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The Impact of Public Performance Reporting on Market Share, Mortality, and Patient Mix Outcomes Associated With Coronary Artery Bypass Grafts and Percutaneous Coronary Interventions (2000–2016)

A Systematic Review and Meta-Analysis

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Objective: Public performance reporting (PPR) of coronary artery bypass graft (CABG) and percutaneous coronary intervention (PCI) outcomes aim to improve the quality of care in hospitals, surgeons and to inform consumer choice. Past CABG and PCI studies have showed mixed effects of PPR on quality and selection. The aim of this study was to undertake a systematic review and meta-analysis of the impact of PPR on market share, mortality, and patient mix outcomes associated with CABG and PCI.

Methods: Six online databases and 8 previous reviews were searched for the period 2000–2016. Data extraction, quality assessment, systematic critical synthesis, and meta-analysis (where possible) were carried out on included studies.

Results: In total, 22 relevant articles covering mortality (n = 19), patient mix (n = 14), and market share (n = 6) outcomes were identified. Meta-analyses showed that PPR led to a near but not significant reduction in short-term mortality for both CABG and PCI. PPR on CABG showed a positive effect on market share for hospitals (3 of 6 studies) and low-performing surgeons (2 of 2 studies). Five of 6 PCI studies found that high-risk patients were less likely to be treated in States with PPR.

Conclusions: There is some evidence that PPR reduces mortality rates in CABG/PCI-treated patients. The significance of there being no strong evidence, in the period 2000–2016, should be considered. There is need for both further development of PPR practice and further research into the intended and unintended consequences of PPR.

Key Words: public performance reporting, systematic review, coronary artery bypass graft, percutaneous coronary intervention

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The origins of public performance reporting (PPR) in health care can be traced to 2 events. First, the publication of annual mortality rate reports for 17 groups of medical and surgical patients by the US Health Care Financing Administration between 1986 and 1992.[1] Second, concern within the New York State Department of Health regarding the substantial variation in in-hospital mortality rates for coronary artery bypass graft (CABG) surgery around that time leading to the publication of risk-adjusted mortality data for all 28 hospitals performing CABG in that State.[1] Other States and professional bodies followed and PPR is well established in the United States. New York, Pennsylvannia, and Massachusetts have subsequently added reporting of percutaneous coronary intervention (PCI). New York, Pennsylvania, and New Jersey report CABG mortality rates for hospitals and individual surgeons.

The establishment of these cardiac registries and PPR arrangements aimed to improve the quality of care in hospitals by providing incentives for hospitals and surgeons to improve performance, and by empowering patients to make informed decisions when selecting a hospital or a surgeon. Earlier studies found that PPR of CABG and PCI outcomes were associated with quality improvement activities among hospitals and surgeons,[2,3] and a reduction in mortality.[4,5] However, PPR remains controversial with some later studies reporting unintended consequences including risk aversion and denial of care to high-risk patients, for example, avoiding operating[6,7] or out-of-State referrals.[8] Other studies found no such effects.[5] Perhaps as a result, New York began excluding high-risk patients with cardiogenic shock from its analysis of mortality rates in 2006.[1]

A considerable literature on the various effects of PPR on CABG and PCI now exists suggesting the need for a systematic review and meta-analysis. However, while several systematic reviews have been undertaken to summarize research at an all-causes level, none have focused exclusively on CABG and PCI (Appendix A, Supplemental Digital Content 1, http://links.lww.com/MLR/B624).[9–16] Only Campanella et al[15] conducted a relevant meta-analysis. None properly considered the type of impact studied—performance (quality) effects or selection (use of health services) (Appendix B, Supplemental Digital Content 1, http://links.lww.com/MLR/B624). The aim of this study is to undertake a systematic review and meta-analysis of the impacts of PPR on health service quality plus any subsequent changes in usage of the health services whose quality indicators have been publicly reported. The topic areas are PPR and its impacts on market share, mortality, and patient mix associated with treatments involving the use of CABG and PCI.

METHODS

Data Sources and Searches

Six databases were searched for articles published from their inception dates to April 16, 2015: Medline; Embase; Psyinfo; the Cumulative Index to Nursing and Allied Health Literature; Evidence-Based Medicine Reviews; and EconLit. Our search strategy was based on Ketelaar et al[13] study which covered: randomized controlled trials (RCTs); cluster RCTs; quasirandomized trials; cluster quasirandomized trials; interrupted time series studies; and controlled before-after studies. We extended our search to include cross-sectional designs where these conformed to the Meta-analysis of Observational Studies in Epidemiology guidelines.[17] Search terms were amended with the assistance of a librarian (Appendix C, Supplemental Digital Content 1, http://links.lww.com/MLR/B624). Results of searches were downloaded into Endnote X7.

