL | -0.007 | 0.005 | | -0.025 | 0.016 | | -0.027 | 0.006 | * | -0.021 | 0.017 | | -0.037 | 0.007 | * | -0.034 | 0.018 | |
| Received SSI (past 12 months) | 0.053 | 0.012 | * | 0.042 | 0.025 | | 0.027 | 0.012 | * | 0.034 | 0.023 | | 0.029 | 0.022 | | 0.020 | 0.032 | |
| Received public assistance (past 12 months) | 0.077 | 0.021 | * | 0.018 | 0.043 | | 0.045 | 0.021 | * | 0.025 | 0.034 | | 0.003 | 0.036 | | 0.075 | 0.048 | |
| Received SNAP benefits (past 12 months) | 0.008 | 0.011 | | 0.020 | 0.019 | | 0.013 | 0.011 | | 0.033 | 0.018 | | -0.023 | 0.011 | * | -0.008 | 0.015 | |
| Medicare: public hierarchy | 0.011 | 0.011 | | -0.008 | 0.025 | | 0.011 | 0.009 | | -0.006 | 0.021 | | 0.067 | 0.017 | * | 0.065 | 0.035 | |
| Medicaid/CHIP: public hierarchy | -0.004 | 0.011 | | 0.044 | 0.021 | * | -0.009 | 0.011 | | 0.020 | 0.020 | | 0.013 | 0.012 | | 0.006 | 0.019 | |
| Other public: public hierarchy | 0.064 | 0.018 | * | -0.013 | 0.044 | | 0.000 | 0.025 | | 0.029 | 0.040 | | 0.008 | 0.032 | | 0.051 | 0.060 | |
| Exchange: public hierarchy | -0.020 | 0.010 | * | -0.018 | 0.029 | | -0.024 | 0.011 | * | -0.042 | 0.035 | | -0.011 | 0.013 | | -0.017 | 0.030 | |
| Direct purchase: public hierarchy | -0.007 | 0.013 | | -0.081 | 0.094 | | -0.007 | 0.012 | | -0.076 | 0.096 | | -0.016 | 0.015 | | 0.048 | 0.054 | |

| | Usual Source of Care | | | | | | Any Provider Visit | | | | | | Specialist Visit | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | White | | | Black | | | White | | | Black | | | White | | | Black | | |
| | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | | β | SE | |
| Other private: | | | | | | | | | | | | | | | | | | |
| public hierarchy | 0.003 | 0.015 | | -0.049 | 0.060 | | -0.015 | 0.016 | | -0.033 | 0.057 | | 0.027 | 0.022 | | -0.023 | 0.032 | |
| Uninsured | -0.293 | 0.012 | * | -0.319 | 0.025 | * | -0.226 | 0.011 | * | -0.222 | 0.024 | * | -0.103 | 0.009 | * | -0.062 | 0.015 | * |
| Constant | 0.864 | 0.012 | * | 0.892 | 0.031 | * | 0.857 | 0.012 | * | 0.840 | 0.035 | * | 0.204 | 0.013 | * | 0.142 | 0.031 | * |
| **Sample size** | **36,696** | | | **6,161** | | | **36,667** | | | **6,155** | | | **36,689** | | | **6,160** | | |

