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RESEARCH ARTICLE

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Effect of mental health staffing inputs on suicide-related events

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Abstract

Objective: To estimate the effects of changes in Veterans Health Administration (VHA) mental health services staffing levels on suicide-related events among a cohort of Veterans.

Data Sources: Data were obtained from the VHA Corporate Data Warehouse, the Department of Defense and Veterans Administration Infrastructure for Clinical Intelligence, the VHA survey of enrollees, and customized VHA databases tracking suicide-related events. Geographic variables were obtained from the Area Health Resources Files and the Centers for Medicare and Medicaid Services.

Study Design: We used an instrumental variables (IV) design with a Heckman correction for non-random partial observability of the use of mental health services. The principal predictor was a measure of provider staffing per 10,000 enrollees. The outcome was the probability of a suicide-related event.

Data Collection/Extraction Methods: Data were obtained for a cohort of Veterans who recently separated from active service.

Principal Findings: From 2014 to 2018, the per-pay period probability of a suicide-related event among our cohort was 0.05%. We found that a 1% increase in mental health staffing led to a 1.6 percentage point reduction in suicide-related events. This was driven by the first tertile of staffing, suggesting diminishing returns to scale for mental health staffing.

Conclusions: VHA facilities appear to be staffing-constrained when providing mental health care. Targeted increases in mental health staffing would be likely to reduce suicidality.

KEYWORDS
access to care, mental health services, resource allocation, suicide, Veterans, workforce

What is known on this topic

- Rising suicide rates and dramatic provider shortages have led some to hypothesize that mental health provider staffing could have a direct impact on suicide rates.
- There is mixed evidence on the effects of mental health staffing on suicide-related outcomes.

- Existing observational work does not account for confounding and may use incomplete measures of staffing inputs.

What this study adds

- Using the VHA's rich administrative data and exogenous variation in mental health provider staffing, this study measures the effect of staffing on suicide-related events.
- The approach can be replicated by other health systems to identify potential interventions to improve mental health outcomes among patients.

1 | INTRODUCTION

In 2018, suicide was the tenth leading cause of death in the US and the second leading cause of death for people between 10 and 34 years old. Suicide rates have been increasing for two decades, alongside a national shortage of mental health professionals.[1,2] Over 75% of US counties have a mental health provider shortage, with over half of needs unmet. Similarly, more than 90% of Federally Qualified Health Centers are estimated to be incapable of meeting patients' mental health needs.[3] These dramatic provider shortages have led some to hypothesize that mental health provider staffing may have a direct impact on suicide rates.

The empirical literature examining the relationship between mental health staffing and health outcomes is limited. Existing evidence uses incomplete measures of staffing and relies on observational methods that do not account for confounding. In work focused on the Veterans Health Administration (VHA), Levine et al. found mixed evidence: while there was no relationship between staffing levels and depression outcomes, there was a positive relationship between staffing and the receipt of psychotherapy. These findings flipped for nurse-specific staffing, however, suggesting complex mechanisms that were not fully accounted for.[4] Also studying the VHA, Boden et al.[5] found positive relationships between mental health staffing ratios and composite outcomes for mental health, while Katz et al. found that VHA networks that increased mental health staffing from 2005 to 2009 saw the largest reduction in suicide among Veterans.[6]

An important limitation of existing work is the observational nature of the research. Indeed, a systematic review of evidence on staffing levels and patient outcomes noted that existing evidence was largely weak, did not include experimental or quasi-experimental evidence, and should not be used to inform policy or clinical practice.[7]

Estimating the direct relationship between mental health staffing and suicide rests largely on the ability to separate causal influences from confounding factors, which is a notoriously difficult task. Two concerns are particularly important. First, individuals who do not use health care services, or do so at a lower rate than others, may experience negative outcomes without those outcomes being recorded by the health care system. This "partial observability" will lead to an undercount of total suicide-related events. Second, mental health staffing is likely related to underlying needs, which may lead to increased staffing. The potential for reverse causality implies that a naïve regression of negative outcomes on staffing will be biased towards the null or may even provide an estimate with the wrong sign.

