CLINICAL INVESTIGATION

Effect of Preoperative Geriatric Evaluation on Outcomes After Elective Surgery: A Population-Based Study

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BACKGROUND/OBJECTIVES: Randomized and nonrandomized single-center studies suggest that preoperative geriatric evaluation improves postoperative outcomes in older adults. The generalizability and population-level effect of preoperative geriatric evaluation has not been determined. Our objective was to measure the adjusted association between preoperative geriatric evaluation and postoperative outcomes.

DESIGN: Multilevel multivariable regression model analysis of a population-based historical cohort.

SETTING: Publicly funded universal healthcare system in Ontario, Canada.

PARTICIPANTS: All adults aged 65 and older having major, elective, noncardiac surgery from 2002 to 2014 (N = 266,499).

INTERVENTION: We studied geriatric consultations and comprehensive assessments performed in the 4 months prior to surgery. These were identified using validated methods.

MEASUREMENTS: Ninety-day survival (primary outcome), in-hospital complications, length of stay, 30-day readmissions, need for supported discharge, and 90-day costs of care.

RESULTS: The 7,352 participants (2.8%) who had a preoperative geriatric evaluation had longer 90-day survival than those who who did not (adjusted hazard ratio = 0.81, 95% confidence interval = 0.68–0.95). Length of stay and complication rates did not differ between groups, but participants evaluated by a geriatrician preoperatively had higher rates of supported discharge, readmission rates, and costs of care. Sensitivity analyses supported the association between preoperative geriatric assessment and 90-day survival.

CONCLUSION: In individuals aged 65 and older undergoing major, elective, noncardiac surgery, preoperative geriatric evaluation was associated with longer 90-day survival, but it is used infrequently. Given these results, and those of previous small studies, the influence of a geriatric evaluation on postoperative outcomes should be determined in a multicenter randomized trial. **J Am Geriatr Soc 2017.**

Key words: surgery; geriatrician; outcomes; epidemiology

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Older people are the fastest growing cohort of individuals undergoing surgery.[1] Advanced age is associated with morbidity, mortality, and resource use after surgery.[2–4] Given the projected increase in the number of older adults requiring surgery, interventions to improve their outcomes are needed.

Although age is independently associated with adverse postoperative outcomes, much of the outcome variation associated with older age may be attributable to geriatric syndromes such as frailty,[5,6] cognitive impairment,[7] and multimorbidity.[8] Geriatricians have in-depth training to address geriatric-specific risk factors, and perioperative geriatric care is associated with lower rates of complications and length of stay (LOS) in single-center randomized and nonrandomized studies.[9,10]

The population-level effect of preoperative geriatric care (including comprehensive geriatric evaluations and geriatric consultations) on outcomes and resource use has not been described. Therefore, our objectives were to describe use of preoperative outpatient geriatric care and

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the association between preoperative geriatric care and postoperative survival (primary outcome), complications, LOS, need for supported discharge, readmissions, and healthcare costs (secondary outcomes).

METHODS

Study Setting and Data Sources

Following approval by the Sunnybrook Health Sciences Centre Research Ethics Board, we conducted a population-based cohort study in Ontario, Canada, where healthcare services are publicly funded and recorded in health administrative datasets using standardized methods.[11,12] Data were linked deterministically using encrypted patient-specific identifiers at the Institute for Clinical Evaluative Sciences (ICES), an independent research institute. Datasets used included the Discharge Abstract Database (DAD), which captures details, diagnoses, and procedures from all hospitalizations; the Ontario Health Insurance Plan (OHIP), which captures physician service claims; the ICES Physician Database, which houses data on physician specialty, demographic characteristics, and training; the Assistive Devices Program Database, which captures receipt of durable medical equipment; the Continuing Care Reporting System, which captures long-term and respite care admissions; the Home Care Database, which captures receipt of home-based health services; the National Ambulatory Care Reporting System, which captures emergency and outpatient care data; the Ontario Drug Benefits Database, which houses prescription drug data for residents aged 65 and older; and the Registered Persons Database (RPDB), which captures demographic and vital statistics. Detailed descriptions of data sources are provided in Supplementary Table S1. The study analyst, CAW, created the analytic data set using normally collected data. A study analyst, CAW and the lead author, DIM performed the analysis, overseen by the senior author, CvW. The study protocol was registered at clinicaltrials.gov (NCT02975375), and this manuscript is reported according to appropriate guidelines.[13,14]