*Flu vaccine and blood pressure check*

| | Flu Vaccine | | | | | | Blood Pressure Check | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | White | | | Black | | | White | | | Black | | |
| | β | SE | | β | SE | | β | SE | | β | SE | |
| Age 26–34 | 0.012 | 0.012 | | 0.017 | 0.025 | | 0.012 | 0.011 | | -0.013 | 0.024 | |
| Age 35–44 | 0.017 | 0.013 | | -0.008 | 0.026 | | 0.019 | 0.011 | | 0.041 | 0.023 | |
| Age 45–54 | 0.006 | 0.013 | | 0.028 | 0.028 | | 0.022 | 0.011 | | 0.014 | 0.024 | |
| Age 55–64 | 0.084 | 0.014 | * | 0.105 | 0.031 | * | 0.017 | 0.011 | | 0.041 | 0.025 | |
| Female | 0.068 | 0.006 | * | 0.057 | 0.016 | * | 0.087 | 0.005 | * | 0.087 | 0.012 | * |
| Lesbian, gay, or bisexual | 0.059 | 0.016 | * | -0.094 | 0.029 | * | 0.021 | 0.012 | | 0.022 | 0.031 | |
| Noncitizen | -0.055 | 0.024 | * | 0.059 | 0.033 | | -0.018 | 0.020 | | -0.093 | 0.035 | * |
| Lives with partner | -0.042 | 0.011 | * | -0.070 | 0.028 | * | -0.038 | 0.009 | * | -0.016 | 0.024 | |
| Widowed, separated, or divorced | -0.017 | 0.009 | * | -0.034 | 0.022 | | -0.019 | 0.006 | * | -0.023 | 0.015 | |
| Never married | -0.031 | 0.009 | * | -0.062 | 0.020 | * | -0.029 | 0.007 | * | -0.036 | 0.015 | * |
| Midwest | -0.009 | 0.010 | | 0.001 | 0.027 | | -0.015 | 0.006 | * | -0.016 | 0.021 | |
| South | -0.026 | 0.009 | * | -0.029 | 0.023 | | -0.020 | 0.006 | * | -0.034 | 0.018 | |
| West | -0.031 | 0.010 | * | -0.024 | 0.033 | | -0.042 | 0.007 | * | -0.023 | 0.027 | |
| Moderate psychological distress | -0.002 | 0.012 | | 0.022 | 0.029 | | 0.021 | 0.008 | * | 0.043 | 0.020 | * |
| Severe psychological distress | -0.020 | 0.016 | | -0.030 | 0.037 | | 0.040 | 0.010 | * | 0.041 | 0.022 | |
| Good health, self-reported | 0.009 | 0.008 | | -0.009 | 0.017 | | 0.006 | 0.005 | | 0.004 | 0.014 | |
| Fair/poor health, self-reported | -0.005 | 0.013 | | -0.006 | 0.025 | | 0.008 | 0.008 | | 0.012 | 0.018 | |
| Obese | -0.013 | 0.007 | | 0.049 | 0.015 | * | 0.014 | 0.005 | * | 0.003 | 0.012 | |
| Current smoker | -0.093 | 0.008 | * | -0.043 | 0.019 | * | -0.039 | 0.007 | * | 0.002 | 0.016 | |
| Former smoker | -0.018 | 0.008 | * | 0.007 | 0.023 | | 0.008 | 0.005 | | -0.005 | 0.019 | |
| Diagnosed hypertension (past 12 months) | 0.046 | 0.009 | * | 0.028 | 0.020 | | 0.081 | 0.004 | * | 0.087 | 0.010 | * |
| Diagnosed diabetes (ever) | 0.092 | 0.013 | * | 0.062 | 0.027 | * | 0.040 | 0.006 | * | 0.031 | 0.013 | * |
| Diagnosed weak/failing kidneys (past 12 months) | 0.085 | 0.028 | * | 0.140 | 0.054 | * | -0.013 | 0.012 | | 0.029 | 0.020 | |

