Rural–Urban Differences in the Effect of Follow-Up Care on Postdischarge Outcomes

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Objective. To assess rural–urban differences in quality of postdischarge care among Medicare beneficiaries, controlling for selection bias of postdischarge services.

Data Sources. The Medicare Current Beneficiary Survey (MCBS), Cost and Use Files from 2000 to 2010, the Area Resource File, Provider of Services File, and the Dartmouth Atlas of Health Care.

Study Design. Retrospective analysis of 30- and 60-day hospital readmission, emergency department (ED) use, and mortality using two-stage residual inclusion; receipt of 14-day follow-up care was the main independent variable.

Data Extraction Method. We defined index admission from the MCBS as any admission without a previous admission within 60 days.

Principal Findings. Noninstrumental variables estimation was the preferred estimation strategy. Fourteen-day follow-up care reduced the risk of readmission, ED use, and mortality. There were no rural–urban differences in the effect of 14-day follow-up care on readmission and mortality. Rural beneficiaries experienced a greater effect of 14-day follow-up care on reducing 30-day ED use compared to urban beneficiaries.

Conclusions. Follow-up care reduces 30- and 60-day readmission, ED use, and mortality. Rural and urban Medicare beneficiaries experience similar beneficial effects of follow-up care on the outcomes. Policies that improve follow-up care in rural settings may be beneficial.

Key Words. Medicare, readmission, ED use, rural, postdischarge, follow-up care

The Patient Protection and Affordable care Act introduced new incentives and reforms to reduce hospital 30-day readmission rates (Grabowski et al. 2012). After hospital discharge, Medicare beneficiaries often have complex medical needs that complicate transitions from hospitals to home. Poor transitions in care lead to preventable hospital readmissions, emergency department (ED) visits, and mortality, costing Medicare \$12 billion each year

(Naylor et al. 1999; Lichtman et al. 2012; Vashi et al. 2013). Studies demonstrate that high-quality transitional care reduces hospital readmissions, lowers costs, and improves other outcomes by facilitating coordination and communication across providers and settings of care (Hansen et al. 2011; Verhaegh et al. 2014).

Key elements of a successful care transition often include an outpatient visit within 1–2 weeks (Hansen et al. 2011; Hubbard et al. 2013; Kripalani et al. 2016). An early follow-up visit can help the patient and doctor identify potential gaps in postdischarge care that, if addressed, may prevent acute service utilization. Additionally, as payers create financial incentives for health care providers to contain costs, early postdischarge follow-up care may be an important strategy for reducing costs by preventing unplanned acute utilization.

The effective use of post discharge services among rural Medicare beneficiaries is poorly understood; however, studies suggest that rural beneficiaries may be at greater risk of using acute care services postdischarge. Although rural and urban beneficiaries have similar access to overall care (Stensland et al. 2013), a recent study found rural Medicare beneficiaries transitioning from the hospital to home had a higher risk of visiting the ED and a lower rate of follow-up care during a 30-day post discharge period, compared to urban beneficiaries (Toth et al. 2015). While there was no relationship between rural residency and readmission, beneficiaries discharged from a rural hospitals had a greater risk of readmission, compared to being discharged from urban hospitals. Other studies show rural beneficiaries receive less follow-up care, but have lower readmission rates, relative to urban beneficiaries (Bennett et al. 2012a,b). Still, more recent work suggests dual-eligible beneficiaries have higher follow-up in rural settings and argues for an increased understanding of the role of follow-up care among rural beneficiaries (Bennett and Probst 2015). Indeed, these findings may be explained by three characteristics of rural (vs. urban) beneficiaries: poorer access to appropriate postacute care; cultural tendencies to avoid care; personal characteristics; and weaker infrastructure

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to integrate and coordinate postdischarge services (Hutchison, Hawes, and Williams 2005; Gamm, Castillo, and Pittman 2003). If so, the effect of receiving follow-up care may differ by whether care is delivered in rural versus urban settings.

While recent literature on the role of follow-up care in improving post-discharge outcomes appears promising, most of this research comes from randomized control trials, in which these services were part of a broader intervention (Verhaegh et al. 2014). Other research, using secondary data or before-after designs, shows the potential for early follow-up care to improve outcomes; however, unobserved characteristics may bias these results by failing to account for selection bias inherent in the receipt of follow-up care (Hernandez et al. 2010; Sharma et al. 2010; Lin, Barnato, and Degenholtz 2011; Fleming and Haney 2013). To our knowledge, no study has addressed the unobserved endogeneity in the receipt of follow-up care.