Study Population

We identified individuals aged 65 and older undergoing elective noncardiac surgery (peripheral arterial bypass, carotid endarterectomy, open abdominal aortic aneurysm repair, endovascular abdominal aortic aneurysm repair, total hip replacement, total knee replacement, large bowel surgery, partial liver resection, pancreaticoduodenectomy, gastrectomy, esophagectomy, pneumonectomy, lobectomy, nephrectomy, cystectomy). These are gender-neutral, intermediate- to high-risk operations and have been used together to study outcomes for individuals undergoing surgery in Ontario. (Codes used are shown in Supplementary Table S2.)[6,15–19] Admissions were elective, and the validity and reliability of codes used to identify these procedures were confirmed through re-abstraction.[20,21] Surgeries were identified between April 1, 2002 (to coincide with the introduction of *International Classification of Diseases, Tenth Revision* (ICD-10; to identify diagnoses) and the *Canadian Classification of Intervention* (to identify procedures)) and March 31, 2014 (the latest time at which all data sets were complete). This was a participant-level cohort; only the first surgery for each participant was included.

Exposure

Our exposure of interest was outpatient geriatric evaluation in the 4 months before surgery. We combined geriatric consultations and comprehensive geriatric assessments (CGAs) recorded in the OHIP database into a single exposure level using OHIP codes that have been previously validated (positive predictive value 93%; negative predictive value 90%[22]). Geriatric medicine specialists in Canada provide several levels of service, ranging from a complete CGA (taking at least 90 minutes to complete) to a focused consultation (lasting 20–75 minutes). These service tiers are reflected in different billing codes, which are time or case complexity based. The participation of other health professionals in a CGA are typically implied, but the tracking of these services is difficult because they are salary based. We cross-referenced the ICES Physician Database to confirm that a geriatrician performed the evaluations. We also identified several subgroups to facilitate sensitivity analyses. Specifically, we used billing codes to specify receipt of a geriatric consultation versus a CGA versus no geriatric evaluation. We also captured billing codes for in-hospital geriatric consultations.

Outcomes

The primary outcome was 90-day postoperative survival (from the RPDB—our reference standard for mortality). In-hospital complications were identified using clusters of ICD-10 type 2 diagnostic codes (captured from the DAD).[23] These codes demonstrated a positive likelihood ratio of 6.5 and a negative likelihood ratio of 0.39 when compared with complications prospectively captured through the National Surgical Quality Improvement Program.[24] LOS (day of surgery to day of discharge) was calculated from the DAD. Supported discharge was defined as being discharged with additional supports or to an institution. Thirty-day readmissions were identified as creation of a new record in the DAD after index discharge. Supported discharge and readmission outcomes were limited to participants discharged alive from hospital. Total healthcare costs in the 90 days after surgery were calculated using standard methods to derive individual-level healthcare costs from the perspective of the provincial health insurance plan; costs were standardized to 2014 Canadian dollars.[25]

Covariates

Demographic characteristics were identified from the RPDB. We identified Elixhauser comorbidities (ICD-9 and ICD-10 codes) from the DAD in the 3 years preceding surgery.[26] American Society of Anesthesiology scores were identified from physician billing. Frailty-defining diagnoses were identified using the Johns Hopkins Adjusted Clinical Group frailty-defining diagnoses indicator.[6,27,28] We identified receipt of the following prescription medications in the 6 months before surgery: angiotensin-converting enzyme inhibitors or angiotensin receptor blockers, antiarrhythmics, anticoagulants, anticonvulsants, antidepressants, antipsychotics, insulin, oral antihyperglycemics, antiplatelet agents, benzodiazepines, beta-blockers, oral corticosteroids, inhaled corticosteroids, inhaled bronchodilators, donepezil, rivastigmine, memantine, and galantamine. (Rivastigmine, memantine, and donepezil were collapsed into a single exposure of dementia medications.) The Hospital-patient One-year Mortality Risk (HOMR) score was calculated for each participant (an externally validated model for predicting mortality in hospitalized individuals (c-statistic 0.89–0.92)).[29] We recorded the year of surgery, emergency visits or hospitalizations in the year before surgery, rural versus urban residence, the hospital surgical volume, and participant's neighborhood income quintile. Polypharmacy was defined as receipt of five or more unique prescription drugs in the 90 days before surgery.