| | Flu Vaccine | | | | | | Blood Pressure Check | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | White | | | Black | | | White | | | Black | | |
| | β | SE | | β | SE | | β | SE | | β | SE | |
| Asthma (time of survey) | 0.057 | 0.012 | * | 0.060 | 0.025 | * | 0.029 | 0.006 | * | 0.036 | 0.016 | * |
| Diagnosed emphysema (ever) | 0.047 | 0.030 | | -0.081 | 0.067 | | 0.008 | 0.013 | | -0.126 | 0.087 | |
| Diagnosed chronic bronchitis (past 12 months) | 0.006 | 0.017 | | 0.077 | 0.041 | | 0.031 | 0.009 | * | 0.049 | 0.017 | * |
| Coronary heart disease (ever) | 0.026 | 0.026 | | 0.079 | 0.056 | | 0.029 | 0.009 | * | 0.025 | 0.019 | |
| Angina pectoris (ever) | 0.021 | 0.030 | | -0.111 | 0.077 | | -0.014 | 0.017 | | -0.049 | 0.045 | |
| Heart attack (ever) | 0.052 | 0.026 | * | -0.142 | 0.048 | * | 0.020 | 0.013 | | -0.033 | 0.030 | |
| Other heart condition (ever) | 0.028 | 0.013 | * | 0.081 | 0.034 | * | 0.009 | 0.008 | | 0.024 | 0.021 | |
| High cholesterol (past 12 months) | 0.071 | 0.009 | * | 0.068 | 0.025 | * | 0.059 | 0.004 | * | 0.047 | 0.010 | * |
| Diagnosed stroke (ever) | 0.042 | 0.025 | | 0.048 | 0.043 | | 0.011 | 0.012 | | 0.037 | 0.019 | |
| Chronic liver condition (ever) | 0.100 | 0.032 | * | 0.047 | 0.101 | | 0.003 | 0.017 | | 0.102 | 0.045 | * |
| Diagnosed hepatitis (ever) | -0.001 | 0.022 | | 0.014 | 0.058 | | 0.017 | 0.016 | | -0.047 | 0.038 | |
| Diagnosed liver condition (past 12 months) | -0.055 | 0.028 | * | -0.074 | 0.073 | | 0.024 | 0.014 | | 0.022 | 0.048 | |
| Diagnosed ulcer (ever) | 0.001 | 0.013 | | -0.019 | 0.033 | | 0.012 | 0.008 | | 0.051 | 0.019 | * |
| Diagnosed cancer (ever) | 0.040 | 0.012 | * | 0.063 | 0.044 | | 0.035 | 0.005 | * | -0.010 | 0.033 | |
| Diagnosed arthritis (ever) | 0.039 | 0.008 | * | 0.044 | 0.022 | * | 0.052 | 0.005 | * | 0.017 | 0.014 | |
| Education: less than high school | -0.136 | 0.014 | * | -0.083 | 0.028 | * | -0.074 | 0.013 | * | -0.096 | 0.024 | * |
| Education: high school graduate | -0.138 | 0.009 | * | -0.086 | 0.023 | * | -0.043 | 0.007 | * | -0.054 | 0.017 | * |
| Education: some college | -0.088 | 0.008 | * | -0.058 | 0.020 | * | -0.018 | 0.005 | * | -0.013 | 0.015 | |
| Works part time | -0.003 | 0.011 | | 0.007 | 0.025 | | 0.002 | 0.008 | | 0.017 | 0.022 | |
| Not working | 0.009 | 0.009 | | 0.014 | 0.020 | | 0.010 | 0.007 | | -0.004 | 0.018 | |
| Rents home | -0.013 | 0.008 | | -0.012 | 0.017 | | -0.005 | 0.006 | | -0.028 | 0.014 | * |
| Does not rent or own home | -0.007 | 0.018 | | 0.103 | 0.047 | * | -0.012 | 0.015 | | -0.009 | 0.033 | |
| Family income < 100% of FPL | -0.030 | 0.013 | * | 0.031 | 0.029 | | -0.030 | 0.011 | * | -0.065 | 0.023 | * |
| Family income 100–200% of FPL | -0.040 | 0.011 | * | 0.009 | 0.024 | | -0.041 | 0.009 | * | -0.036 | 0.020 | |
| Family income 200–400% of FPL | -0.029 | 0.008 | * | 0.014 | 0.021 | | -0.030 | 0.006 | * | -0.016 | 0.016 | |
| Received SSI (past 12 months) | 0.041 | 0.024 | | 0.040 | 0.037 | | 0.036 | 0.012 | * | 0.030 | 0.028 | |
| Received public assistance (past 12 months) | 0.112 | 0.043 | * | -0.025 | 0.047 | | 0.051 | 0.022 | * | 0.042 | 0.028 | |
| Received SNAP benefits (past12 months) | 0.017 | 0.013 | | 0.007 | 0.021 | | 0.008 | 0.011 | | 0.021 | 0.017 | |
| Medicare: public hierarchy | 0.033 | 0.018 | | -0.008 | 0.035 | | 0.005 | 0.009 | | -0.004 | 0.023 | |
| Medicaid/CHIP: public hierarchy | -0.059 | 0.014 | * | -0.022 | 0.026 | | -0.017 | 0.011 | | -0.003 | 0.021 | |
| Other public: public hierarchy | 0.025 | 0.043 | | 0.039 | 0.069 | | 0.030 | 0.021 | | 0.044 | 0.040 | |
| Exchange: public hierarchy | -0.106 | 0.014 | * | -0.102 | 0.038 | * | -0.022 | 0.011 | * | -0.044 | 0.033 | |
| Direct purchase: public hierarchy | -0.090 | 0.018 | * | -0.130 | 0.052 | * | -0.020 | 0.013 | | 0.042 | 0.064 | |
| Other private: public hierarchy | 0.013 | 0.024 | | -0.035 | 0.059 | | -0.001 | 0.015 | | 0.055 | 0.048 | |
| Uninsured | -0.191 | 0.010 | * | -0.125 | 0.021 | * | -0.221 | 0.011 | * | -0.210 | 0.023 | * |
| Constant | 0.446 | 0.016 | * | 0.314 | 0.039 | * | 0.832 | 0.012 | * | 0.876 | 0.031 | * |