Therefore, this study will model the effect of early follow-up care among rural and urban beneficiaries on 30- and 60-day readmission, ED use, and mortality using instrumental variables (IV) to correct for endogeneity. We hypothesize that accounting for selection bias in the effect of follow-up care will show that these treatments reduce the risk of readmission, ED use, and mortality, although this reduction will be smaller among rural beneficiaries.

METHODS

Data Source

The Medicare Current Beneficiary Survey (MCBS), Cost and Use files from 2000 to 2010, was the main data source for this study. The MCBS is a longitudinal panel survey of a nationally representative sample of the Medicare population. Survey data are collected from each respondent three times a year over a 4-year period among rotating panels of participants. Each year of data had approximately 12,000 respondents representing four panel periods. These data include beneficiaries' health status, access to care, household composition, health service use and expenditures, and sociodemographic characteristics. Survey data are linked with Medicare claims, which are collected continuously for beneficiaries over the duration of their participation in the MCBS. Claims data include utilization of inpatient care, outpatient care, home health care, and physician office-based services. Hospital-level characteristics were captured from CMS Provider of Service File, which comes from a yearly

survey of Medicare certified hospitals. County-level characteristics were captured from the Area Resource File and the Dartmouth Atlas of Health Care.

Study Sample

The sample included Medicare beneficiaries with an acute admission in the MCBS from 2000 to 2010 who were (1) ≥ 65 years; (2) enrolled in traditional fee-for-service Medicare within the contiguous 48 states; and (3) hospitalized for acute medical problems and subsequently discharged to home with or without home health care. To establish the beginning of acute care episodes (index admission), the sample excluded beneficiaries with an admission within the previous 60 days, admissions that occurred within 60 days of the first survey year, admissions for cancer management, and admissions with a primary diagnosis of a psychiatric or rehabilitation condition (not usually in acute stay hospitals) (Horwitz et al. 2012). We excluded observations with a readmission, ED event, or mortality within 3 days of discharge as these acute events are more likely markers of quality of care delivered in the hospital. Those who died within 60-days discharge in the readmission and ED models were excluded. A complete case analysis was used. We started with 41,970 admissions to an acute care hospital from 2000 to 2010. After selection criteria were applied, our analytical sample was 11,492 for the readmission and ED models, and 11,824 for the mortality models.

Key Variables and Measures

The main dependent variables were 30- and 60-day readmission, 30- and 60-day ED use (treat and release), and 30- and 60-day mortality. Hospital readmission was defined using the Yale/CMS all-condition 30-day unplanned readmission measure (Horwitz et al. 2011). Because it is difficult to assess unplanned admission using administrative claims data, we excluded admissions for maintenance chemotherapy or rehabilitation and readmissions in which at least one of 35 typically planned procedures occur without having a primary diagnosis of an acute event (MedPac 2013).

The main independent variables were receipt of follow-up care within 14 days post discharge. We defined follow-up care as any Evaluation and Management visit in an outpatient or office based setting (HCPCS codes ≥ 99241 & ≤ 99245 , ≥ 99201 & ≤ 99205 , and ≥ 99210 & ≤ 99215) during the 14-day period before either an ED visit or hospital readmission (Hubbard et al. 2013). To insure adequate statistical power, we created a dichotomous variable

for rural residency (Rural vs. Urban) using Rural–Urban Commuting Codes (RUCA). RUCA codes combine standard Census definitions with area commuting behaviors to capture functional and work relationships between regions; they are frequently used by federal agencies and for policy analysis (Toth et al. 2014; WWAMI: Rural Health Research Center 2015).

Covariates

Demographic variables included both continuous (income) and categorical (age, race, gender, education, Medicaid status, and household composition) variables. Health status included number of limitations in activities of daily living and instrumental activities of daily living, length of the index hospitalization, receipt of home health care (defined as having any home health claim within 14 days of the discharge date that occurred prior a postdischarge ED visit or hospital readmission), and the Charlson Comorbidity Index (CCI). The CCI predicts risk of mortality based on beneficiaries' primary and secondary diagnosis and is frequently used to control for confounding of disease severity on postdischarge outcomes (Joynt, Orav, and Jha 2011; Anderson 2014). Hospital facility-level variables included a categorical hospital location using RUCA codes (Urban, Large rural, Small rural, and Isolated rural), for-profit status, and a continuous number of registered nurses and number of beds.