Sample Size

This was a population-based study; we enrolled all eligible individuals as opposed to limiting our sample based on a sample size calculation. With estimated short-term mortality of 2% in similar populations, 80% power at a 5% level of significance would require 7,286 participants per arm to detect a 30% relative decrease in mortality.

Analysis

SAS Enterprise Guide 6.1 (SAS Institute, Inc., Cary, N.C.) was used for all analyses. Participant characteristics were compared according to exposure levels using absolute standardized differences; differences greater than 0.1 are suggested to represent a substantial difference.[30]

Our primary outcome was analyzed using proportional hazards regression; adherence to the proportional hazards assumption was verified using log-negative-log plots. We first performed an unadjusted analysis and then a multivariable adjusted analysis, which clustered participants within hospitals using a robust sandwich covariance matrix estimate. The variables listed under Covariates were all adjusted for, and their forms are reported in the results.

Unadjusted and adjusted analyses were performed for all secondary outcomes. Secondary analyses were adjusted for the same variables as the primary analysis. LOS was analyzed as time to hospital discharge using proportional hazards regression (hazard ratios (HRs) >1 signify shorter LOS); participants were clustered within hospitals. In-hospital deaths were treated as competing risks by calculating the subdistributional hazard ratio.[31] Generalized linear models with a log-link that clustered participants within hospitals using generalized estimating equation methods with an exchangeable correlation matrix were used for all other analyses. Complications, discharge dependence, and readmissions were analyzed using a binary response distributions; costs were analyzed using a gamma response distribution.[32]

Sensitivity Analyses

We performed several prespecified sensitivity analyses. We repeated our primary 90-day survival analysis with two different exposures expressed as three-level categorical variables: preoperative geriatric consultation, preoperative CGA, no geriatric evaluation (reference category) and preoperative CGA or geriatric consultation only, preoperative CGA or consultation plus in-hospital geriatric care, no geriatric care (reference category). We also tested postulated effect modifiers by adding an interaction term to our primary adjusted model, which multiplied the covariate (frailty-defining diagnosis, procedure, sex, income quintile, multimorbidity (Charlson Comorbidity Index ≥2), HOMR score, and polypharmacy) by our main exposure.

We conducted several post hoc sensitivity analyses to ensure that our findings were robust. Baseline hazards may vary between procedures, so we stratified our primary analysis according to procedure (as opposed to including procedure as a fixed effect). Because most individuals who received a preoperative geriatric evaluation had orthopedic surgery, we repeated our primary analysis restricted to joint arthroplasties. To demonstrate that our findings were not dependent on our regression analysis, we performed a propensity score analysis by specifying a logistic regression model to predict the probability of receiving a preoperative geriatric evaluation for each participant using the same covariates that we adjusted for in our survival model. We then calculated inverse probability of treatment weights and trimmed individuals with weights outside the 2.5th to 97.5th percentiles. We calculated the rates of screening colonoscopy between main exposure levels to evaluate for a healthy user bias.

Missing Data

No exposure or outcome variables were missing. Sixty-two cases were excluded from analysis because of incomplete linkage (0.02%). Income quintile was missing for 0.4% of cases; the median value (3rd quintile) was imputed. Rural residency status was missing for 0.1% of cases; the most common value (not rural) was imputed. No other data were missing. Our study registration included a health utility index, but values were available for only 10.4% of individuals, so this outcome was not analyzed.