The search strategy was later extended because, when comparing the output of our search with that of other systematic reviews, particularly Campanella et al,[15] it became clear that a number of studies had been conducted by non-epidemiologists (eg, health economists) with these study designs not using standard epidemiological descriptors. A second search of the databases was conducted on November 14, 2016 to include: experimental study; non-randomized study; observational cohort; time trend; and comparative study. Articles were also screened from previous systematic reviews on PPR.[12,13,15,16,18–20] Articles published before 2000 were not included because the practice of PPR, before then was significantly different. This is because of the widespread use of PPR online in the 2000s and the growth of PCI as a substitute for CABG in the mid-1990s which may have had the added effect of changing the overall population receiving CABG in the 2000s.[21,22]

Study Selection

Articles were included if: (1) they examined the effect of PPR on outcomes among health care purchasers, providers, or consumers; (2) the study design was observational or experimental. Articles were excluded if: (1) performance reporting was not publicly disclosed; (2) they reported hypothetical choices; (3) the study design was qualitative; (4) they were published in languages other than English; (5) they were published before the year 2000; (6) where pay-for-performance effects were not disaggregated from PPR; and (7) if they involved long-term care.

Two authors independently screened at the title and abstract level for relevance. The remaining articles were screened at full-text level. A screening guide adapted from a previous study was used (Appendix D, Supplemental Digital Content 1, http://links.lww.com/MLR/B624).[23] Discrepancies between authors were discussed between them and if they remained unresolved, a third author made the final decision. Studies were grouped per (a) the type of provider/service whose performance was being publicly reported and (b) the effect of impact of PPR (an improvement in performance or a selection/change in health service usage either by provider or consumer).

Data Extraction and Quality Assessment

The following information was extracted from the articles: authors, year of publication, country, study design, study population, sample size, type of PPR data, outcome measures, statistical analysis, and findings. A risk of bias assessment was then made. The methodological quality of observational studies was assessed with the Newcastle-Ottawa Scale (NOS)[24] and RCT studies with the Cochrane Collaboration's tool.[25] Both tools are commonly used in systematic reviews[26,27] and previous evaluation studies have shown satisfactory psychometric properties.[28,29] The NOS uses a star system to evaluate the quality of the studies based on 3 domains: the selection of the study groups; the comparability of the groups; and the ascertainment of either the exposure/outcome of interest (Appendix E, Supplemental Digital Content 1, http://links.lww.com/MLR/B624). The Cochrane Collaboration's tool uses 6 domains to evaluate the quality of RCT studies: selection bias; performance bias; detection bias; attrition bias; selective reporting; and other sources of bias. These methods of bias assessment ensure that the only studies included are ones where the effects of potentially confounding variables on study findings have been adjusted for. For example, the effects of changes in risk levels in populations over time on mortality or other outcomes, as a result of risk-averse practices.

Data Synthesis and Analysis

Effect size estimates were extracted from the studies where possible by one author and reviewed by a second author. Pooled random effect size estimate was calculated using Comprehensive Meta-Analysis software version 3 (Biostat, Englewood, NJ). Studies that did not report appropriate/sufficient data were not included in the meta-analysis but retained in the systematic review. A random effect was selected to account for the heterogeneity of the measures across the studies. Heterogeneity was calculated with the I^2 statistics. I^2 describes the percentage of total variation across studies that are due to heterogeneity rather than chance, with a value of 0% indicating no observed heterogeneity and larger values indicating increasing heterogeneity. I^2 values of 25%, 50%, and 75%, correspond to low, moderate, and high levels of heterogeneity, respectively.[30] Publication bias was assessed with the Egger test. Egger is a statistical test that detects asymmetry in a funnel plot, where the null hypothesis denotes no publication bias (symmetry) and the alternative hypothesis indicates publication bias (asymmetry).[31]

RESULTS

Study Selection and Quality Assessment

We identified 5961 records through our search and previous reviews search (Fig. 1). Following titles and abstracts screening, 5875 records were excluded, leaving 86 articles for full-text screening. Following this, 32 articles were excluded including 11 that were deemed low quality. An additional 5 articles were included via hand search and 1 from the EconLit search. We included 60 articles in our synthesis. These were categorized into 4 groups: (1) CABG and PCI; (2) health plans; (3) hospital performance; and (4) physician performance. Results of the latter 3 will be reported elsewhere. We found 22 studies examining the impact of public reporting of CABG and PCI performance data on market share, mortality, and patient mix outcomes. All studies were rated as moderate quality.

Study Characteristics

Characteristics of the included CABG and PCI studies are described in Tables 1–3 by outcome type. There were 13 CABG studies, 6 PCI studies and 3 studies that included both CABG and PCI samples. All studies were published between