Instrumental Variables

Valid instruments must pass at least two criteria: (1) significantly explain variation in the endogenous treatment variable; and (2) not predict the dependent variables except through the treatment variable. Unobserved factors may predict the use of follow-up care, and also increase the propensity to use acute care services. Endogeneity may have a positive bias in the effect of follow-up care because sicker beneficiaries are more likely to both receive more outpatient services and acute services. That is, if the true effect is negative, the coefficients may show no effect or even a positive effect on readmission, ED use, and mortality. We acquired three potential IVs from the Area Resource File and from the Dartmouth Atlas of Health Care.

Provider Distance. Distance to provider has been used as instrument for health outcomes in other studies (Hadley et al. 2003). We argue that the closer a

resident is to the discharging hospital, the more likely that person would receive a follow-up visit as he or she is more likely to live within that hospital's network of providers and outpatient facilities, simplifying care coordination. Once controlling for patient-level demographic, health, and hospital characteristics, distance to the hospital should have no relationship with the outcome, except through follow-up care (Yamashita and Kunkel 2010). We calculated straight line distances between the geographic center of the beneficiaries' residency zip code to the center of the admitting hospital zip code. Because of the skewed distribution of miles from the hospitals, we estimated log-distance from residency to admitting hospital (*distance to hospital*).

Physician Supply. We used the Area Resource File to construct a county-level primary care provider supply variable. This included total general practitioners, family practitioners, and specialists per 10,000 elderly (*physician supply*). Physician supply should be strongly associated with follow-up care (Yasaitis, Bynum, and Skinner 2013). Some research suggests supply of primary care physicians reduces the number of preventable hospitalization in a county (Laditka, Laditka, and Probst 2005). Yet we argue that the beneficial effect of physician supply on 30- and 60- day postdischarge outcomes is a result of better access to primary care and better continuity of care (Macinko, Starfield, and Shi 2007), which in this case would represent an early follow-up visit. Thus, *physician supply* can be validly excluded from our outcome models because its principal relationship with 30- and 60-day outcomes will be captured by whether the beneficiary receives an early follow-up visit. Residents living in communities with a higher density of primary care providers should be more likely to receive a follow-up visit within 14 days of discharge, compared to residents living in areas with lower primary care density.

County Follow-up Rate. County-level rates of 14-day primary care follow-up (*percent follow-up*) were acquired from the Dartmouth Atlas of Health Care. The Dartmouth Atlas of Health Care calculates these rates using 100 percent Medicare claims among those who were discharged from an acute facility, identifying follow-up visits as any visit with a physician in an outpatient setting. We linked Federal Information Processing Standard (FIPS) values from the Dartmouth files with beneficiary zip codes. Geographic practice patterns are frequently used as IVs in the literature and should be a good candidate for an IV in this analysis (Bound, Jaeger, and Baker 1995; Hadley et al. 2010).

County-level 14-day follow-up care is an indicator of coordination of care within the county (Goodman, Fisher, and Chang 2013). We argue that any relationship between county-level rates of 14-day follow-up care with patient level outcomes in readmission, ED use, or mortality postdischarge can be attributed to whether a patient receives a physician visit postdischarge. Any additional correlation can be accounted for through our demographic, health, and hospital characteristics controls. We use years 2004, and 2008 to 2010. Imputed values for missing years were calculated by taking the average percent change between the actual values at the FIPS level and applying to the missing year. Higher county level rates of 14-day follow-up care should be positively associated with the probability that a beneficiary with a residency in the same county would receive a follow-up visit.