RESULTS

We identified 266,499 individuals aged 65 and older having their first elective noncardiac surgery (Figure 1). Preoperative geriatric evaluations were billed in 7,352 (2.8%) participants. The majority (6,506) were consultations; 846 CGAs were performed. Postoperative in-hospital geriatric evaluations were performed in 1,472 (20.0%) participants seen preoperatively. The proportion of participants receiving geriatric evaluations did not increase over time (Figure 2). Rates of screening colonoscopy or sigmoidoscopy were lower in the group of participants who received geriatric evaluation (38.1% vs 49.0%). Most participants who had a preoperative geriatric evaluation had a joint replacement. Rates of frailty dementia and mean participant age were higher in the preoperative geriatric evaluation group (summary Table 1; Supplementary Table S3).

In the 90 days after surgery, 1.4% (n = 103) of participants who received a preoperative geriatric evaluation

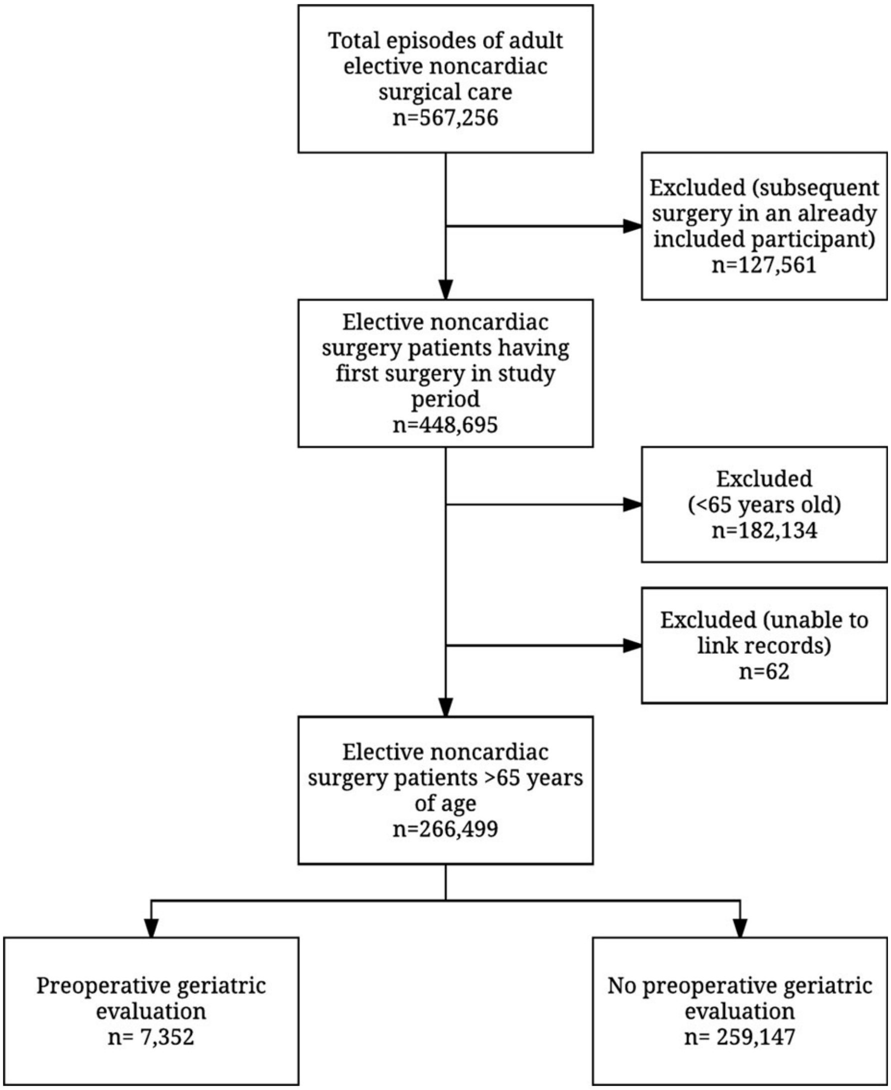

Figure 1. Creation of the participant-level analytic data set.

