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Long-term Projections of the Number Needed to Screen and Additional Number Needed to Treat in Prostate Cancer Screening

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Abstract

Objective—To project long-term estimates of the number needed to screen (NNS) and the additional number needed to treat (NNT) to prevent one prostate cancer death with prostate-specific antigen screening in Europe and in the US.

Study Design and Setting—Mathematical model of disease-specific deaths in screened and unscreened men given information on overdiagnosis, disease-specific survival in the absence of screening, screening efficacy, and other-cause mortality. A simulation framework is used to incorporate competing causes of death.

Results—Assuming overdiagnosis and screening efficacy consistent with ERSPC results, we project that, after 25 years, 262 men need to be screened and 9 additional men need to be screen detected to prevent one prostate cancer death. Corresponding estimates of the NNS and the additional NNT under a range of overdiagnosis rates that are consistent with US incidence are 186–220 and 2–5.

Conclusions—Long-term estimates of the NNS and the additional NNT are an order of magnitude lower than the shorter-term estimates published with the results of the ERSPC trial and may be consistent with cost-effective PSA screening in the general US population.

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Keywords

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Introduction

The diagnosis and treatment of prostate cancer that would never have been detected without screening—known as overdiagnosis and overtreatment—have long been recognized as the primary harms associated with PSA screening. In contrast with many other cancers, overdiagnosis of prostate cancer is relatively widespread because of the high prevalence of pathological prostate cancer relative to its clinical incidence. The lifetime probability of histological prostate cancer has been estimated to be about 36% [1] while the lifetime probability of being diagnosed just prior to the advent of PSA screening was 9% [2]. These numbers suggest that as many as three out of four prostate cancers would never become clinically apparent.

Estimates of overdiagnosis [3–7] have varied widely, due primarily to different populations and methods used for estimation. Populations may differ in their disease incidence before the introduction of screening, the screening protocol, and the intensity of diagnostic follow-up after a test. Estimating the overdiagnosis frequency is challenging because it requires making inferences about a counterfactual quantity, namely the time at which a screen-detected tumor would have been diagnosed had screening not occurred. Analytic methods typically model this latent event but there is a range of models and, correspondingly, a range of results.

In a collaboration involving three independently developed statistical models [4, 5, 8], Draisma et al. [7] estimated the overdiagnosis frequency among US men aged 50–84 in 1985–2000 under common assumptions about PSA utilization [9]. The models projected that 23–42% of screen-detected cases were overdiagnosed. However, since not all prostate cancer cases receive curative treatment, the estimates do not directly correspond to overtreatment. In fact, data from the Surveillance, Epidemiology, and End Results (SEER) registry suggest that, if overdiagnosed cases elect primary treatment with frequencies similar to those in the general case population, then around 70% of overdiagnosed cases are overtreated, representing 16–29% of screen detections in the US.

Estimates of overdiagnosis in the European setting are considerably higher than those in the US. For example, Draisma et al. [4] modeled disease incidence data from the Rotterdam section of the European Randomized study of Screening for Prostate Cancer (ERSPC) and estimated that 66% of screen-detected tumors were overdiagnosed. This result is consistent with a lower baseline incidence of prostate cancer in Europe, a lower PSA cutoff for biopsy referral (3 ng/mL in several ERSPC centers versus 4 ng/mL in the US), and a much higher frequency of compliance with biopsy referral in the ERSPC [10] versus the US [11].

While knowing the frequency of overdiagnosis is important, policy decisions about PSA screening will ultimately depend on the tradeoff between harms and benefits. In 2009, estimates of screening benefit became available with the publication of interim results from the ERSPC and the US-based Prostate, Lung, Colorectal, and Ovarian (PLCO) cancer screening trial [10, 12]. After a median of 9 years of follow-up, the ERPSC showed a relative risk reduction of 20% in the frequency of prostate cancer death due to screening, and an absolute risk reduction of 0.71 deaths per 1,000 men screened or, reciprocally, a number needed to screen (NNS) of 1,410 to prevent one prostate cancer death. In an

extended study of the Göteborg center of the ERSPC [13], PSA screening was associated with a 44% reduction in the risk of prostate cancer death after 14 years and an NNS of 293. In contrast, after 7 years of follow-up, the PLCO trial showed no difference in the prostate cancer death rates in the screened and control arms. However, the PLCO trial was conducted in a population already undergoing screening, and thus "usual care" in the control arm included screening for a substantial fraction of participants [14].

