



Malnutrition Coding and Reimbursement: How Documentation Practice Shapes Reported Prevalence

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Abstract

Reported hospital malnutrition prevalence is produced by documentation practice as much as by patients. Coded diagnoses appeared in 8.9 per cent of United States non-maternal, non-neonatal discharges in 2018, while screening studies in acute settings report 12 to 53 per cent. A single-centre audit identified 1,817 patients as malnourished over two years and found 64.4 per cent of them carried no malnutrition code. The gap is well described and the usual conclusion is that coding should improve. The complication lies in what happens when it does. Between 2016 and 2018 codes for any malnutrition rose from 6.6 to 8.6 per cent and for severe malnutrition from 3.3 to 4.7 per cent, while the adjusted death rate among those coded with severe malnutrition fell by 0.54 per cent per year, a decline absent in those without a code. Falling mortality among the coded is what a lowered coding threshold looks like. Reported prevalence therefore cannot serve simultaneously as an epidemiological estimate and as a billing artefact, and the literature measuring coding improvement reports revenue far more often than it reports patient outcomes.

Keywords: Malnutrition; Clinical coding; Reimbursement; Administrative data; Prevalence estimation

Introduction

Hospital malnutrition prevalence is quoted from two incompatible sources. Screening and assessment studies measure patients; administrative datasets count diagnostic codes [1,2,3]. The two produce numbers that differ several-fold, and both circulate as prevalence [2,4].

Coding is not a neutral record of clinical findings. Under case-mix payment systems a malnutrition code functions as a comorbidity that alters the payment weight and the expected length of stay for an admission, so there is a financial reason to record it and an administrative cost to doing so [5,6,7].

This review examines what determines the coded figure, what happens to it when coding practice changes, and what that implies for anyone using administrative data to describe nutritional status in populations.

Objective

This review sets out to establish four things from the published literature. First, the size of the gap between coded and screened prevalence. Second, what the payment system attaches to a malnutrition code. Third, what happened to national coded prevalence and to mortality among the coded when coding intensity rose. And fourth, what the coding-improvement literature actually measures.

Literature Review

The size of the gap: Analysis of United States hospital discharge data found that 8.9 per cent of non-maternal, non-neonatal discharges in 2018 carried a coded malnutrition diagnosis, and that those coded were older, stayed longer, cost more, and had higher readmission and inpatient mortality than those not coded [8]. Screening studies in Australian acute settings report prevalence between 12 and 53 per cent, and a point prevalence study of 189 adult inpatients using a subjective global assessment tool classified 53 per cent as malnourished [2,9].

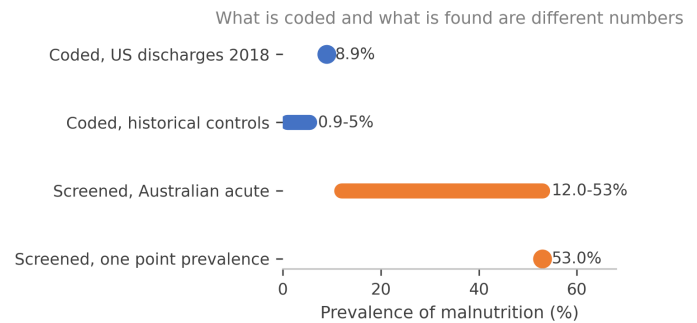


Figure 1: Coded and screened prevalence from the same era [2,8,9]

The same hospital, measured twice: In that point prevalence study, 98 per cent of patients identified as malnourished were coded as such in the medical record during the study period [9]. Audit of historical control groups in the same institution showed between 0.9 and 5.4 per cent coded as malnourished [9]. The difference is attributed principally to identification rather than to coding of identified cases, and the unclaimed annual reimbursement was estimated at AU\$8,536,200 [9].

Where the gap sits in the pathway: A single-centre audit spanning March 2015 to June 2017 found dietitians had identified 1,817 adult patients as malnourished, of whom 1,171, or 64.4 per cent, carried no malnutrition code [6]. Of those uncoded, 475, or 40.6 per cent, would have had a different diagnosis-related group, relative weight and expected length of stay had the code been applied [6]. Here the patients had been identified; the loss occurred between identification and the coded record [6,10].

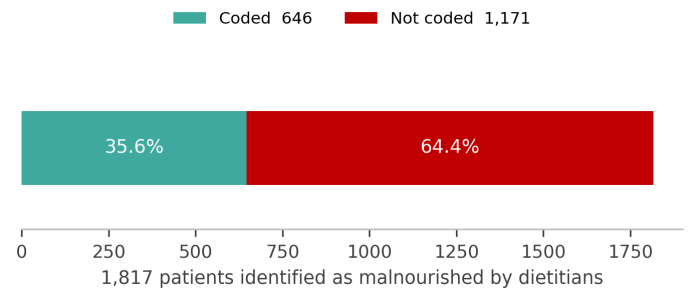


Figure 3: Coding of patients already identified as malnourished [6]

What the code changes: Under a severity-adjusted payment system a malnutrition diagnosis functions as a complication or comorbidity and alters the relative weight attached to the admission [5,6,11]. In the audited hospital the expected geometric mean length of stay implied by the assigned groups

was 3.5 days against an actual mean of 5.3 days, and correct coding would have produced an expected 5.2 days [6]. Simulations under the German system estimated additional reimbursement of EUR 263.96 per at-risk patient among 1,244 elective surgical admissions and EUR 360 per malnourished patient among 541 gastroenterology admissions [12,13].