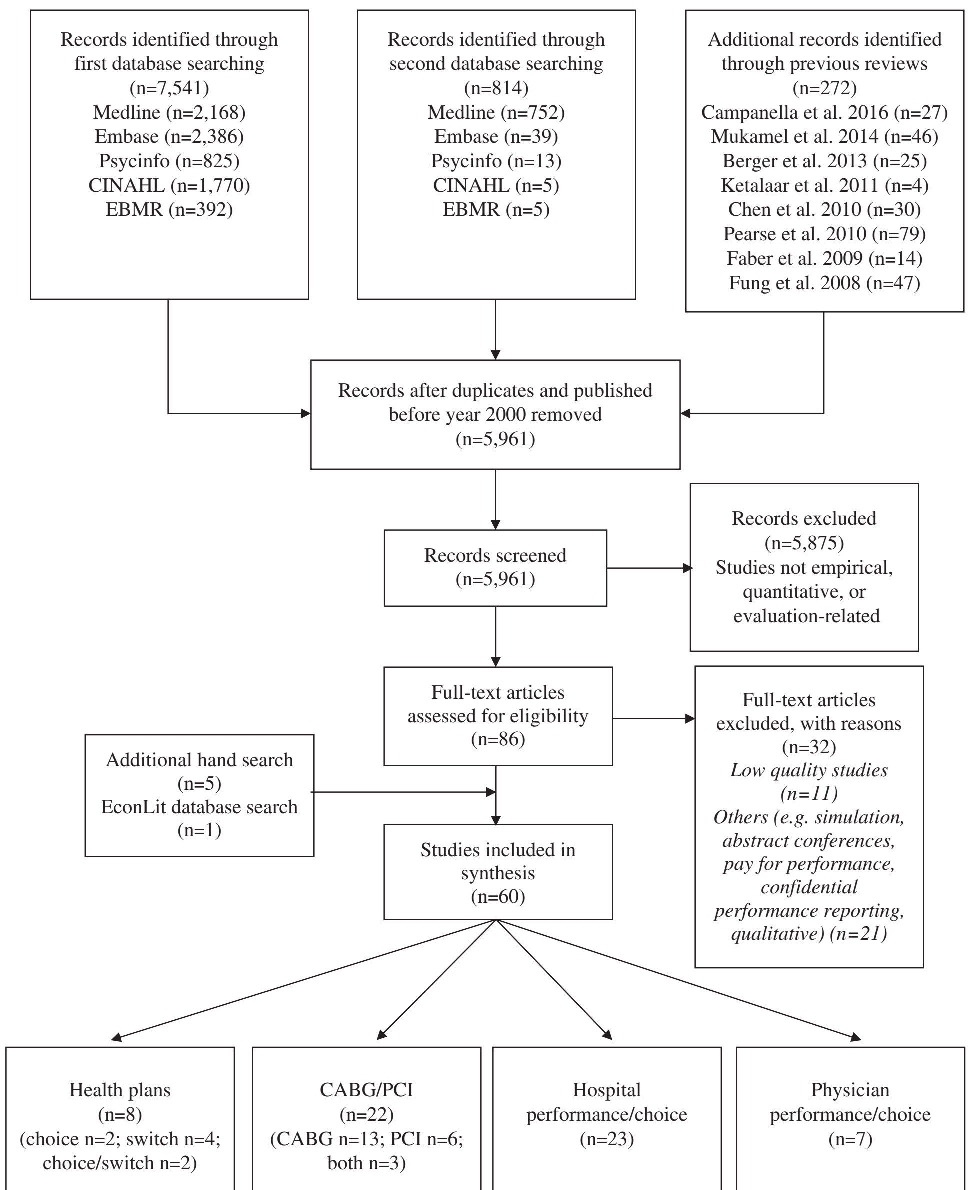

FIGURE 1. Flow diagram for retrieval of articles. CABG indicates coronary artery bypass graft; PCI, percutaneous coronary intervention.

TABLE 1. Data Extractions for Market Share Outcome Studies (n = 6)

References	Country	Study Design	Study Population	Sample Size	Type of PPR	Outcomes	Statistical Analysis	Findings
Romano and Zhou[32]	US	Interrupted time series (without comparison group)	CABG	Not provided	Report cards	Market share	OLS and ARIMA	Effect: very temporary effects only
Jha and Epstein[33]	US	Retrospective cohort study	CABG	Not provided	Report cards	Market share	Linear regression model	No effect for hospitals but 15% more high-mortality surgeons retired
Dranove and Sfekas[36]	US	Before and after study	CABG	23,854	Report cards	Market share	Econometric method	No effect for high-ranking hospitals but a reduction in market share for hospitals with "negative news"
Romano et al[34]	US	Before and after study	CABG	169,718	Report cards	Market share	Paired student *t* test, multivariable linear mixed regression	Effect: low and high-mortality hospitals (market share +8.9%; −5%) in first 6 mo
Wang et al[37]	US	Retrospective cohort study	CABG	114,039	Report cards	Market share	Mixed logit model	No effect for hospitals but more high-mortality surgeons retired
Shukla[35]	US	Nonrandomized controlled trial	CABG	281,818	Report cards	Market share	Difference in differences OLS regression	Effect: low and average mortality outlier hospitals (market share +6%; −8%)

ARIMA indicates autoregressive integrated moving average; CABG, coronary artery bypass graft; OLS, ordinary least squares; PPR, public performance reporting.

2003 and 2016. In total, 21 studies were published in academic journals and 1 was a PhD dissertation. Studies were conducted in the United States (n = 19), Canada (n = 1), Italy (n = 1), and the UK (n = 1). Study designs included non-RCT quasiexperiment (n = 12), before and after (n = 6), retrospective cohort (n = 2), and time series (n = 2). The total sample size across all studies excluding two[32,33] (not provided) consisted of 4,201,388 participants. The sample size per study ranged from 545 to 967,882. The most common type of PPR were report cards (n = 16). Outcomes examined included market share (n = 6), mortality (n = 19), and patient mix outcomes (n = 14). The total number of outcomes does not reflect the total number of studies given that many studies examined > 1 outcome.