| | Flu Vaccine | | | | Blood Pressure Check | | | |
|---|---|---|---|---|---|---|---|---|
| | White | | Black | | White | | Black | |
| | β | SE | β | SE | β | SE | β | SE |
| **Sample size** | **36,607** | | **6,155** | | **36,690** | | **6,158** | |

**Source:** Authors' analysis of the 2016–18 National Health Interview Survey.

**Notes:** β = regression coefficient; SE = standard error of regression coefficient; FPL = federal poverty level; SSI = Supplemental Security Income; SNAP = Supplemental Nutrition Assistance Program; CHIP = Children's Health Insurance Program.

* *p*-value < 0.05 on a two-tailed test.

APPENDIX TABLE A.8

**Detailed Decomposition Results**

*Usual source of care, any provider visit, and specialist visit*

| | Usual Source of Care | | | | | | Any Provider Visit | | | | | | Specialist Visit | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Separate regressions | | | Single regression | | | Separate regressions | | | Single regression | | | Separate regressions | | | Single regression | | |
| | Est. | SE | | Est. | SE | | Est. | SE | | Est. | SE | | Est. | SE | | Est. | SE | |
| White mean | 0.860 | 0.002 | | 0.860 | 0.002 | | 0.849 | 0.002 | | 0.849 | 0.002 | | 0.279 | 0.003 | | 0.279 | 0.003 | |
| Black mean | 0.825 | 0.007 | | 0.825 | 0.007 | | 0.837 | 0.006 | | 0.837 | 0.006 | | 0.190 | 0.006 | | 0.190 | 0.006 | |
| Total difference | 0.035 | 0.007 | * | 0.035 | 0.007 | * | 0.012 | 0.007 | | 0.012 | 0.007 | | 0.089 | 0.007 | * | 0.089 | 0.007 | * |
| IOM disparity | 0.031 | 0.007 | * | 0.037 | 0.007 | * | 0.024 | 0.008 | * | 0.021 | 0.007 | * | 0.091 | 0.007 | * | 0.085 | 0.007 | * |
| Difference explained by observed characteristics | 0.045 | 0.008 | * | 0.043 | 0.004 | * | 0.023 | 0.008 | * | 0.025 | 0.004 | * | 0.027 | 0.008 | * | 0.043 | 0.004 | * |
| Age/sex/health | 0.005 | 0.004 | | -0.002 | 0.002 | | -0.012 | 0.004 | * | -0.009 | 0.002 | * | -0.002 | 0.005 | | 0.004 | 0.003 | |
| Coverage | 0.011 | 0.004 | * | 0.014 | 0.002 | * | 0.008 | 0.004 | * | 0.012 | 0.002 | * | 0.002 | 0.003 | | 0.002 | 0.002 | |
| Other SES | 0.029 | 0.008 | * | 0.031 | 0.003 | * | 0.027 | 0.008 | * | 0.022 | 0.003 | * | 0.026 | 0.008 | * | 0.036 | 0.003 | * |
| Unexplained difference | -0.010 | 0.009 | | -0.008 | 0.007 | | -0.011 | 0.010 | | -0.013 | 0.007 | | 0.062 | 0.011 | * | 0.046 | 0.007 | * |
| Explained by coefficients | -0.008 | 0.007 | | | | | -0.012 | 0.007 | | | | | 0.043 | 0.007 | * | | | |
| Explained by interaction | -0.002 | 0.008 | | | | | 0.001 | 0.008 | | | | | 0.020 | 0.009 | * | | | |
| **Sample size** | | | | **42,857** | | | | | | **42,822** | | | | | | **42,849** | | |
| **Sample size (white)** | **36,696** | | | | | | **36,667** | | | | | | **36,689** | | | | | |
| **Sample size (Black)** | **6,161** | | | | | | **6,155** | | | | | | **6,160** | | | | | |