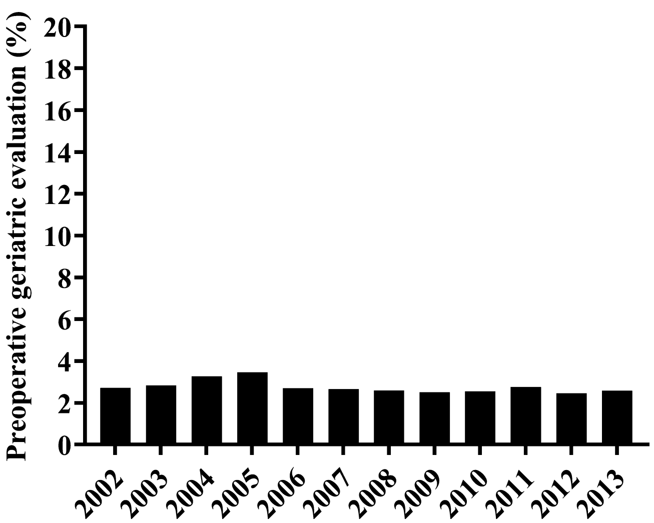

Figure 2. Proportion of participants receiving a preoperative geriatric evaluation in each study year. Data were organized according to fiscal year, so, for example, participants in 2002 had surgery between April 1, 2002, and March 31, 2003.

died, compared with 2.4% (n = 6,172) of participants who did not (HR = 0.59, 95% confidence interval (CI) = 0.48–0.71, $P < .001$). After multilevel multivariable adjustment, preoperative geriatric evaluation was significantly associated with longer survival (adjusted HR (aHR) = 0.81, 95% CI = 0.68–0.95, $P = .01$; Supplementary Table S4—full model specification).

Unadjusted and adjusted rates of secondary outcomes are provided in Table 2. Before adjustment, complication rates were lower and postoperative LOS was shorter in the preoperative geriatric evaluation group. Preoperative geriatric evaluation participants were less likely to be discharged home independently and more likely to be readmitted and had higher costs. After adjustment, costs, readmissions, and rates of nonindependent discharge continued to be significantly higher in the preoperative geriatric evaluation group.

Sensitivity Analyses

When we modeled our primary exposure as a three-level categorical variable with no evaluation as the reference,

Table 1. Baseline Characteristics of Study Population

Characteristic	Geriatric Evaluation, n = 7,352	No Geriatric Evaluation, n = 259,147	Absolute Standardized Difference[a]
Demographic			
Age, mean ± SD	75.0 ± 6.3	74.2 ± 6.2	0.13
Female, n (%)	4,400 (59.8)	137,096 (52.9)	0.14
Rural, n (%)	492 (6.7)	43,856 (16.9)	0.32
Neighborhood income quintile, median (interquartile range)	3 (4,5)	3 (4,5)	0.00
Baseline health status			
Frailty-defining diagnosis, n (%)	1,282 (17.4)	33,832 (13.1)	0.01
Number of comorbidities, mean ± SD	0.8 ± 1.4	1.0 ± 1.8	0.12
American Society of Anesthesiologists score ≥3, n (%)	3,118 (42.4)	121,467 (46.9)	0.09
Hospital One-year Mortality Risk score, mean ± SD	22.7 ± 7.7	25.1 ± 8.4	0.30
History of dementia, n (%)	63 (0.9)	1,162 (0.4)	0.06
Polypharmacy, n (%)	5,809 (79.0)	183,187 (70.7)	0.01
Preoperative healthcare use in year before surgery, n (%)			
Emergency department visit	2,847 (38.7)	96,717 (37.3)	0.03
Acute hospitalization	1,275 (17.3)	43,827 (16.9)	0.01
Surgery type, n (%)			
Total hip replacement	2,292 (31.2)	58,641 (22.6)	0.19
Total knee replacement	3,963 (53.9)	101,136 (39.0)	0.30
Carotid endarterectomy	228 (3.1)	7,248 (2.8)	0.02
Endovascular abdominal aortic aneurysm repair	23 (0.3)	3,639 (1.4)	0.12
Open abdominal aortic aneurysm repair	65 (0.9)	7,797 (3.0)	0.15
Peripheral vascular surgery	109 (1.5)	9,265 (3.6)	0.13
Partial or total nephrectomy	81 (1.1)	7,817 (3.0)	0.13
Total and radical cystectomy	21 (0.3)	3,328 (1.3)	0.11
Large bowel and rectal surgery	451 (6.1)	44,876 (17.3)	0.35
Partial liver resection	15 (0.2)	1,512 (0.6)	0.06
Pancreaticoduodenectomy	18 (0.2)	1,464 (0.6)	0.06
Gastrectomy or esophagectomy	36 (0.5)	5,027 (1.9)	0.13
Pneumonectomy and lobectomy	50 (0.7)	7,397 (2.9)	0.17

Full description of study population provided in Supplementary Appendix 1.
[a]Absolute standardized differences >0.1 represent a substantial difference.
SD = standard deviation.