Effects of PPR on Market Share (CABG)

Six of the 13 CABG studies examined the effects of report cards on hospitals market share.[32–37] Romano et al[34] and Shukla[35] reported an increase in mean market share of low-mortality outlier hospitals and decrease in high-mortality hospitals postrelease of report cards. Dranove and Sfekas[36] found that, while high-ranking hospitals in New York reported no effect of report card scores on market share, those hospitals with "negative news" in the original report experienced a decrease in market share. Jha and Epstein[33] and Wang et al[37] reported no effect of report cards on market share for both high and low-performing hospitals. Romano and Zhou[32] reported only very temporary effects.

However, Jha and Epstein[33] and Wang et al[37] reported a higher proportion of poorly performing surgeons had retired postrelease of the report cards. The former found that > 20% of surgeons in the bottom quartile (ie, those with high-risk-adjusted mortality rates) stopped practicing CABG surgery within 2 years after the release of the reports, in comparison to 5% of surgeons in the top quartile. No meta-analysis was conducted as we were only able to extract data from 2 studies.[33,34]

Effects of PPR on Mortality (CABG)

Ten of the 13 CABG studies examined the effect of PPR on mortality.[33–35,38–44] Definitions of mortality varied across the studies: operative mortality (n = 3)[34,38,39]; in-hospital mortality (n = 4)[35,42–44]; 30-day mortality (n = 1)[40]; mortality within 1 year of admission (n = 1)[41]; and mortality undefined (n = 1).[33] Five studies reported no changes in mortality rates.[34,35,38,40,42] In contrast, Li et al,[39] Hannan et al,[43] Jha and Epstein,[33] Dranove et al,[41] and Chou et al[44] found a significant reduction in mortality rates following the dissemination of report cards. Jha and Epstein[33] did so by reporting changes in risk-adjusted mortality rates for high-performing and low-performing hospitals and surgeons after the introduction of report cards. Dranove et al[41] concluded that the decline in 12-month mortality rates for a CABG population but not an acute myocardial infarction (AMI) population, that was partly but not fully risk-adjusted, was due to a shift in patient mix of CABG procedures toward healthier patients rather than report cards. Chou et al[44] reported a 5% to 10% reduction in mortality in more competitive hospital markets.

A meta-analysis was conducted on 5 of the 8 short-term mortality studies (Fig. 2A), as we were unable to extract data from 3 studies.[35,40,44] Result of the random-effects meta-analysis indicated that PPR was associated with reduced short-term mortality; however, this was not statistically significant [odds ratio (OR) = 0.86; 95% confidence interval (CI) = 0.71-1.04; $P = 0.11$]. Substantial heterogeneity was observed between effect sizes ($I^2 = 91.52\%$). Result of the Egger test was not statistically significant ($P = 0.33$).

Effects of PPR on Mortality (PCI)

Six of the 6 PCI studies examined the effect of PPR on 30-day mortality (n = 2)[45,46] and in-hospital mortality

TABLE 2. Data Extractions for Mortality Outcome Studies (n = 19)

References	Country	Study Design	Study Population	Sample Size	Type of PPR	Outcomes	Statistical Analysis	Findings
Dranove et al[41]	US	Before and after study	CABG	967,882	Report cards	Mortality within 1 y of admission	Difference in differences regression	Effect: a decrease in mortality rate in states with PPR (NY and PA) compared with states without PPR
Hannan et al[43]	US	Non-RCT	CABG	911,407	Report cards	In-hospital/ 30-d mortality	Stepwise logistic regression	Effect: lower mortality rates in states with PPR (Northern New England, northeastern OH, NJ, NY, and PA) compared with the rest of the USA
Moscucci et al[50]	US	Non-RCT	PCI	80,422	PCI mortality of operator and hospital-specific outcomes	In-hospital mortality	Multivariate logistic regression	No effect
Guru et al[40]	Canada	Before and after study	CABG	67,693	Report cards	30-d mortality	Logistic regression	No effect
Jha and Epstein[33]	US	Retrospective cohort study	CABG	Not provided	Report cards	Mortality (undefined)	Linear regression model	Effect: lower mortality rates among high-performing hospitals and surgeons compared with low-performing hospitals and surgeons in NY (PPR state)
Khan et al[38]	UK	Before and after study	CABG	2111	Surgeon-specific data	Operative mortality	1-way analysis of variance, χ^2, Fisher exact test	No effect
Apolito et al[51]	US	Non-RCT	AMI including CABG/PCI	545	NY state cardiac surgery and PCI reporting system	In-hospital mortality	Multivariate logistic regression	Effect
Li et al[39]	US	Before and after study	CABG	36,923	Report cards	Operative mortality	Multivariable logistic regression	Effect: a decrease in mortality rate for most hospitals in CA (PPR state)
Romano et al[34]	US	Before and after study	CABG	169,718	Report cards	Operative mortality	Paired student *t* test, multivariable linear mixed regression	No effect
Chen and Meinecke[42]	US	Non-RCT	Heart problems including CABG	952,200	Report cards	In-patient mortality	Difference in differences regression	No effect
Joynt et al[45]	US	Non-RCT	AMI including PCI	97,802	Report performance on PCI	30-d mortality	Hierarchical logistic regression	No effect
McCabe et al[48]	US	Interrupted time series	PCI	116,227	PCI in-hospital mortality rate	In-hospital mortality	Generalized estimating equations for multivariate regression	Effect: a decrease in mortality rate in MA (PPR state)
Chou et al[44]	US	Non-RCT	CABG	76,862	Online report cards	In-hospital mortality	Conditional logit models	Effect: lower mortality rates in competitive and most competitive compared with other markets among the more severely ill patients
Renzi et al[46]	Italy	Non-RCT	AMI including PCI	64,150	Website	30-d mortality	Logistic regression	No effect