The publication that reported the ERSPC results [10] also reported a new measure of the harm-benefit tradeoff associated with PSA screening, namely the "additional number needed to treat to prevent one prostate cancer death." By comparing the excess prostate cancer incidence in the screened arm with the excess prostate cancer mortality in the control arm, they concluded that 48 additional cases of prostate cancer would potentially need to be treated to prevent one death. A similar calculation by investigators reporting the results of the Göteborg trial [13] yielded a far lower estimate of 12. Although this measure has been termed the additional number needed to treat (NNT) to prevent one death, it should, strictly speaking, be interpreted as the additional number needed to diagnose (NND) to prevent one death, because presumably not all detected cases in these trials received curative treatment.

The variability in published estimates of the NNS and the additional NNT makes it difficult to determine what the harm-benefit tradeoff of PSA screening is likely to be for a given setting. A major problem with these measures is their dependence on the duration of follow-up [15]. The denominator of both of these measures – the absolute mortality reduction – is inevitably understated under limited follow-up. The numerator is the empirically observed excess incidence in the screened group relative to the control group, a proxy for the incidence of overdiagnosis. But this is also inaccurate under limited follow-up because excess incidence consists of a combination of overdiagnoses and early detections—cases who would have surfaced clinically in the absence of screening given sufficient follow-up time. Excess incidence over a limited time interval underestimates early detections and overestimates overdiagnosis.

In this article we estimate the long-term NNS and additional NNT based on the mortality reduction observed in the ERSPC trial. We examine the impact of varying estimates of screening efficacy by considering efficacy results from both the ERSPC and the Göteborg trials. We use the European efficacy results because contamination in the PLCO trial prevents direct comparison of screening versus no screening in the US setting [14]. However, in an effort to examine how the NNS and additional NNT might differ in the US setting, we also estimate these measures given the screening efficacy reported in the ERSPC trial but under overdiagnosis frequencies consistent with US data.

Methods

Projecting long-term NNS and additional NNT

Long-term measures of the NNS and the additional NNT are based on long-term disease-specific mortality in the presence and absence of screening. To project disease-specific mortality in the absence of screening we estimate the incidence of non-overdiagnosed cancers in the trial population and model their survival based on the long-term disease-specific survival distribution without screening. To project disease-specific mortality in the presence of screening we then inflate the disease-specific survival distribution by a hazard ratio that reflects screening efficacy. The difference between the long-term, cumulative incidence of disease-specific mortality in the absence and presence of screening provides the denominator of the NNS and the additional NNT.

For the numerators, we do not yet have an estimate of overdiagnosis from the ERSPC as a whole. Draisma et al. [4] estimated the frequency of overdiagnosis in the Rotterdam section of the trial and found it to be as high as 66% of screen-detected cases. However, the excess incidence in the trial as a whole (34 per 1,000 men screened) amounted to only 58% of the screen-detected cases. This is almost certainly greater than the frequency of overdiagnosis in the trial after a median of 9 years of follow-up. In our calculations we use this value in the absence of a more accurate estimate of overdiagnosis in the European trial. In the US setting we use 23% and 42%, which correspond to the lower and higher endpoints of a recently published range of overdiagnosis frequencies among screen-detected cases in the US [7].

Long-term NNS and additional NNT in the absence of other-cause mortality

We estimate the incidence of non-overdiagnosed cancer in the screened arm as:

$$N=S(1-O)+C,$$

where O denotes the overdiagnosis frequency among screen detections and S and C denote the fraction of men screen- and non-screen-detected. For example, setting $O = 58\%$ based on Draisma et al. [4] and using $S = 4{,}235/72{,}890 \approx 5.8\%$ and $C = 1{,}755/72{,}890 \approx 2.4\%$ based on Table 1 in Schröder et al. [10], we obtain $N \approx 4.8\%$.

Because only non-overdiagnosed cases are at risk of disease-specific death, we project the long-term disease-specific survival for men on the screened arm, had they not been screened, as:

$$P=NR+(1-N),$$

where R is the long-term disease-specific survival in the absence of screening. We use $R = 41.5\%$, the 25-year net (i.e., in the absence of other-cause death) relative survival observed for cases diagnosed in SEER in 1981–1985, just prior to the dissemination of PSA screening in the US [16]. A comparison of 5-year relative survival rates between SEER 1981–1985 diagnoses and 1995–1999 diagnoses in Europe [17] yields fairly comparable results (75.8% in SEER versus 76% in Europe). In the ERSPC example, the resulting 25-year survival in the absence of screening is $P = 0.972$.