*Flu vaccine and blood pressure check*

| | Flu Vaccine | | | | | | Blood Pressure Check | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Separate regressions | | | Single regression | | | Separate regressions | | | Single regression | |
| | **Est.** | **SE** | | **Est.** | **SE** | | **Est.** | **SE** | | **Est.** | **SE** |
| White mean | 0.393 | 0.003 | | 0.393 | 0.003 | | 0.856 | 0.002 | | 0.856 | 0.002 |
| Black mean | 0.305 | 0.007 | | 0.305 | 0.007 | | 0.845 | 0.006 | | 0.845 | 0.006 |
| Total difference | 0.088 | 0.008 | * | 0.088 | 0.008 | * | 0.011 | 0.007 | | 0.011 | 0.007 |
| IOM disparity | 0.087 | 0.009 | * | 0.089 | 0.008 | * | 0.019 | 0.008 | * | 0.017 | 0.007 |
| Difference explained by observed characteristics | 0.030 | 0.010 | * | 0.043 | 0.005 | * | 0.046 | 0.007 | * | 0.031 | 0.004 |
| Age/sex/health | 0.001 | 0.005 | | -0.001 | 0.002 | | -0.007 | 0.004 | | -0.005 | 0.002 |
| Coverage | 0.005 | 0.004 | | 0.011 | 0.002 | * | 0.012 | 0.004 | * | 0.013 | 0.002 |
| Other SES | 0.023 | 0.009 | * | 0.033 | 0.004 | * | 0.042 | 0.007 | * | 0.024 | 0.003 |
| Unexplained difference | 0.058 | 0.013 | * | 0.044 | 0.009 | * | -0.035 | 0.009 | * | -0.020 | 0.007 |
| Explained by coefficients | 0.040 | 0.009 | * | | | | -0.015 | 0.007 | * | | |
| Explained by interaction | 0.018 | 0.010 | | | | | -0.020 | 0.007 | * | | |
| **Sample size** | | | | **42,762** | | | | | | **42,848** | |
| **Sample size (white)** | **36,607** | | | | | | **36,690** | | | | |
| **Sample size (Black)** | **6,155** | | | | | | **6,158** | | | | |

**Source:** Authors' analysis of the 2016–18 National Health Interview Survey

**Notes:** Est. = estimate; SE = standard error of estimate; IOM = Institute of Medicine; SES = socioeconomic status.

* *p*-value < 0.05 on a two-tailed test.

# Notes

1 "Shared Terms," University of Minnesota School of Public Health, Center for Antiracism Research for Health Equity, accessed December 7, 2022, https://carhe.umn.edu/our-work/shared-terms.

2 In yet another recent study using the residual direct effect approach, estimates of racial disparities in self-reported heart failure rates included covariate controls for education, which the authors used as a proxy for socioeconomic status. The study found that racial disparities were unchanged over the study period without explanation of what the disparity estimate measured (Rethy et al. 2020).