(*Continued*)

TABLE 2. Data Extractions for Mortality Outcome Studies (n = 19) (*continued*)

References	Country	Study Design	Study Population	Sample Size	Type of PPR	Outcomes	Statistical Analysis	Findings
Shukla[35]	US	Nonrandomized controlled trial	CABG	281,818	Report cards	In-hospital mortality	Difference in differences OLS regression	No effect
Boyden et al[49]	US	Non-RCT	PCI	105,511	Report cards	In-hospital mortality	Logistic regression	Effect: a decrease in mortality rate in state with PPR (NY) compared with state without PPR (MI)
Waldo et al[47]	US	Non-RCT	AMI including PCI	84,121	Report cards	In-hospital mortality	Logistic regression	Effect: lower mortality rate in states with PPR (MA and NY), compared with states without PPR (CT, ME, MD, NH, RI, and VT)
Bangalore et al[53]	US	Non-RCT	AMI with cardiogenic shock (PCI/CABG)	2126	Report cards	In-hospital mortality (for PCI/CABG)	Multivariate logistic regression	Effect: a decrease in mortality rate in state with PPR (NY) and state without PPR (MI) over time. No differences between NY and MI
McCabe et al[52]	US	Non-RCT	AMI with cardiogenic shock (PCI/CABG)	45,977	Report cards	In-hospital mortality (for PCI/CABG)	Poisson regression	Effect: lower mortality rate in state with PPR (NY) compared with states without PPR (MA, MI, NJ, and CA)

AMI indicates acute myocardial infarction; CA, California; CABG, coronary artery bypass graft; CT, Connecticut; MA, Massachusetts; MD, Maryland; ME, Maine; MI, Michigan; NH, New Hampshire; NJ, New Jersey; NY, New York; OH, Ohio; OLS, ordinary least squares; PA, Pennsylvania; PCI, percutaneous coronary intervention; PPR public performance reporting; RCT, randomized controlled trial; RI, Rhode Island; VT, Vermont.

(n = 4).[47–50] A further 3 studies examined in-hospital mortality for both CABG/PCI samples.[51–53] Three studies reported no differences in 30-day mortality among AMI patients undergoing PCI in State(s) with and without PPR.[45,46,50] In contrast, Waldo et al,[47] McCabe et al,[48] and Boyden et al,[49] reported lower in-hospital mortality rate for AMI patients treated in States with PPR relative to States without PPR. Waldo et al[47] also reported higher in-hospital mortality rate among AMI patients who did not undergo PCI in States with PPR, compared with States without PPR. These different outcomes in the PCI and non-PCI populations, each determined after appropriate risk adjustment, were attributed by the authors to different risk-severity levels in the 2 populations as a result of risk-averse practices among surgeons. McCabe et al[48] also concluded that risk-averse practices among surgeons were responsible for their findings. However, they reported only unadjusted observed rates (and adjusted predicted mortality rates).

Among AMI patients with cardiogenic shock, Apolito et al[51] reported no difference in in-hospital mortality for AMI patients with cardiogenic shock treated with PCI/CABG, though an increase in those not revascularized in New York compared with States without PPR. In contrast, McCabe et al[52] reported lower in-hospital mortality rate for patients with cardiogenic shock undergoing PCI/CABG in New York compared with States without PPR following a change in PPR of mortality rates in New York in 2006 to exclude AMI patients with cardiogenic shock from analysis. Bangalore et al[53] reported lower in-hospital mortality rate for patients with cardiogenic shock undergoing PCI, over time for both New York and Michigan (non-PPR State) but no differences between the States at each timepoint.

A meta-analysis was conducted on the 5 short-term mortality studies (Fig. 2B) with the exception of McCabe et al[48] (unable to extract data) and the AMI patients with cardiogenic shock studies.[51–53] Result of the random effect meta-analysis indicated that PPR was associated with reduced short-term mortality but this was not statistically significant (OR = 0.86; 95% CI = 0.71-1.05; $P = 0.15$). Substantial heterogeneity was observed between effect sizes ($I^2 = 87.33$). Result of the Egger test was not statistically significant ($P = 0.92$).