Statistical Analysis

For descriptive statistics, we by adjusted for the complex survey design of the MCBS using cross-sectional weights. Bivariate comparisons by rural category were tested using chi-squared statistics for categorical variables and t-tests for continuous variables. We estimated the main outcome equation using a survey adjusted logit with replicate weights. There is potential selection bias due to unobserved characteristics (e.g., severity of illness at discharge) that may increase both the propensity for a follow-up visit while also increase the likelihood of an unplanned acute event. To address this selection bias, we used an IV analysis in the form of a two-stage residual inclusion (2SRI). This approach is preferred because the two-stage-least-squares approach yields inconsistent estimates in nonlinear models (Terza, Basu, and Rathouz 2008). 2SRI entails calculating the residual from the first-stage equation, and including the first-stage residuals and the endogenous treatment variables in the second-stage equation. As we are also interested in the interaction between rural residency and follow-up care, we acquired two residuals by running a first-stage logit on follow-up treatment, calculated the residuals, and interacted the residuals with a dichotomous rural residency variable. The first-stage logit consisted of our three exogenous IVs and controls:

1st Stage: Prob(Follow Up, |Z₁, Z₂)

$$\begin{aligned} &= \alpha + \beta_1(\text{County Level Physician Supply}) + \beta_2(\%14\text{day Follow upper County}) \\ &+ \beta_3(\text{Logged Hospital Distance}) + \delta X + \varepsilon \end{aligned}$$

where $\beta_1 - \beta_3$ are the coefficients for the IVs, δ represents the coefficients for the control variable including demographic, health status, and admitting hospital characteristics, and year of admission, as described above, and ε represents the error term. The second-stage equation consisted of a survey weighted multivariate logistic regression, interacting rural and follow-up care.

$$\begin{aligned} 2\text{st Stage: } Prob(Y|X_1) = & \alpha + \beta_1(Rural) + \beta_2(Early Follow Up) \\ & + \beta_3(Rural * FollowUp) + \delta X + \gamma Follow Up_{RES} \\ & + \lambda Rural * Follow Up_{RES} \end{aligned}$$

where β_2 and β_3 were the endogeneity adjusted coefficients for the early follow-up treatment, and λ and γ were the coefficients on the residuals from the first-stage equation on the probability of follow-up treatment within 7 days and within 14 days. We calculated average marginal effects of the treatment variables and the rural-interaction terms using method of recycled prediction

$$\begin{aligned} Marginal Effect_{ATE} = & 1/N \sum_i (Prob(Y_i | FOLLOW UP = 1, X_i) \\ & - Prob(Y_i | FOLLOW UP = 0, X_i)) \end{aligned}$$

We estimated the standard errors of the marginal effects using STATA's bootstrapping procedure with 1,000 replications. To demonstrate the consistency of our estimates, we report marginal effects of both the logistic regression without IV and the 2SRI latent average treatment effects. This study was exempt from the University of North Carolina at Chapel Hill's IRB review.

RESULTS

Descriptive Statistics

Our sample included 7,860 hospital admissions among beneficiaries residing in urban areas and 3,964 admissions among beneficiaries residing in rural areas (Table 1). Compared to urban beneficiaries, rural beneficiaries were slightly younger, had less education, less income, and were more likely to be on Medicaid. There were no statistically significant differences in the unadjusted outcomes, except that rural beneficiaries used the ED more frequently at 30 and 60 days compared to urban beneficiaries ($p < .01$).

Table 1: Summary Statistics of Index Admissions among Medicare Beneficiaries by Rural Status, MCBS, $N = 11,824$

	<i>Total</i> (<i>N</i> = 11,824)	<i>Urban</i> (<i>N</i> = 7,860)	<i>Rural</i> (<i>N</i> = 3,964)	<i>p-value</i>
Outcomes				
30-day readmission	11.33	11.25	11.49	0.7
60-day readmission	18.2	17.93	18.84	0.17
30-day ED use	6.67	6.2	7.74	0.003
60-day ED use	10.69	9.95	12.35	<0.001
30-day mortality	1.37	1.36	1.4	0.87
60-day mortality	2.66	2.64	2.7	0.87
Demographics				
Age				
65–70	21.51	20.89	22.91	0.1
71–86	63.77	63.84	63.61	
86–90	10.26	10.64	9.39	
90 and older	4.47	4.63	4.09	
Female	56	56.16	55.63	0.7
Race				
White	87.91	85.96	92.35	0.002
Black	8.52	9.97	5.23	
Hispanic	1.48	1.79	0.78	
Other	2.08	2.27	1.65	
Education				
Less than high school	35.03	31.7	42.61	<0.001
High school	48.31	48.99	46.75	
College or graduate	16.66	19.31	10.64	
Living situation				
Lives alone	31.87	31.37	33.03	0.04
Spouse only	41.29	40.85	42.3	
Spouse and others	6.93	7.16	6.4	
Children only	9.35	9.46	9.11	
Children & others	4.84	5.47	3.4	
Others only	3.34	3.36	3.3	
Nonrelative	2.37	2.33	2.46	
Income (thousands)	29.05 (47.93)	30.88 (53.50)	24.91 (27.68)	<0.001
Medicaid status	17.93	16.67	20.79	0.02
Health status				
No. of ADLs	1.09 (1.60)	1.10 (1.56)	1.08 (1.68)	0.7
No. of IADLs	0.35 (0.70)	0.35 (0.68)	0.36 (0.74)	0.51
Charlson comorbidity score	3.26 (2.61)	3.33 (2.59)	3.09 (2.65)	0.002
Length of stay	4.00 (3.69)	4.06 (3.84)	3.88 (3.26)	0.12
Home health use	18.92	19.91	16.67	0.007