Table 2. Secondary Study Outcomes

Outcome	Geriatric Evaluation, n = 7,352	No Geriatric Evaluation, n = 259,147	Unadjusted	Adjusted
			Relative Association[a] (95% Confidence Interval)	
Length of stay, mean ± SD[b]	6.0 ± 7.3	7.1 ± 10.3	1.19 (1.17–1.22)	1.03 (0.89–1.19)
Readmission, n (%)	643 (8.7)	20,656 (8.0)	1.10 (1.01–1.19)	1.13 (1.02–1.24)
Costs of care, mean ± SD[c]	22,571 ± 14,643	22,333 ± 19,614	1.01 (1.00–1.01)	1.03 (1.01–1.05)
In-hospital complication, n (%)	1,129 (15.4)	50,256 (19.4)	0.75 (0.71–0.80)	0.99 (0.92–1.08)
Supported discharge, n (%)	5,264 (71.6)	145,822 (56.3)	1.96 (1.86–2.06)	1.32 (1.23–1.41)

[a]Length of stay is a hazard ratio (HR); readmission, complications, and supported discharge are odds ratios; costs of care is an incidence rate ratio.
[b]In this time-to-discharge analysis, HR>1 signifies shorter length of stay.
[c]Expressed in 2014 Canadian dollars.
SD = standard deviation.

preoperative geriatric consultations were significantly associated with longer survival (aHR = 0.79, 95% CI = 0.65–0.97, P = .02); survival was longer in participants who underwent CGA as well, although not significant at the 5% level (aHR = 0.88, 95% CI = 0.61–1.27, P = .50). Comparing no preoperative evaluation with preoperative evaluation only or with preoperative and postoperative evaluation, preoperative evaluation only was associated with longer survival (aHR = 0.77, 95% CI = 0.62–0.93, P = .008); there was no difference in survival between participants evaluated by a geriatrician pre- and postoperatively and those who were not evaluated (aHR = 1.09, 95% CI = 0.79–1.50, P = 0.6). None of the interaction terms were statistically significant.

When we stratified our primary analysis according to procedure, our effect estimate was unchanged (aHR = 0.81, 95% CI = 0.68–0.95, $P = .01$). Limiting analysis to hip and knee replacements, a significant association between preoperative geriatric evaluation and survival was found (aHR = 0.71, 95% CI = 0.52–0.98, $P = .04$). Our inverse probability of treatment weights analysis also found a significant association between preoperative geriatric evaluation and survival (aHR = 0.65, 95% CI = 0.54–0.78, $P < .001$).

DISCUSSION

In this population-based cohort of older adults undergoing elective noncardiac surgery, preoperative geriatric evaluations were associated with greater likelihood of 90-day survival, although the proportion of participants who received preoperative geriatric care was low. Causal mechanisms underlying this association remain to be determined, but increased rates of supported discharge could contribute by better matching patient needs to appropriate postdischarge care. Given the evidence for better outcomes with preoperative geriatric care from small trials and this population-based study, future multisite randomized trials are warranted to guide appropriate, efficient application of preoperative geriatric care.

An emerging evidence base supports the association between preoperative geriatric care and better outcomes. A systematic review found that preoperative geriatric evaluation was associated with shorter LOS and lower complication rates, although heterogeneity in geriatric assessment models, study designs, outcomes, and small sample sizes limited findings.[9] A randomized trial of CGA in older adults undergoing vascular surgery found that CGA shortened LOS and decreased rates of some complications, including delirium,[10] but no studies have evaluated preoperative geriatric care in elective surgery at a population level or been adequately powered to study the association with survival. Based on our findings, preoperative geriatric evaluation is significantly and independently associated with greater likelihood of 90-day survival in older adults. Furthermore, this finding was robust across surgery types and when using different analytical approaches.