Next we apply the observed rate ratio H for prostate cancer death in the screened versus the control arm to obtain long-term survival in the presence of screening. The observed mortality rate ratio is based on the entire trial population (cases and non-cases) and approximates the hazard ratio of disease-specific death associated with screening [18]. The difference in long-term survival in the presence and absence of screening becomes the denominator in the NNS and the additional NNT. Using $H = 0.80$ from the ERSPC, we estimate that the 25-year survival in the presence of screening would be $P^H = 0.972^{0.80} \approx 0.977$ (actual calculations performed using 10 significant digits), implying that the NNS is $1/(P^H - P) = 1/(0.977 - 0.972) \approx 178$ and the additional NNT is $O \times S/(P^H - P) = 0.58 \times 0.058/(0.977 - 0.972) \approx 6$. These estimates are an order of magnitude smaller than the measures reported based on interim ERSPC results. The discrepancy is partly because we are projecting survival far beyond the limited follow-up of the trial and partly because this approximation is in the absence of other-cause deaths.

Long-term NNS and additional NNT in the presence of other-cause mortality

We use a simulation-based competing risks model to incorporate other-cause deaths.

First, we simulate disease-specific and other-cause deaths without screening for a cohort representing the screened arm of the ERPSC. To do this, we (i) generate records for this cohort and randomly designate a fraction N to be non-overdiagnosed cases, (ii) generate a time from diagnosis to disease-specific death for each non-overdiagnosed case using relative survival curves from SEER for cases diagnosed in 1981–1985, and (iii) use life tables for men aged 55–69 at diagnosis to generate a time from diagnosis to other-cause death for each member of the cohort. Men for whom disease-specific death precedes other-cause death are assigned prostate cancer as their cause of death. We use life tables for men aged 55–69 at diagnosis to match the cohort used to derive the mortality reduction reported in Schröder et al [10].

We use the simulated dates of disease-specific and other-cause death to construct the corresponding cumulative incidence curve for the event of prostate cancer death. Cumulative incidence of prostate cancer death stabilizes after approximately 25 years and we use this value as our estimate of cumulative disease-specific mortality in the absence of screening.

We repeat this exercise, raising the disease-specific survival curve for the cohort to the power H to generate times to disease-specific death given screening, and we generate the corresponding cumulative incidence curve for prostate cancer death in the presence of other-cause mortality. The NNS is the reciprocal of the difference in the cumulative incidence of prostate cancer death at 25 years in the presence of screening and in the absence of screening. The additional NNT is the overdiagnosis rate times the NNS.

Sensitivity analysis

One simplification in the proposed model is that we start the survival clock for non-overdiagnosed cases at the time of entry to the trial. Ideally, we would add an offset for these cases, representing the interval from entry to their date of clinical diagnosis. Because this interval varies from case to case, and we do not have data on its distribution, we do not incorporate this offset in the model. Consequently, the age distribution for other-cause deaths likely reflects a younger age distribution (age at the start of the trial) than the one that is the case in practice (age at diagnosis). To assess the impact of this simplification, we redid our calculations assuming an age distribution for competing mortality that was 5 years older than the one used in the baseline model.

A related simplification is that we do not explicitly account for lead time among screen detections. To assess the impact of this simplification, we generated results under 20-year net relative survival for screen detections and 25-year net relative survival for non-screen-detected cases. By doing this, we effectively assigned screen-detected cases the long-term survival that would correspond to a 5-year lead time.

Results

The upper panel of Table 1 presents calculations of NNS and additional NNT under in the absence of competing causes of death. We observe that a higher overdiagnosis rate implies better long-term, disease-specific survival (P) in the absence of screening. Given a specified mortality hazard ratio (H), this produces a lower number of deaths prevented, a higher NNS and a higher additional NNT. These relationships also hold in the presence of other-cause death as shown in the lower panel, which has fewer prostate cancer deaths in both the absence and presence of screening and correspondingly fewer prostate cancer deaths prevented [19, 20].

Assuming screening efficacy as reported in the ERSPC, we find that, after 25 years, the NNS is 262 and the additional NNT is 9 in the European setting. Even though this is based on our highest setting for the overdiagnosis frequency the additional NNT is much lower than the estimate of 48 presented in Schröder et al [10]. Corresponding estimates in the US setting are 186–220 and 2–5. Accounting for a 5-year delay between entering the trial and diagnosis increases the additional NNT by about 12%, and accounting for a 5-year lead time increases the additional NNT by 12–15%.

Discussion

The NNS and the additional NNT have the potential to be highly informative about the harm-benefit tradeoff of PSA screening. However, the additional NNT of 48 [10] has been repeatedly misinterpreted, often justifying an overly negative impression of the tradeoff [21–23]. The additional NNT is not a measure of overdiagnosis and should not be misinterpreted as such; it is the ratio of a specific harm of screening (overdiagnosis) to a specific benefit (deaths prevented). Moreover, its value depends not only on the efficacy of screening but also the extent of overdiagnosis and the time horizon.