Continued

Table 1 *Continued*

	<i>Total</i> (<i>N</i> = 11,824)	<i>Urban</i> (<i>N</i> = 7,860)	<i>Rural</i> (<i>N</i> = 3,964)	<i>p-value</i>
Hospital characteristics				
Number of beds	372.48 (280.52)	430.02 (269.10)	241.69 (258.58)	<0.001
Hospital location				
Urban	77.45	97.76	31.28	<0.001
Large rural	15.47	1.61	46.99	
Small rural	4.55	0.49	13.77	
Isolated rural	2.53	0.13	7.97	
Profit status				
Nonprofit	69.41	72.43	62.57	0.005
For-profit	13.03	12.83	13.5	
Government	17.55	14.74	23.94	
Total full time RNs	405.68 (421.27)	468.64 (428.00)	262.59 (357.32)	<0.001
Instrumental variables				
Avg. % 14-day follow-up	44.06 (7.04)	42.77 (5.98)	46.99 (8.43)	<0.001
Distance to hospital	40.85 (173.91)	36.42 (172.57)	50.92 (175.69)	<0.001
Physician supply per 10,000 elderly	107.78 (69.21)	130.13 (68.14)	56.96 (31.94)	<0.001

Notes. *p*-values by weighted linear regression for continuous variables and weighted chi-squared test for binary/categorical variables. Values are expressed as mean (standard deviation) or %. ADLs, activities of daily living; IADLs, instrumental activities of daily living.

Instrumental Variable Specification

Joint Wald tests on the three instruments in the first-stage equation indicated that they strongly predicted the use of 14-day follow-up care. The instruments for 14-day follow-up care were both individually and jointly significant predictors of receiving follow-up care ($\chi^2 = 35.71$, $p < .001$). Additionally, each IV was individually significant, and the coefficients were in the hypothesized direction (e.g., *distance to hospital*, $\beta = -0.064$, $p < .01$; *provider supply*, $\beta = 0.001$, $p < .05$; *percent follow-up*, $\beta = 0.009$, $p < .01$).

Table 2 displays the exclusion restriction tests, the test for exogeneity, and the preferred model for each dependent variable. We followed a method similar to Bollen, Guilkey, and Mroz (1995) for testing the over-identified restriction of the second-stage model. We first calculated the predicted residuals of the first-stage estimation of 14-day follow-up care with the identified IVs. We then included the over-identified restriction in the second-stage equation with the treatment variables and the predicted residuals from the first-stage equation. We conducted a Wald test on the over-identified IV, where the null hypothesis was that the IV was validly excluded from the second-stage model (Column 1). Regardless of which IV was tested, all passed the over-

Table 2: Exclusion Restriction and Exogeneity Tests for Dependent Variables, MCBS, Cost, and Use, 2000–2010

Dependent Variables	(1) Exclusion Restriction for Over-Identified IV (χ^2_1)	(2) Hausman–Exogeneity Test (χ^2_2)	(3) Preferred Estimation Method
30-day readmission	0.08	0.70	Logit
60-day readmission	0.08	0.09	Logit
30-day ED use	5.95*	2.44	Logit
60-day ED use	2.86	1.27	Logit
30-day mortality	0.15	4.48	Logit
60-day mortality	0.46	1.30	Logit

Notes. * $p < .05$, Column 1 displays a Wald test on physician supply. Physician supply passed every model with exception to 30-day ED use. We use exact identification in that mode. A variant of the Hausman-Exogeneity Test consisted of a joint Wald test on the first-stage residual and the first-stage residual interacted with a dichotomous rural residency. We estimated the covariance of the residuals using STATA's bootstrapping procedure with 1,000 replications.