The aim of this study is to determine whether changes in facility-level mental health staffing have a causal effect on individual suicide-related events, and—if they do—to investigate how the nature of that relationship varies by the size of the facility (as measured by the amount of staffing relative to enrollees). This paper contributes to the existing literature in multiple ways. First, we address identification challenges by relying on quasi-random variation in exposure to mental health staffing through an instrumental variables (IV) identification strategy using provider staffing data from 125 VHA facilities and suicide-related events data for over 100,000 US Veterans. The second contribution of this study is to measure mental health staffing more accurately. Notably, much of the existing research on mental health staffing and patient outcomes relies on simple staffing ratios to measure mental health staffing. This may lead to measurement error if staffing ratios do not adequately capture the amount of service actually provided to patients. Our approach uses scheduling data to identify hours during the day that providers are actually available and providing care to patients, rather than payroll-based measures that do not account for time actually in clinic.

Lastly, the VHA provides an important setting for examining the relationship between mental health provider staffing and outcomes. This is because the VHA has a substantial number of mental health providers already (over 14% of clinical providers are psychiatrists or psychologists) and because Veterans tend to be overrepresented among suicides.[8] Understanding the direct impact of changes in mental health provider staffing in the VHA will be critical as policy makers and health practitioners aim to set optimal staffing levels and ultimately reduce the incidence of suicide in patient populations more generally.

2 | DATA AND METHODS

2.1 | Study design and cohort

We used a panel study design over a 5-year (2014–2018) period among a cohort of US Veterans separating from active duty ($N = 109{,}376$). Veterans were eligible for inclusion in the analytic cohort if they separated from active duty between 2010 and 2017. These restrictions were imposed based on data available from the

Department of Defense (DoD). Data were aggregated to a two-week facility-level pay period.

2.2 | Data sources

The data used in the analyses were obtained from multiple administrative datasets. The cohort was identified through the DoD and Veterans Administration (VA) Infrastructure for Clinical Intelligence, an electronic network linking the two agencies.[9] Patient-level demographics, health care utilization, comorbidities, and suicide-related outcomes were obtained from the VHA Corporate Data Warehouse (CDW). In addition, CDW data were used to measure facility-level characteristics. We used the VHA Survey of Enrollees[10] to obtain market-level data on a range of potential confounders, including rates of insurance coverage and employment among Veterans. Data on Medicare Advantage market penetration were obtained via the Centers for Medicare and Medicaid Services, and data on population density and the availability of non-VHA care in a given market were obtained from the Area Health Resources Files.

2.3 | Measures

2.3.1 | Outcome: Suicide-related events

The outcome was a dichotomous variable indicating whether a Veteran experienced a suicide-related event (SRE) in each two-week pay period between 2014 and 2017. SREs included suicidal self-directed violence, suicide attempts, suicide, and self-directed violence with undetermined intent. We identified SREs through the VA-Suicide Prevention and Application Network, the VA Comprehensive Suicide Risk Evaluation, and the Suicide Behavior and Overdose Report. These data are collected by the VHA's Program Evaluation and Resource Center and the Office of Mental Health and Suicide Prevention and are based on notes in a patient's record from clinicians and suicide prevention coordinators. This chart review approach is likely to more reliably capture suicide-related events compared to ICD-10 codes.[11,12]

2.3.2 | Independent variable: Mental health clinic staffing

The key independent variable was the facility-level mental health provider staffing level in a given pay period. This was defined as the amount of time between a clinician's first and last mental health visit each day, summed over the pay period. Work performed by providers was only included if it was performed in a mental health stop code (the VHA's categorization of clinical services provided). Work included was classified as therapy, care coordination, medication management, or pharmacy.

2.3.3 | Covariates

We controlled for multiple time-varying and time-invariant individual characteristics. These included race, age, sex, marital status, branch of service, year of separation from service, number of dependents, as well as the past volume of mental health service use during the Veteran's active-duty service. A binary indicator for the Veteran's use of VHA mental health services ("engagement") was created at the pay period level. Engagement was defined based on whether the Veteran used any VHA mental health services within a two-week span. Lastly, we obtained a measure of Veteran-specific drive distance from the nearest VHA primary care facility.

We also included facility and market characteristics in our models that may have influenced the relationship between facility mental health staffing and patient outcomes. These included the population density per square mile, average Medicare Advantage penetration, and insurance coverage and income among Veterans in the facility's catchment area. Lastly, we measured the average health status among a facility's patient population using the Nosos risk score.[13] Market-level characteristics (such as those measured at the county level) were aggregated to a facility-year-level using enrollee-weighted averages and were linearly interpolated to the pay period level. Individual-level measures from the Survey of Enrollees were similarly aggregated. We use enrollee-weighted averages to account for the fact that facilities draw different shares of their enrollment from different geographies.