3 "Combahee River Collective Statement," Combahee River Collective, accessed December 7, 2022, https://americanstudies.yale.edu/sites/default/files/files/Keyword%20Coalition_Readings.pdf; and "Social Determinants of Health: Healthy People 2030," US DHHS OASH, published in 2022, https://health.gov/healthypeople/priority-areas/social-determinants-health.

4 Provisional Guidance on the Implementation of the 1997 Standards for Federal Data on Race and Ethnicity, 66 Fed. Reg. 3830 (Jan. 16, 2001).

5 See Ellen and Steil (2019) for discussion of policies to potentially disrupt factors that sustain residential segregation and its consequences for health.

6 "IPUMS Health Surveys," IPUMS, accessed December 7, 2022, https://healthsurveys.ipums.org/.

7 For both the single and group-specific full regression approaches, we use the oaxaca command in Stata to decompose the components of the total difference and produce standard errors for the difference explained by all observed characteristics and the specified components (age, sex, health, insurance coverage, and socioeconomic status). We then calculate the IOM disparity and its standard error using the lincom command to subtract the estimated difference due to age, sex, and health from the total difference.

8 "Measures to Advance Health and Opportunity," HOPE Initiative, accessed December 7, 2022, https://www.hopeinitiative.org/.

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# About the Authors

**Lisa Clemans-Cope** is a health economist and senior research fellow in the Health Policy Center at the Urban Institute. Her areas of expertise include substance use disorder and opioid use disorder and treatment, health spending, access to and use of health care, private insurance, Medicaid, the Children's Health Insurance Program, Medicare, dual Medicare and Medicaid enrollees, health reform legislation and regulation, and health-related survey and administrative data. She has led qualitative and quantitative research projects examining the impacts of policies to integrate physical, behavioral, and substance use treatment; assessing treatment interventions for opioid use disorder; and assessing policies to increase access to treatment. Her research includes analyses of the Affordable Care Act, Medicaid program costs and quality, hospital costs under Medicaid and Medicare, access to care under Medicare, and private health insurance. Clemans-Cope has published her research in the *New England Journal of Medicine*, *Health Affairs*, *Pediatrics*, and *Inquiry*. Her work has been widely cited in the media, including the *New York Times*, *Wall Street Journal*, *Los Angeles Times*, Bloomberg, *Forbes*, *National Journal*, FactCheck.org, Huffington Post, *Incidental Economist*, and *Modern Healthcare*. She has appeared on NPR and Fox News. Clemans-Cope has a BA in economics from Princeton University and a PhD in health economics from the Johns Hopkins Bloomberg School of Public Health.

**Bowen Garrett** is an economist and senior fellow in the Health Policy Center. His research focuses on health reform and health policy topics, including health insurance and labor markets and Medicare financing. He leads the development of the Urban Institute's Medicare policy simulation model (MCARE-SIM). Previously, Garrett was chief economist of the Center for US Health System Reform and McKinsey Advanced Health Analytics at McKinsey and Company. Garrett received his PhD in economics from Columbia University and was a postdoctoral research fellow in the Robert Wood Johnson Foundation's Scholars in Health Policy Research Program at the University of California, Berkeley.

**Stacey McMorrow** is a principal research associate with extensive experience using quantitative methods to study the factors that affect individual health insurance coverage and access to care as well as the impacts of state and national health reforms on employers and individuals. Her current work uses the Affordable Care Act and past Medicaid expansions to explore the effects of expanding insurance coverage on access to care, service use, and health outcomes for various populations. Through this and other work, McMorrow has substantial expertise in analyzing data from several federal surveys, including the NHIS and the Medical Expenditure Panel Survey. Other research interests include the role of community health centers and safety net providers under health reform, receipt of preventive and reproductive health services among women, barriers to care for low-income children, and the market-level effects of insurance expansions. McMorrow received her PhD in health economics from the University of Pennsylvania in 2009.

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