identification test for most the outcomes with the exception of *physician supply* and *hospital distance*. Physician supply passed the over-identification tests for all models except for 30-day ED use ($\chi^2_1 = 5.95$, $p < .05$), and *hospital distance* passed all models except 30-day mortality ($\chi^2_1 = 3.91$, $p = .05$). Thus, we modeled 30-day ED use and 30-day mortality with exact identification using only *distance to hospital* and *percent follow-up*, and *percent follow-up* and *physician supply*, respectively, for each model. We also performed an additional falsification tests consistent with Pizer's (2015) approach using beneficiary-level Medicare prescription expenditures reported during the survey year. This alternative outcome should be independent of the treatment variable, associated with our main outcomes, and potentially associated with our IVs. Should our IVs be associated with prescription expenditures, it may indicate other pathways through which readmission, ED use, or mortality are reduced. After adjusting for demographic, health, and hospital characteristics, we were unable to reject the null hypothesis that our IVs are jointly related to annual Medicare expenditures on prescription medications ($\chi^2_3 = 3.53$, $p = .32$).

Column 2 of Table 2 presents the results of the exogeneity tests for the treatment variables, and the preferred models based off these tests. Because the 2SRI approach is similar to a variant of the Hausman test for exogeneity, the second-stage models were run with the residuals and a joint-test of significance was conducted on the residuals, where the null hypothesis is that the treatment variables are exogenous. If the treatment variables are exogenous, then the residuals should have no explanatory power and the non-IV logistic

model is preferred because the standard errors are more precise. This point is important; choosing an unbiased but imprecise estimator can increase the chance of Type-II error. We conducted joint Wald tests on the residuals where the covariance matrix of the residuals were estimated using a bootstrapping procedure with replacement and 1,000 replications. We found that for all models there was no evidence of endogeneity in 14-day follow-up care, suggesting that the preferred estimate comes from the non-IV logistic regression model. Column 3 of Table 2 indicates the preferred estimation strategy for this model.

Main Results

Based on the logistic regression model, the average marginal effect of 14-day follow-up care on 30 and 60-day readmission was 3.82 percent point and 2.69 percent point reductions for 30- and 60-day readmission, respectively ($p < .01$) (Table 3). The marginal effects of 14-day follow-up on 30- and 60-day readmission were similar in direction and magnitude in both the 2SRI and logit models, though the 2SRI estimates were nonsignificant. As a secondary analysis, we examined the marginal effects of 14-day follow-up as the number of comorbidities increased; we found that the 14-day

Table 3: Average Marginal Effects and 2SRI Estimates of 14-Day Follow-up Care on Readmission and ED Use, $N = 11,492$

	30-Day Readmission		60-Day Readmission	
	2SRI	LOGIT	2SRI	LOGIT
Follow-up care	−0.0009 (0.119)	−0.0382** (0.006)	−0.0356 (0.130)	−0.0269** (0.007)
Rural×follow-up	0.063 (0.071)	0.0214 (0.012)	0.0302 (0.083)	0.00621 (0.014)
	30-Day ED Use		60-Day ED Use	
	2SRI	LOGIT	2SRI	LOGIT
Follow-up care	−0.0303 (0.137)	−0.0307** (0.005)	−0.0496 (−0.135)	−0.0211** (0.006)
Rural×follow-up	0.0731 (0.082)	−0.0266* (0.012)	0.0639 (−0.081)	−0.0172 (0.013)
Observations	11,492	11,492	11,492	11,492

Notes. ** $p < .01$, * $p < .05$.
Bootstrapped standard errors in parenthesis.
2SRI estimates represent the local average treatment effect, whereas the logit estimates represent the average treatment effect.

follow-up offered a greater reduction in the risk of 30-day readmission as the beneficiaries CCI score increased (Figure 1). For example, among those with a CCI score of 1, receiving 14-day follow-up was associated with a 2.50 percent point reduction in the risk of 30-day readmission, while among those with a score of 4, follow-up care reduced 30-day readmission risk by 4.19 percent points. There were no treatment effect differences between rural and urban beneficiaries.