2.4 | Instrumental variables approach

The direct relationship between the probability of an SRE and facility-level mental health staffing is subject to three potential endogeneity concerns. First, pre-existing mental health needs are likely to predict the future use of mental health services. Not accounting for baseline mental health status could lead to selection bias. Second, if facilities adjust provider supply to reflect changes in the needs of their patient population (reverse causality), an increase in SREs may spur a facility to increase staffing. This confounding would, at best, bias an uncorrected analysis towards the null, or at worst, result in coefficients with the wrong signs. The third concern is the partial observability of SRE outcomes. While we rely on system-wide administrative databases to detect documented SREs, some instances of SREs will be undocumented in the data if the Veteran did not use VHA services at the time.

Our study attempts to address these concerns in several ways. First, to address selection bias, our cohort of interest was defined based on recently separated Veterans. We focus on a recently separated sample of Veterans in order to better account for simultaneity between mental health diagnosis and mental health utilization. Because we have DoD data on the mental health utilization of recently separated Veterans, we are able to partly account for pre-existing mental health diagnoses. This variable is included as a categorical covariate in all specifications.

To address issues of reverse causality, we used an instrumental variables (IV) approach. Valid instruments must be strongly correlated with the endogenous explanatory variables (staffing and VHA services utilization) but not have a direct effect on patient outcomes (SREs). We used three distinct instruments: (1) clinic-specific vacation time, (2) sick leave for mental health providers, and (3) federal holidays, all of which vary by pay period and have been previously used as instruments for staffing.[14] Variation in vacation time and sick leave at the facility level is driven by provider-level illness and decisions about vacation timing. Similarly, while federal holidays are anticipated, it is unlikely that schedulers can shift around patient burdens to fully match the change in supply. Indeed, in prior work, researchers have cited discussions with providers that vacation time does not need to be negotiated with clinic managers (a necessary condition for being able to plan staffing more smoothly around vacation time). Additionally, variation in vacation timing appears to be related to school vacations, while health care utilization and staffing do not fluctuate in the same way.

Lastly, we address the issue of partial observability of the outcome by relying on an additional instrument: individual-level distance from any VHA facility providing primary care. To do so, we predict engagement with VHA mental health services by adding distance from the facility as a predictor. We exclude distance in our outcome model but include the predicted residual from this specification. This helps to account for the non-random use of VHA services, similar to the Heckman correction.[15]

We use an IV Probit estimator with standard errors clustered at the individual level to account for repeated observations, relying on maximum likelihood estimation. All models include facility, year, and quarter fixed effects to account for time-invariant confounding, seasonality, and secular trends. While maximum likelihood estimators may be problematic in the presence of fixed effects, this appears to be less of a concern with greater than 20 time periods (our specification includes 100 pay periods).[16] Our equations are as follows:

$$\text{VHAUse}_{it} = \beta_0 + \phi \text{Distance}_{it} + X_{ft} + Z_{it} + \mathrm{T} + Q + F + \varepsilon_{ift}, \qquad (1)$$

$$\text{Staffing}_{ft} = \tau_0 + \Omega_n \text{Instruments}_{ft} + X_{ft} + Z_{it} + \mathrm{T} + Q + F + \mu_{ift}, \qquad (2)$$

$$\Pr(\text{SRE}_{it}) = f\left(\text{Staffing}_{ft}, \widehat{\varepsilon_{ift}}, X_{ft}, Z_{it}, \mathrm{T}, Q, F\right), \qquad (3)$$

where i indexes an individual, t indexes a pay period, and f indexes a facility. VHAUse is a binary indicator of whether an individual used VHA mental health care in a given pay period, Capacity is a measure of facility-level staffing in a given pay period as discussed earlier, and SRE is a binary indicator of whether an individual had an SRE in a given pay period. X represents a vector of time-varying facility/market-level characteristics, Z represents a vector of time-varying individual characteristics, T is a vector of year fixed effects, Q is a vector of quarter-fixed effects, and F is a vector of facility fixed effects. ε, μ, and η are serially correlated error terms.

Equation (1) generates a residualized measure of VHA service use with distance as an instrument. Note that this approximates the inverse mills' ratio in a Probit setting and, by extension, in an OLS setting.[17] Equation (2) generates a predicted value of facility-level staffing using the instruments described earlier. Lastly, Equation (3) is the Probit estimate of the effect of changes in staffing on the probability of an SRE. Our primary coefficient of interest is on Staffing, which represents the effect of a change in staffing on the probability of an SRE.