Geriatric evaluation may improve outcomes through numerous mechanisms. A geriatric medicine approach recognizes that older individuals require a holistic evaluation accounting for the interactions of comorbid conditions with the aging process, in contrast to an organ system–specific approach, in which optimization of an individual before surgery may focus on individual comorbidities. Geriatricians may also be better positioned to identify risk factors for geriatric syndromes such as delirium, falls, adverse drug events, and functional impairment.

In our study, in addition to better survival, participants who saw a geriatrician before surgery were more likely to be discharged to a nonhome location or with additional supports. This stands in contrast to randomized trial data that found CGA to be associated with greater rates of independent discharge.[33] Although these secondary outcomes must be interpreted with caution, geriatric care in a real-world setting may differ from trials. If independent discharge is specified as a trial outcome, then the intervention and those applying it would take specific actions to facilitate independent discharge. In our study, greater rates of supported discharge may have been applied to improve the match between patient needs and care delivery. In other words, perhaps discharging older adults after surgery with support contributes to better survival. Characteristics of postdischarge care were found to affect survival after emergency hip fracture surgery to a greater degree than in-hospital care.[34] Perhaps similar mechanisms contribute with elective surgery in older adults.

One must also consider whether the findings of an observational study represent a causal relationship and the extent to which findings were subject to bias. As expected, participants who saw a geriatrician before surgery were older and had higher rates of dementia and frailty, which all predict poorer survival. In contrast, HOMR scores were lower, partly reflecting the larger proportion of orthopedic patients who saw geriatricians (one of the lowest-risk services in the HOMR score). Our models adjusted for surgery type and HOMR score, so our findings are independent of these confounders. A healthy user bias was not apparent, because people evaluated by a geriatrician before surgery were less likely to have undergone cancer screening.

Optimal application of preoperative geriatric evaluation requires further study. Although we identified geriatric evaluations using validated codes, we were unable to measure the actual processes resulting from evaluation. Consultations and CGAs were both associated with greater likelihood of survival, but only consultations were significantly associated. This could be because of inadequate power in the case of CGA or because CGAs can only be billed in our healthcare system when an individual has dementia, which is a strong predictor of adverse outcomes. No survival advantage was found when participants saw a geriatrician both pre- and postoperatively, possibly because of unmeasured confounding, because geriatricians are more likely to be consulted postoperatively for individuals with complications. We were also unable to identify effect modifiers to suggest subgroups of individuals that would be most likely to benefit from geriatric evaluation. Because most participants in our study and a recent systematic review were undergoing orthopedic procedures,[9] future studies should assess geriatric care in surgeries with higher baseline risk, as well as specific populations, such as frail people, who may derive maximal benefit from this scarce resource.

Strengths and Limitations

This was an observational study, so we cannot prove causality, but because it was a population-based study, our findings may be generalizable to individuals who may not have been eligible for randomized trials. Unmeasured confounding is a risk to causal inference, but we were able to control for a large number of confounders, including mortality risk, diagnoses, preoperative healthcare use, and unmeasured hospital-level confounders through use of multilevel analyses and sensitivity analyses. Our primary outcome was measured using the criterion standard data source, and secondary outcomes have been validated. We preregistered our study protocol, which minimizes the risk of outcome switching and type 1 error. Finally, although our data suggests possible causal pathways, we were unable to study all possible pathways, such as improvements in medication appropriateness, that may be attributable to geriatric care.

CONCLUSION

In a population-based cohort of elderly adults undergoing elective noncardiac surgery, preoperative geriatric evaluation was associated with greater likelihood of 90-day postoperative survival. At a population level, preoperative geriatric evaluations were rarely performed; future efforts are needed to optimize use of geriatric care in our growing population of older adults undergoing surgery.

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Conflict of Interest: The authors have no conflicts of interest to declare.

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SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article:

Table S1. Description of linked data sources used to create the analytic data set

Table S2. Procedural codes to define study population

Table S3. Full description of participant characteristics

Table S4. Full specification of adjusted primary survival model

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