Table 3 also presents the average marginal effects for 30-and 60-day ED use. The average marginal effect of 14 day follow-up care was a 3.07 percent point decrease in the risk of 30-day ED use ($p < .01$), and 2.11 percent point decrease in the risk of 60-day ED use ($p < .01$). Rural residency modified this effect by reducing the risk of 30-day ED use by 2.66 percent points, but it had no interaction effect on 60-day ED use, compared to urban beneficiaries. Indeed, the effect of 14-day follow-up care on 30-day ED use among rural beneficiaries was a 4.90 percent point reduction ($p < .05$) compared to a reduction of 2.23 percent points among urban beneficiaries ($p < .05$) (data not shown). With exception to the interaction effect, the 2SRI estimates were generally in

Figure 1: Average Marginal Effects of Early Follow-up Care on 30-Day Readmission by Charlson Comorbidity Index ($N = 11,492$)

the same direction and magnitude as the non-IV estimates, but they were non-significant. These similar estimates suggest any bias in the non-IV model were mitigated by our covariates.

Table 4 displays the marginal effects of 14-day follow-up care on 30- and 60-day mortality. Follow-up care within 14 days reduced 30- and 60-day mortality by 1.33 percent and 1.57 percent points on average, respectively ($p < .01$). There were no differences in the effect of follow-up care on mortality between rural and urban beneficiaries. While the 2SRI models did not produce significance results, it is interesting to note that the average marginal effects are larger and in the opposite direction.

Several sensitivity analyses were conducted. We eliminated the 3-day washout period to test whether our results were sensitive to including short-term readmission and ED use; results remained consistent with original outcomes. Additionally, to explore the large effect sizes for our 2SRI models, we ran the models with exact identification and different combinations of *hospitals distance*, *provider supply*, and *percent follow-up*. The results were similar to our original models. We also conducted a stratified analysis of beneficiaries with a principal diagnoses of conditions subject to penalties under Medicare’s Hospitals Readmission Reduction Program (HRRP). While our sample was reduced to 2,084, we found the direction, magnitude, and significance of the marginal effects to be similar in all our models. Finally, we tested whether follow-up care within 7-days produced similar results, but we were unable to establish 7-day follow-up care as having any effect on 30- and 60-day outcomes.

Table 4: Average Marginal Effects and 2SRI Estimates of 14-Day Follow-up Care on Mortality, $N = 11,824$

	30-Day Mortality		60-Day Mortality	
	2SRI	LOGIT	2SRI	LOGIT
Follow-up care	0.169 (0.151)	−0.0133** (0.002)	0.0439 (0.175)	−0.0157** (0.003)
Rural × follow-up	0.131 (0.087)	−0.00168 (0.004)	0.0759 (0.0867)	0.00245 (0.006)
Observations	11,824	11,824	11,824	11,824

Notes. ** $p < .01$.
Bootstrapped standard errors in parentheses.
2SRI estimates represent the local average treatment effect, whereas the logit estimates represent the average treatment effect.

DISCUSSION

This study sought to determine whether early follow-up care reduced the risk of readmission, ED use, and mortality during 30- and 60-day post discharge periods, and whether rural and urban beneficiaries experience different treatment effects. We found evidence that 14-day follow-up care reduces the risk of 30- and 60- day readmission, ED use, and mortality. Notably, rural beneficiaries experienced a differential effect of 14-day follow-up care in reducing the risk of visiting the ED within 30-days post discharge; however, there was no statistically significant difference in treatment effects for readmission and mortality. These findings have several important implications.

First, our findings further support the importance of a physician follow-up visit soon after hospital discharge (Hernandez et al. 2010; Sharma et al. 2010; Hubbard et al. 2013). Specifically, we found that 14-day follow-up reduced the risk of 30- and 60-day readmission. Moreover, examining the effect of 14-day follow-up care as the number of co-morbidities increases, the effect persists and increases. This suggests that Medicare beneficiaries with multiple comorbidities should be targeted for early follow-up care, particularly in the context of Medicare's HRRP. Additionally, current post-acute bundled payment demonstrations include Part B services and are considering 60 days as an appropriate time frame for an episode of care (Morley et al. 2014; Centers for Medicare and Medicaid Services 2015). These findings suggest that early follow-up care may help reduce inpatient utilization for those discharged home with or without home health care over 60 days, and may be advantageous to providers in the context of bundled payments. While we did not find treatment effect differences between rural and urban beneficiaries, stratified analysis did show rural and urban beneficiaries both benefited from a 14-day follow-up visit. Importantly, these findings were robust to a stratified sample of conditions subject to penalties under HRRP. Given recent concerns on how rural providers and patients will fare financially under HRRP and other reforms, targeting resources toward helping sicker patients to receive an early follow-up appointment may help reduce rural hospital costs of acute readmissions (Joynt and Jha 2011; MacKinney, Mueller, and McBride 2011; Hung, Casey, and Moscovice 2015).