Lastly, we hypothesized that there may be differential returns to scale. An increase in staffing at a facility with low staffing might have a different effect on outcomes than an increase in a facility with high staffing. To investigate, we categorized facilities into tertiles based on their per-enrollee staffing in a given pay period and stratified Equations (1)–(3) by each tertile. We used contemporaneous tertile categorization because we expected that contemporaneous measures of staffing are most likely to affect mental health outcomes, particularly because of the relatively high frequency with which we measure staffing.

2.5 | Sensitivity analyses

We examined several variations of our primary specification. First, we lag the staffing variable in an endogenous Probit specification to examine whether endogeneity may be addressed by breaking the simultaneity of outcomes and predictors. We considered the same tagged version IV Probit specification. Additionally, because end-of-year leave may occur in December and January, we tested the validity of our instruments by adding calendar month fixed effects as well.

All analyses were performed using Stata MP version 16.1. This work was intended to support internal operations efforts at the VHA. It was classified as non-research by the Quality Enhancement Research Initiative per Office of Research and Development policy 1200.21 and was exempt from review by the institutional review board.

3 | RESULTS

The final analytic sample follows 109,367 unique patients across 125 VHA medical centers over 5 years (July 2014–June 2018) and 100 pay periods, for a total of 8,911,906 observations at the individual-pay period level. Table 1 presents summary statistics for key characteristics of patients and facilities. The majority of the Veterans in the sample came from the Army branch (66%), had four or more mental health visits during their service (73%), and had at least one dependent (73%). Across the full sample, the per pay period probability of an SRE occurring was relatively low at 0.05%. There were few differences in characteristics across tertiles (Table A1).

Our sample of Veterans had a higher prevalence of service-related disabilities compared to the general Veteran population. Those in priority groups 1–3 (a VA classification for determining benefit eligibility that indicates a compensable service-related disability) represent 60.1% of our sample, compared to 46.9% among enrolled Veterans at the end of 2018.

TABLE 1 Sample characteristics (across all time periods)

Individual characteristics		
SRE (%)		0.05 (0.2)
Drive distance to primary care (miles)		14.17 (12.9)
Race	White	69.1%
	Black or African American	20.5%
	American Indian or Alaska Native	1.5%
	Asian or Pacific Islander	5.7%
	Other	3.3%
Age	≤25 yrs	21.3%
	26–29 yrs	27.2%
	30–39 yrs	26.7%
	≥40 yrs	24.8%
Female		18.1%
Service branch	Army	66.0%
	Coast guard	0.9%
	Air force	10.7%
	PHS corps	<0.01%
	Marine corps	12.1%
	Navy	10.3%
	NOAA	<0.01%
Married		68.2%
DOD MH utilization	≤3 visits	26.90%
	3–7 visits	23.90%
	8–15 visits	25.50%
	≥16 visits	23.60%
Dependents	No dependents	26.70%
	1 dependent	38.50%
	2–3 dependents	18.20%
	≥4 dependents	16.70%
Facility characteristics		
Population density (per sqm)		843.3 (802.7)
MA penetration (%)		31.35 (11.27)
Insurance coverage	Comprehensive coverage	60.0%
	Medicaid	7.0%
	Non-comprehensive coverage	23.2%
	Missing insurance	0.2%
Employment	FT employment	18.0%
	Unemployed	3.0%
Income	Income < 20 k	21.1%
	Income 20–50 k	37.3%
	Income 50–75 k	11.8%
	Income > 75 k	14.8%
	Income Missing	15.0%

(Continues)

TABLE 1 (Continued)

Facility characteristics	
Mental health provider staffing per 10 k enrollees	7.6 (1.9)
Annual leave (% of hours)	6.8
Sick leave (% of hours)	3.6
Holidays (% of hours)	2.8

Note: Years include 2014–2018. Total number of patients: 109,367. Standard deviations are in parentheses. Total number of facilities: 125. Insurance coverage, employment, and income are facility-month variables. $N = 8{,}911{,}906$ individual-pay period observations.
Abbreviations: DOD, department of defense; FT, full-time; MA, medicare advantage; NOAA, National Oceanic and Atmospheric Administration; PHS, public health service; SRE, suicide related event.