Effects of PPR on Patient Mix (CABG)

Six of the 13 CABG studies examined the effect of report cards on patient mix.[34,37,39,41–43] An additional 2

TABLE 3. Data Extractions for Patient Mix Outcomes Studies (n = 14)

References	Country	Study Design	Study Population	Sample Size	Type of PPR	Outcomes	Statistical Analysis	Findings
Dranove et al[41]	US	Before and after study	CABG	967,882	Report cards	Patient mix (incidence and quantity of CABG surgery and matching of patients to providers)	Difference in differences regression	Effect: a decline in the illness severity of patients treated with CABG in states with PPR (NY and PA) compared with states without PPR
Hannan et al[43]	US	Non-RCT	CABG	911,407	Report cards	Patient mix (proportion of high-risk patients)	Stepwise logistic regression	No effect
Moscucci et al[50]	US	Non-RCT	PCI	80,422	PCI mortality of operator and hospital-specific outcomes	Patient mix (comorbidities and indications for PCI)	Multivariate logistic regression	Effect: patients with comorbidities were more likely to be treated with PCI in state without PPR (MI) than in state with PPR (NY)
Apolito et al[51]	US	Non-RCT	AMI including CABG/ PCI	545	NY state cardiac surgery and PCI reporting system	Patient mix (coronary angiography and revascularization to high-risk patients)	Multivariate logistic regression	Effect: high-risk patients were less likely to be treated with CABG/PCI in state with PPR (NY) compared with states without PPR (non-NY centers)
Li et al[39]	US	Before and after study	CABG	36,923	Report cards	Patient mix (patient preoperative clinical profiles)	Multivariable logistic regression	No effect
Romano et al[34]	US	Before and after study	CABG	169,718	Report cards	Patient mix (mean expected mortality)	Paired student *t* test, multivariable linear mixed regression	Effect: sick patients were less likely to be treated with CABG by high-mortality outlier hospitals in CA (PPR state)
Wang et al[37]	US	Retrospective cohort study	CABG	114,039	Report cards	Patient mix (matching between patients and providers)	Mixed logit model	Effect: patients were less likely to be treated with CABG by high-mortality outlier hospitals and low-performing surgeons in PA (PPR state)
Chen and Meinecke[42]	US	Non-RCT	Heart problems including CABG	952,200	Report cards	Patient mix (average mortality rate)	Difference in differences regression	No effect
Joynt et al[45]	US	Non-RCT	AMI including PCI	97,802	Report performance on PCI	Patient mix	Hierarchical logistic regression	Effect: high-risk patients (ie, ST-segment elevation, cardiogenic shock, and cardiac arrest) were less likely to be treated with PCI in states with PPR (NY, MA, and PA), compared with states without PPR (ME, VT, NH, CT, RI, MD, and DE)
McCabe et al[48]	US	Interrupted time series	PCI	116,227	PCI in-hospital mortality rate	Patient mix (outlier's hospitals)	Generalized estimating equations for multivariate regression	Effect: high-risk patients were less likely to be treated with PCI in negative outlier hospitals compared with nonoutlier hospitals in MA (PPR state)
Boyden et al[49]	US	Non-RCT	PCI	105,511	Report cards	Patient mix (proportion of patients with NSTEMI, STEMI and cardiogenic shock)	Logistic regression	Effect: patients with comorbidities and high-risk patients were more likely to be treated with PCI in state without PPR (MI) than state with PPR (NY)
Waldo et al[47]	US	Non-RCT	AMI including PCI	84,121	Report cards	Patient mix (proportion of patients with cardiogenic shock, cardiac arrest, or STEMI)	Logistic regression	Effect: high-risk patients (ie, older age, medicare insurance, STEMI, cardiogenic shock or cardiac arrest) were more likely to be treated with PCI in states without PPR (CT, ME, MD, NH, RI, and VT) than states with PPR (MA and NY)

(*Continued*)

TABLE 3. Data Extractions for Patient Mix Outcomes Studies (n = 14) (*continued*)

References	Country	Study Design	Study Population	Sample Size	Type of PPR	Outcomes	Statistical Analysis	Findings
Bangalore et al[53]	US	Non-RCT	AMI with cardio-genic shock (PCI/CABG)	2126	Report cards	Patient mix (proportion of patients undergoing PCI/CABG)	Multivariate logistic regression	Effect for PCI but not CABG: an increase in PCI performed in state with PPR (NY) and state without PPR (MI) over time but overall rate in NY remained much lower than the other states (MI, NJ, and CA)
McCabe et al[52]	US	Non-RCT	AMI with cardio-genic shock (PCI/CABG)	45,977	Report cards	Patient mix (proportion of patients undergoing PCI/CABG)	Poisson regression	Effect for PCI but not CABG; an increase in PCI performed in state with PPR (NY) compared with states without PPR (MA, MI, NJ, and CA) but overall rate in NY remained much lower than the other states

AMI indicates acute myocardial infarction; CA, California; CABG, coronary artery bypass graft; CT, Connecticut; DE, Delaware; MA, Massachusetts; MD, Maryland; ME, Maine; MI, Michigan; NH, New Hampshire; NSTEMI, non-ST-segment elevation myocardial infarction; NY, New York; PA, Pennsylvania; PCI, percutaneous coronary intervention; PPR, public performance reporting; RCT, randomized controlled trial; RI, Rhode Island; STEMI, segment elevation myocardial infarction; VT, Vermont.

studies focused solely on CABG among AMI patients with cardiogenic shock.[52,53] Romano et al[34] concluded that the release of hospital performance reports in California was associated with increased volume at low-mortality hospitals, and may have reduced referrals of high-risk patients to high-mortality hospitals. Dranove et al[41] reported that illness severity fell in patients receiving CABG following report cards release. However, the authors also found that the proportion of severe cases of AMI in teaching hospitals increased in States with PPR. On the contrary, 4 studies[37,39,42,43]