Second, 14-day follow-up care reduced the probability of 30- and 60-day ED use for both rural and urban beneficiaries. Contrary to our original hypothesis, rural beneficiaries appear to benefit more from a 14-day follow-up visit relative to urban beneficiaries. This finding does not necessarily indicate

better quality of care in rural settings, but rather may reflect unmet need and relatively poorer access to primary care in rural settings (Laditka, Laditka, and Probst 2009; Hossain and Laditka 2011). Our findings suggest that rural beneficiaries who receive a follow-up visit within 14 days of discharge will also experience a reduced probability of visiting the ED 30 and 60 days after hospital discharge. Policy makers and providers working to establish or design Accountable Care Organizations and postacute bundled payments should consider whether to incentivize rural areas to invest in efforts to improve follow-up care. Policies that support primary care in rural settings, such as enhancing reimbursement for primary care activities by physician assistants and nurse practitioners, may be beneficial, particularly in helping to overcome potential barriers to early follow-up care in rural settings (Toth et al. 2015). Moreover, direct investments or grants to hospitals to improve postdischarge care, case management services, or engagement with community resources and independent outpatient providers has been considered in other settings, and it may be appropriate for rural providers (MedPac 2013; Bernheim 2014; Joynt et al. 2014). Doing so may benefit rural providers, payers, and beneficiaries by reducing costs associated with preventable ED visits and readmissions, including Medicare Part B expenditures and reduced patient cost-sharing.

Finally, consistent with other studies (Van Walraven et al. 2004; Ezekowitz et al. 2005), follow-up care was associated with lower rates of mortality 30 and 60 days after hospital discharge. These results provide further evidence that rural providers may have the opportunity to reduce 30-day mortality outcomes by investing in efforts to ensure an early follow-up visit. Indeed, earlier work shows that critical access hospitals (CAHs), mostly serving rural beneficiaries, have higher risk adjusted 30-day mortality than non-CAHs (Lichtman et al. 2012). As such, the findings of this study suggest that better follow-up care may be beneficial in helping to lower mortality in rural and urban settings.

There are some important limitations to this study. Our conclusions are based on non-IV logistic regressions and may be biased. Indeed, results from the non-IV models may under-estimate the magnitude of the effect size or, in the case of the 30- and 60-day mortality, may be biased in the opposite direction. On the other hand, there was no empirical evidence of endogeneity in our follow-up variables, suggesting that our covariates account for much of the variation in the receipt follow-up care. Additionally, limited sample size precludes stratified analysis by condition category. While we are able to provide a sensitivity test stratified by all conditions subject to penalties under the HRRP, there may be variation of need for follow-up care, and its relationship

with the outcomes by condition category. Limited sample size also precluded our analysis from examining more precise rural categories; we are unable to determine whether the interaction effect of early follow-up care on 30- and 60-day outcomes varies from small to isolated rural settings.

In conclusion, we found that follow-up care within 14 days of hospital discharge can reduce 30-day and 60-day readmission, ED use, and mortality, and that rural beneficiaries experience a greater beneficial effect on 30-day ED use. Early follow-up care appears to benefit beneficiaries with multiple comorbidities. Investing in efforts to improve follow-up care in rural areas may help reduce costs associated with ED use and readmission for rural providers and Medicare. Furthermore, focusing efforts to ensure an early follow-up visit among beneficiaries with multiple comorbidities may be an efficient use of resources. Further research is needed to better understand how follow-up care may impact Medicare expenditures following hospital discharge.

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SUPPORTING INFORMATION

Additional supporting information may be found in the online version of this article:

Appendix SA1: Author Matrix.

Table SA1: Naïve Logistic Regression Results for 14-Day Follow-up Care on 30- and 60-Day Outcomes.

Table SA2: 2SRI Logistic Regression Results for 14-Day Follow-up Care on 30- and 60-Day Outcomes.