TABLE 2 Impact of an increase in mental health provider staffing on the probability of suicide-related events: results from Probit and IV Probit models

Variables	Probit	IV
Mental health provider staffing (per 10 k enrollees)		
Coefficient	−0.03 (0.0614)	−0.06 (0.008)
Marginal effect (change in probability)	−0.00006	−0.0001
Percent reduction in probability of an SRE for a one unit increase	11.4%	21.4%
Elasticity	0.9	1.6
Individual covariates	x	x
Facility covariates	x	x
Facility fixed effects	x	x
Quarter (calendar) fixed effects	x	x
First-stage F-statistic	–	1,394,019

Note: Robust standard errors clustered at individual level. Endogenous variable is mental health provider staffing per 10,000 enrollees. Outcome is a suicide-related event. Olea-Pflueger F-statistic is reported. Models are specified as described in Equations (1)–(3).

At the facility-pay period level, the average number of standardized clinic days per 10,000 enrollees was 7.6 (SD: 1.9). There was substantial geographic variation in staffing levels across the facilities in our sample. In 2018, for instance, average mental health provider staffing ranged from 4.7 standardized clinic days per pay period per 10,000 enrollees to 21.4 standardized clinic days (Figure A1). There was a similar variation in engagement with VHA services. In 2018, average engagement ranged from virtually no engagement (0%) to 45.5% by the facility-pay period (Figure A2).

3.1 | Naïve Probit estimates

Estimating a naïve Probit model without accounting for the endogeneity of SREs and facility staffing, we found a small, negative

TABLE 3 Impact of annual leave, holidays, and sick leave on mental health staffing: results from first stage models

Endogenous variable	Instrument	Coefficient	SE	F-statistic
Mental health staffing per 10 k enrollees	Annual leave	−11.6	0.01	1,394,019
	Holidays	−12.6	0.009	
	Sick leave	−9.4	0.04	

Note: First stage from IV Probit specification with robust standard errors clustered at individual level. Olea-Pflueger F-statistic is reported.

TABLE 4 Impact of an increase in mental health provider staffing on the probability of suicide-related events, stratified by tertiles of staffing

Variables	Probit coefficient	Marginal effect	Percent reduction in probability of an SRE for a one unit increase	Elasticity	First stage F-stat
Staffing T1	−0.134 (0.019)	−0.0002	44.0%	2.6	1,002,654
Staffing T2	−0.06 (0.04)	−0.0001	19.8%	1.5	192,183
Staffing T3	−0.03 (0.02)	−0.00005	10.3%	1.0	127,778

Note: Results from Probit IV model in Table 2 are stratified by tertile of mental health staffing. All models controlled for individual covariates, facility covariates, facility fixed effects, and quarter (calendar) fixed effects. Percent reduction in the probability of an SRE is calculated as the coefficient divided by the probability of an SRE across the entire sample. Marginal effect represents the predicted change in probability of the outcome. Olea-Pflueger F-statistic is reported.
Abbreviations: T1, tertile 1; T2, tertile 2; T3, tertile 3.

but statistically significant Probit coefficient (0.03, SE: 0.006). The average marginal effects implied a 0.8% reduction in SREs for a 1% increase in staffing or a 10.4% reduction in SREs for a one-unit increase in staffing. (Table 2).

3.2 | IV Probit estimates

We expected selection bias, reverse causality, and partial observability to all bias the naïve Probit results towards the null. We addressed selection in all models by including DoD mental health utilization during a Veteran's active duty as an additional control.

Addressing reverse causality with respect to staffing and SREs similarly requires a strong instrument. The Olea-Pflueger first-stage F-statistic on all excluded instruments is 1,002,654. This is substantially greater than the Olea-Pflueger critical value of 31.5, for a maximum bias of 5%.[18] (Table 3) Similarly, addressing partial observability requires that driving distance from a facility be strongly associated with the use of VHA mental health services. The estimation results for this first stage show a strong relationship: a one-mile increase in drive distance from a facility is associated with a 0.007 (SE: 0.0009) percentage point reduction in the probability of using VHA mental health services.

Our IV Probit coefficient was −0.06 (SE: 0.008), roughly twice our naïve estimate and consistent with our assumptions regarding the direction of bias. Average marginal effects indicated that this corresponded to a 1.6% reduction in the probability of an SRE for a 1% increase in staffing, or a 21.4% reduction in the probability of an SRE for a one-unit increase in staffing. Notably, these results suggest that SREs are elastic with respect to staffing, while the naïve estimates indicate that SREs are inelastic.

To estimate whether these effects vary by the level of staffing, we stratified our models by tertiles of staffing (Table 4). We found that the largest effect on SREs was in the first tertile – a 44% relative reduction for a one-unit increase in staffing or a 2.6% reduction in the probability of an SRE for a 1% increase in staffing. We found directionally declining effects in the second and third tertiles (reductions of −1.45% and −0.98%, respectively, for a 1% increase in staffing), but these results were not statistically significant.