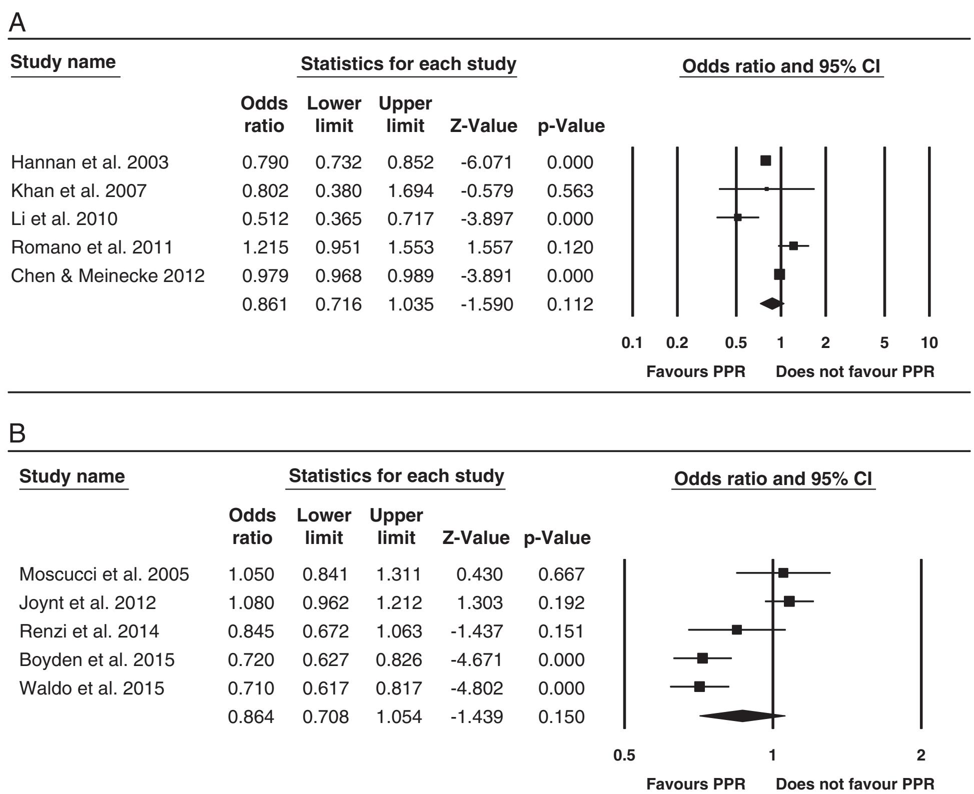

FIGURE 2. Forest plots of the association between PPR and short-term mortality for coronary artery bypass graft studies (A); PPR and short-term mortality for percutaneous coronary intervention studies (B). CI indicates confidence interval; OR, odds ratio; PPR, public performance reporting.

reported no changes in overall patient case mix, concluding there was no decrease in access for high-risk patients receiving CABG surgery. Hannan et al[43] reported a higher proportion of high-risk patients undergoing CABG surgery in States with PPR than the rest of the country.

Among AMI patients with cardiogenic shock, McCabe et al[52] and Bangalore et al[53] reported no change in the proportion of patients who underwent CABG in New York after the exclusion of cardiogenic shock from PPR on mortality rates in 2006. Given the various measures of patient mix across the studies, no meta-analysis was conducted.

Effects of PPR on Patient Mix (PCI)

Five of the 6 studies examined the effect of PPR on AMI patient mix.[45,47–50] Three additional studies investigated the impact of PPR on patient mix: 1 comprised both PCI and CABG populations[51] and 2 studies focused solely on AMI patients with cardiogenic shock.[52,53] All 5 studies found differences in AMI patient mix.[45,47–50] They reported that high-risk patients were less likely to be treated with PCI in States with PPR, compared with States without PPR. Similarly, among AMI patients with cardiogenic shock, Apolito et al[51] reported that high-risk patients with cardiogenic shock in New York were less likely to undergo CABG/PCI treatments than States without PPR. McCabe et al[52] and Bangalore et al[53] found a substantial increase in PCI being performed following the exclusion of patients with cardiogenic shock from PPR of mortality rates in New York. However, the overall rate of PCI performed in New York remained much lower than in States without PPR. Given the different definitions of patient mix, no meta-analysis was conducted.

DISCUSSION

Findings varied across type of outcomes and procedures. For short-term mortality and CABG, 5 of the 10 studies reported a reduction in mortality.[33,39,41,43,44] Meta-analysis of a subset of studies indicated a near significant decline (OR = 0.86; 95% CI = 0.71-1.04; $P = 0.11$). For short-term mortality and PCI, 3 of the 6 studies[47–49] reported a reduction in mortality. Meta-analysis of a subset of studies indicated a near significant decline (OR = 0.86; 95% CI = 0.71-1.05; $P = 0.15$).

For market share and CABG, the results provided some evidence with 3 of the 6 studies indicating an increase in market share in low-mortality hospitals,[32,34,35] 2 of these also showing a decrease in market share in high-mortality hospitals.[32,34] Two studies reported withdrawal from practice by poorly performing surgeons.[33,37] For patient mix and PCI, the results provided moderate evidence with 5 of the 6 studies reporting a change in mix.[45,47–50] In 4 studies, the change was toward PCI in patients with reduced severity of disease.

