



Optimal slices of the healthcare spending pie: can traditional comparative effectiveness research address resource allocation?

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Substantial attention has been focused upon rising healthcare costs with resulting concern about sustainability. USA spending rose to 5.3% in 2014 (exceeding US\$3.0 trillion, US\$9523/person, approaching 18% of US economy) and is expected to grow at an annual rate of 5.8% through 2024 [1]. This rate will likely exceed gross domestic product growth by 1.1% per year.

As part of the solution, the Institute of Medicine estimates that approximately 30% of healthcare spending (US\$750 billion/year) could be avoided, a significant portion due to unnecessary services [2]. In a 2014 survey by the American Board of Internal Medicine, nearly three of four US physicians identified ordering unnecessary tests and procedures as a substantial problem [3]. Another study estimated that up to 42% of medicare beneficiaries received at least one low-value service [4]. Examination of individual services has corroborated these findings: nearly one in five women who undergo hysterectomy may not need the procedure [5], and 22.5% of implantable cardiac defibrillators are unnecessary [6].

Despite concerns about the growth of the healthcare pie, little attention has been paid to the spending upon slices within it: are resources appropriately allocated among medications, surgery, devices, diagnostic imaging or hospital care? Should low-value spending in one area be better allocated to another (e.g., from surgery to diagnostic imaging or the reverse)? How might one assess whether the allocation is optimal? Can traditional comparative effectiveness research (CER) methods (e.g., randomized trials, database analyses, cohort studies) answer these macrolevel questions, or is a different approach needed?

CER typically compares one diagnostic or therapeutic approach with another, and can provide insights into a microview of resource allocation. For example, the Spine Patient Outcomes Research Trial used a randomized controlled trial design and showed that more costly surgical treatment for lumbar disk herniation was not superior to less costly nonoperative care in pain reduction or improvement in ability to work. From this study, policy-makers might consider reallocating a portion of lumbar surgical dollars to physical therapy practitioners. Similar CER study designs showed that in patients with chronic stable chest pain, drug-eluting cardiac stents were little better than medications for preventing clinical events or improving quality of life (COURAGE Trial) [7] and in a study comparing imaging options to identify breast cancer, mammography with MRI was superior to ultrasound [8]. Traditional CER could be used to also assess clinical outcomes, patient wellbeing and resource impact for robotic versus standard surgery, insulin pumps versus injections, proton beam versus usual radiation therapy. Evidence from studies like these could theoretically help in resource allocation decisions. However, would it be logistically feasible, affordable or efficient?

The Patient Centered Outcomes Research Institute has developed the Patient Centered Outcomes Research Network to explore questions like these. But, at perhaps US\$10 million per pragmatic clinical trial, the costs to compare every set of services would be prohibitive. Randomized trials, cohort studies or administrative database analysis may not be ideally suited for this issue.

Perhaps resource allocation questions could be answered using an alternative

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design: step one: quantify the historic gains in patient wellbeing, and step two: partition those gains among improvements in surgery, hospital care, devices, medications and imaging. Are the gains from advancements in each area commensurate with their cost growth (i.e., providing substantial health gains with modest spending increase? Providing minimal health gains with large spending increases? Or providing health gains proportional to changes in spending)? Decision makers might find this type of evidence useful for policies related to reducing low-value care and improving resource allocation.

At a more granular level, a study using this design might begin by identifying improvements in patient wellbeing for key diseases. The Global Burden of Disease project quantifies health loss from 291 diseases and injuries across 187 countries. It publishes data on years of life lost (YLL; mortality), years of life with disability (morbidity) and a composite measure, disability adjusted life years (DALYs) which combines the two. In 1990, the conditions causing the highest US DALYs (excluding injury) were, in order: ischemic heart disease, lung cancer, chronic obstructive pulmonary disease (COPD), stroke, low back pain, major depressive disorder, other musculoskeletal diseases, diabetes, neck pain and HIV/AIDS. Similar data are available for 2010. On an age-adjusted basis for each disease, how much improvement was there in YLL, years of life with disability and DALYs? As an example, between 1990 and 2010, ischemic heart disease had a 43.6% reduction in age-standardized death rates, and a 45.2% reduction in YLL (USA) [9].

The next design phase would determine the likely causes of those improvements in cardiovascular morbidity and mortality (and similarly for the other conditions). Albeit subjective, a panel of cardiovascular providers (e.g., noninvasive cardiologists, invasive cardiologists, cardiovascular surgeons) could estimate the contribution toward those reductions in disease burden due to: better diagnostic screening (e.g., low-density lipoprotein testing, noninvasive stress imaging), new medications (e.g., statins, new antithrombotics), development of drug eluting cardiac stents or enhancements in cardiac surgery. A systematic review of the literature on key changes in cardiac care might assist the panel in those assessments. Perhaps the panel estimates that 60% of the health gains reflect new medications, 25%

are due to innovations in non-invasive imaging and 15% relate to improved surgical care.

Third, across all of the key conditions, what contributed to the greatest reductions in disease burden? One might hypothesize, for cardiovascular disease, major depressive disorder, COPD, diabetes and HIV/AIDS, new medications may have been the primary driver (for instance, 70% of the attributable health gains). For stroke and neck pain, perhaps surgical improvements were predominant. And for lung cancer, better modes of screening (imaging) might have been most influential.

Finally, compare the contributions to health improvement with the amount of growth in spending for each modality. In aggregate, were increases in expenditures on imaging commensurate with attributed increases in health (hypothetically, 30% of the spending growth but only 10% of the health gains)? Or for the top conditions, were increases in drug expenditures from 1990 to 2010 more impactful (e.g., 40% of the health gains and 20% of the expenditure growth)?

The above design has strength in quantifying improvements in health over time. The determination of what caused those improvements and partitioning them among resource types has more subjectivity and less certainty. However, since the funds, resources and time would likely not enable the more granular traditional CER methodology for every service compared with every alternative, perhaps the more macrodesign, even with its noted limitations might provide adequate information. With concerns about the growing healthcare pie and uncertainty about the optimal size of the slices within it, good policy making requires good information. In its absence, those policies may misallocate scarce resources. Perhaps creative alternatives to traditional designs might be useful to support these types of decisions.

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