By estimating the long-term NNS and additional NNT, we have eliminated variability due to the follow-up duration, allowing us to study their dependence on overdiagnosis and screening efficacy. We find that even using the excess incidence observed in the ERSPC as an upper bound for overdiagnosis, the resulting NNS and additional NNT remain an order of magnitude lower than the published estimates from the ERSPC as a whole. Using a different method, a recent study found that these measures decreased substantially even after only 12 years of follow-up [15].

Our additional NNT estimates are also lower than those of Welch and Albertsen [24], who obtained estimates of 23 for the additional NND and 18 for the additional NNT in the US. We note, however, that even though these results are predicated on an apparently optimistic assumption, namely that the entire disease-specific mortality decline is attributable to screening, the corresponding deaths prevented are computed over an interval that ends in 2005, and that certainly excludes many deaths prevented by screening that took place towards the end of this interval. Thus, this estimate of deaths prevented may ultimately underestimate the true mortality benefit due to screening over the calendar interval considered. In addition, Welch and Albertsen use excess incidence (relative to the incidence just before the advent of PSA screening) rather than true overdiagnosis as their additional NND and additional NNT numerators. We conclude that their results, although lower than 48, likely also overestimate the true additional NND and additional NNT in the US.

The NNT is established as a measure of harm-benefit tradeoff in the setting of treatment trials, where it serves as an interpretable summary of crude (in the presence of other-cause death) survival differences among the groups being compared. Its analog in the screening trial setting is the NNS, which similarly depends only on survival in the screening and control groups. It is important to recognize that the NNS depends not only on the screening protocol implemented in practice, but also on the frequency of treatment in the trial population [25], and therefore it may not be transferable to populations that differ from the trial population in these respects. While the NNS and the additional NNT are closely related, they are different measures and both should be considered when evaluating the harms and benefits of a screening policy.

We conclude by noting that a recent cost-effectiveness study suggests that the projected long-term additional NNT estimated in this paper for PSA screening is sufficiently low for PSA screening to satisfy conventional thresholds for cost-effectiveness in the US population

[26]. It is likely that even greater cost-effectiveness will be achieved under screening strategies targeted at younger and healthier men [27]. However, additional modeling will be necessary to go beyond empirical observation of screening trials with specified protocols and to determine optimal screening strategies in the general population.

What is new?

Early estimates of the harm-benefit tradeoff from prostate cancer screening trials are based on short-term follow-up

These estimates, presented in terms of the number needed to screen and the additional number needed to treat to prevent one prostate cancer death, understate benefits and overstate harms.

We use modeling to project long-term estimates of the harm-benefit tradeoff from screening trial results.

The long-term harm-benefit tradeoffs of prostate cancer screening are substantially more favorable than early trial results suggested.

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Table 1

Projected 25-year NNS and additional NNT under different assumptions about the overdiagnosis rate among screen detections (O) and hazard ratios (H) for disease-specific mortality associated with screening.

Projections in the absence of competing cause of death

Overdiagnosis rate among screen detections	**Hazard ratio for disease-specific mortality associated with screening**	**Incidence rate of non-overdiagnosed cancers in the screened arm**	**Disease-specific survival in screened arm in the absence of screening**	**Disease-specific survival in screened arm in the presence of screening**	**Recipricol of deaths prevented by screening (NNS)**	**Overdiagnoses relative to deaths prevented by screening (additional NNT)**
O	H	$N = S(1-O) + C$	$P = NR + (1 - N)$	P^H	$1/(P^H - P)$	$O \times S/(P^H - P)$
0.58	0.80	0.048	0.972	0.977	178.35	6.01
0.58	0.56	0.048	0.972	0.984	80.79	2.72
0.42	0.80	0.054	0.968	0.975	159.48	4.45
0.42	0.56	0.054	0.968	0.982	72.21	2.01
0.23	0.80	0.069	0.960	0.968	126.27	1.69
0.23	0.56	0.069	0.960	0.977	57.11	0.76

Projections in the presence of competing causes of death

Overdiagnosis rate among screen detections	Hazard ratio for disease-specific mortality associated with screening	Cumulative incidence of disease-specific mortality in screened arm	Cumulative incidence of disease-specific mortality in control arm	Recipricol of deaths prevented by screening (NNS)	Overdiagnoses relative to deaths prevented by screening (additional NNT)
O	H	M_s	M_c	$1/(M_c - M_s)$	$O \times S/(M_c - M_s)$
0.58	0.80	0.015	0.019	262.49	8.85
0.58	0.56	0.011	0.019	118.76	4.00
0.42	0.80	0.018	0.023	219.83	5.36
0.42	0.56	0.013	0.023	99.96	2.44
0.23	0.80	0.022	0.027	185.95	2.48
0.23	0.56	0.015	0.027	83.92	1.12