3.3 | Sensitivity analyses

Models with lagged measures of staffing by a single pay period—both in IV Probit and naïve Probit—yielded nonsignificant estimated effects of staffing on SREs, with marginal effects implying a reduction of 0.03% or an increase of 0.08% in the probability of an SRE for a 1% increase in staffing.

The inclusion of month-fixed effects led to a slightly smaller Probit coefficient (−0.0.05, SE:0.009) that was similar in magnitude and statistical significance to our primary specification. (Sensitivity models are available in Table A2).

Lastly, while not including the Heckman correction reduced the effect size slightly (Probit coefficient of −0.05, SE:0.00009), and varying the instruments included similarly slightly affected the coefficient, the results were directionally consistent, similar in magnitude, and were all highly precise. (Sensitivity models are available in Table A2).

4 | DISCUSSION

In this analysis of mental health staffing in the VHA, we find that mental health staffing has a large causal effect on the probability of an SRE. This effect appears to be concentrated in facilities with the lowest staffing levels, demonstrating diminishing returns to scale for investments in additional staffing. To our knowledge, this is the first analysis using granular measures of provider staffing levels combined with quasi-experimental methods to identify this relationship.

Our results have practical implications for health systems as well as for policy. From a macro perspective, our results imply that optimizing staffing requires at least one of three interventions: reducing demand and/or need; increasing staffing; or increasing efficiency. Given that reducing need or demand is unlikely given the well-documented shortage of mental health providers, a priority for health systems should, at minimum, be the retention of the existing workforce. Absent a readily available workforce, maximizing workflow efficiency will be crucial. This will require better leveraging of virtual care, re-thinking triage, and assessment protocols to better align patient needs with service, and considering differential scheduling strategies for new and established patients. Notably, our results finding no statistically significant effect of lagged staffing measures implies that mental health staffing is vital as an available resource within a narrow window of time—an appointment in a few weeks or months may not be good enough.

Another short-term strategy for health systems can be to better understand potential staffing bottlenecks. The diminishing return to staffing suggests that first expanding staffing in areas and facilities that have the lowest staffing levels relative to enrolled patients will have the biggest effect on SREs. Similarly, health systems should not ignore the importance of staff composition. Our measures of mental health staffing include social workers, psychiatrists, and psychiatric nurse practitioners. While we do not measure the distinct effects of different groups of providers on outcomes, it's likely that expanding staffing for provider types that are in particularly high demand will have bigger benefits in terms of patient outcomes.

While health systems can take short- to medium-term actions to mitigate staff shortages, longer-term support from policy makers will also be critical. Our findings are consistent with the view that broad expansions of mental health staffing are likely to be beneficial. Additionally, to the extent that providers are maldistributed geographically, policies targeting the most understaffed regions or those that make virtual care more accessible are similarly likely to be helpful.

5 | LIMITATIONS

Our work has important limitations. First, while the VHA provides an important setting for studying mental health outcomes because of its data and the high prevalence of mental health conditions among the population, our results may not necessarily generalize to other health systems or populations. Second, our sample is limited to a cohort of Veterans who were separated from service between 2014 and 2018. Although the sample is diverse among many socio-demographic characteristics, caution should be taken in generalizing the findings to Veterans who were separated from service outside the study period. Moreover, we cannot observe service use outside of the VHA. While our models account for important predictors of non-VHA service use and we account for partial observability of our outcome, Veteran enrollees often use non-VHA health services and outcomes occur in the course of non-VHA service use, and these data are not accounted for in our analysis.

Another important limitation is that our analysis does not address the possibility that Veterans may move to a different VHA medical facility due to factors correlated with pay period to pay period variations in mental health clinic staffing, which could potentially bias our results. In addition, this study relied on surveillance data rather than diagnosis codes to ascertain suicidal outcomes. While this method is widely considered to be the gold standard, analyses that rely on diagnosis codes may yield different results.[11,12] Finally, it should be noted that this study was limited to suicide-related events and did not examine mortality as a separate outcome.

6 | CONCLUSION

Adequate provider staffing is a prerequisite for individuals to receive access to health care. Our work finds that even in an integrated health system like the VHA, staffing constraints for mental health care appear to be binding and increasing mental health staffing would have substantial benefits for patients. It is likely that other health systems face similar constraints, and improving mental health staffing more generally would also lead to improved patient outcomes.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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