Our mortality findings, although not statistically significant, are similar to those previously reported. Campanella et al[15] reported that PPR was associated with reduced mortality for cardiovascular diseases [risk ratio, 0.83 (95% CI, 0.77-0.91; $P < 0.0001$; $I^2 = 95\%$)] based on a meta-analysis of 6 studies. The difference is likely due to: (1) 7 additional studies, not considered by Campanella et al[15] being included in our meta-analyses; (2) our treatments were stratified (CABG vs. PCI); (3) including studies in our meta-analyses, that only focused on short-term mortality; (4) time period restricted to 2000 onwards; (5) consideration of study quality; and (6) inability to extract data from Guru et al[40] for meta-analysis—though retained in review. Totten et al[14] reported that 8 of 13 studies reported small declines in mortality of cardiac reporting programs.

Although there is some evidence of PPR reducing mortality rates in CABG/PCI-treated patients, evidence for major effects, > 25 years after the introduction of PPR does not exist. This is a matter of some concern. Akin to previous reviews, we have identified, not for the first time, both positive and other effects of PPR. The former includes some movement in the treatment of patients from high-mortality to low-mortality hospitals and the withdrawal/retirement of low-performing surgeons. The latter includes risk-averse practices for PCI patients by their doctors. However, this should not explain reduction in mortality rates in the treated population in studies with proper risk adjustment. The significance of risk aversion, however, may be complex—detrimental for AMI patients with cardiogenic shock (now largely avoided in New York as these cases are no longer subject to PPR), beneficial for patients with coronary artery disease with multivessel coronary artery disease and or concomitant diabetes mellitus.[54] Perhaps, the reduction in mortality rates may be attributed to surgeons wanting to maintain or improve their reputation by ceasing to perform inadvisable procedures on potentially unsalvageable patients. This requires further study. Other system impacts can also be further researched (eg, the workforce impacts of the withdrawal of low-performing surgeons).

PPR practice could also be improved. Wasfy et al[55] have argued for a shift in PPR focus from procedures to disease-based population health. Positive impacts on patients undergoing the relevant procedures may obscure the fact that negative effects may, as an indirect consequence, occur in patients not undergoing the relevant procedure. The results of Waldo et al,[47] while only 1 study, attest to this. They reported higher mortality rates among patients with AMI not treated with PCI in States with PPR, compared with States without PPR. Therefore, PPR effects on patients not treated by CABG/PCI also requires further study.

Wasfy et al[55] have argued that better measures of outcomes are desirable. Mortality, while being easy to measure and to understand, may not be the best measure of quality as it is of low frequency and differences may not discriminate well between provider groups. Other outcome measures like postprocedure angina, revascularizations performed, and process measures may be desirable. Publicly reporting these outcomes can drive improvement in the delivery of care as providers identify underperforming areas. For consumers, transparency and accountability of providers can increase awareness, trust, and confidence in the health system, and support health care decision-making.

Hannan[1] has argued for improvements in the completeness, accuracy, and risk-adjustment of rates. This is necessary both to overcome "gaming the system" and to build confidence in the results. The further audit of results, as reported by hospitals, may be necessary. Both Hannan[1] and

Wasfy et al[55] agree that the use of large administrative databases should be avoided as they do not properly record clinical data, such as diagnosis and risk factors.

Both Hannan[1] and Wasfy et al[55] argued that the involvement of multiple-constituency stakeholders including experts, providers, and consumers is desirable in the development of PPR systems to promote public acceptance, use, and impact. Finally, PPR is only 1 quality assurance approach and is perhaps best undertaken in conjunction with other approaches such as Continuous Quality Improvement, Pay-for-Performance, and Evidence-based/clinical guidelines.

Limitations

We did not include articles published pre-2000. This means that impacts of PPR, particularly positive impacts, could have occurred in the 1990s and we would not have detected these. The search did not include studies in languages other than English, gray literature or qualitative studies. Studies that did not explicitly describe their research design may have also been missed. Results of the meta-analyses should be interpreted with caution as only a subset of studies were suitable for meta-analysis. In addition, there were high levels of heterogeneity, likely due to the small number of studies and the inclusion of various study designs.[30] Subgroup analysis was not possible, particularly to look at PPR effects at different times in this 16-year period. This would have been beneficial, as studies conducted in the same States (CABG in California—Romano et al[34] and Li et al[39]) and (PCI in New York compared with Michigan—Moscucci et al[50] and Boyden et al[49]) showed somewhat more positive PPR effects in the study conducted later. The study period difference for Moscucci et al[50] and Boyden et al[49] was 13 years (1998–1999 to 2011–2012). Romano et al[34] and Li et al[39] were closer together—the former, 1997–2002, the latter 2003–2006. However, there was voluntary participation in the former (1997–2002) compared with mandated hospital participation in the latter (2003–2006). The literature has overwhelmingly been derived from 1 country and 1 health system (United States). It should also be noted that meta-analyses being oriented to average effects are insensitive to differences in study results due to differences in context and minor methodological differences between individual studies. These should be further studied.

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