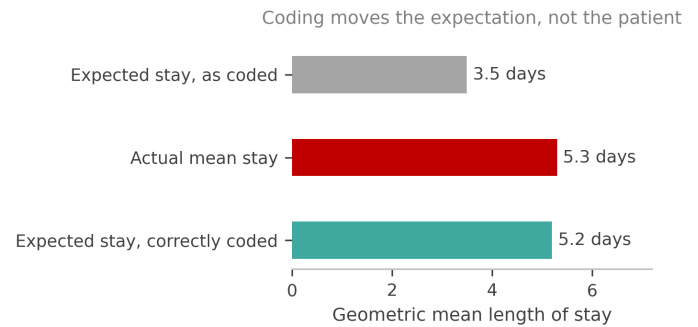


Figure 4: Expected against actual length of stay in the audited cohort [6]

Setting	Estimated financial effect
Australian teaching hospital	AU\$8,536,200 unclaimed annually [9]
German elective surgery, n=1,244	EUR 263.96 per at-risk patient [12]
German gastroenterology, n=541	EUR 360 per malnourished patient [13]
US community hospital audit	40.6% of uncoded would change DRG [6]

Table 1: Reimbursement effects reported in the coding literature

The honest complication: National United States data show codes for any malnutrition rising from 6.6 per cent of hospitalisations in 2016 to 8.6 per cent in 2018 ($p = .03$), and codes for severe malnutrition from 3.3 to 4.7 per cent ($p = .01$) [14]. Over the same period the adjusted rate of death among hospitalisations carrying a severe malnutrition code fell by 0.54 per cent per year ($p = .03$), a decline not seen in hospitalisations without a malnutrition code [14]. The authors note the increase may partly reflect a lower threshold for coding and assignment of the diagnosis to less ill patients [14].

Why that matters for the gap: The standard reading of the coded-screened gap is that coding undercounts a fixed underlying prevalence, and that closing the gap recovers a true figure [4,9,10]. The mortality trend is not consistent with that reading. If additional coding were capturing previously missed severe cases, mortality among the coded would be expected to hold or rise; a fall suggests the added cases are less ill (Figure 5) [14]. Coded prevalence is therefore not converging on a fixed target, and its rise cannot be read as a change in patients [14,15].

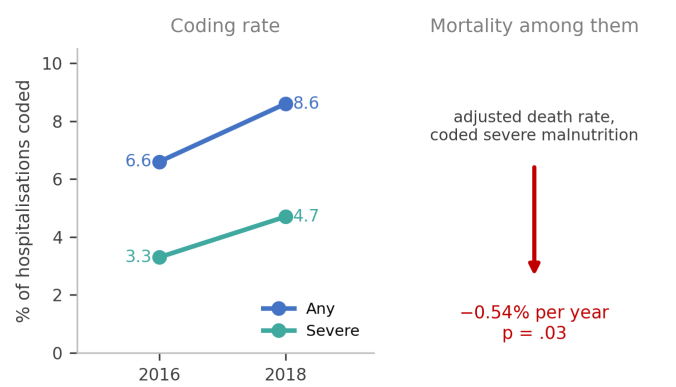


Figure 2: Coding rate and mortality among the coded, 2016 to 2018 [14]

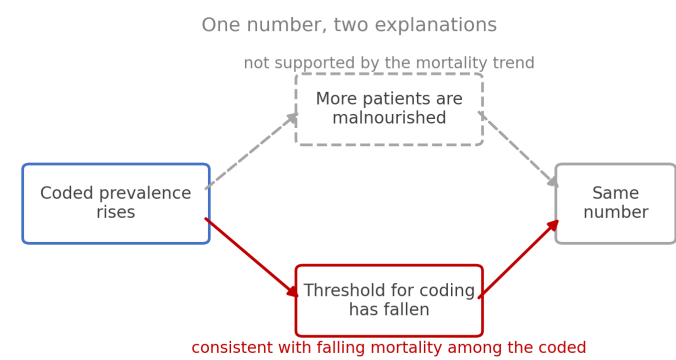


Figure 5: Two explanations for rising coded prevalence [14]

What the improvement literature measures: Studies of coding improvement report documentation completeness, group assignment and reimbursement recovered, and report nutritional or clinical outcomes for the patients concerned far less often (Figure 6) [6,9,12,13,16]. A physician documentation initiative reported a threefold increase in reimbursement from its pre-intervention baseline [16]. Embedding a screening tool in the electronic record has been reported to increase detection by 47 per cent [17]. Both are real effects on the recorded figure and neither is an effect on nutritional status [16,17,18].

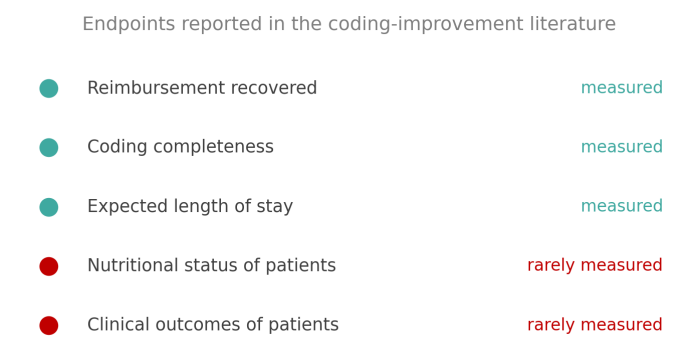


Figure 6: Endpoints reported in coding-improvement studies [6,9,16,17]

Claim	What it rests on
Coding undercounts malnutrition	Coded 8.9% against screened 12-53% [2,8]
Better coding recovers revenue	Multiple audits and simulations [6,9,12]
Better coding finds sicker patients	Not supported by mortality trend [14]
Better coding improves care	Rarely measured [16,17]

Table 2: Four claims commonly made together

The incentive is not neutral: Where a code carries payment, the intensity with which it is sought will vary with the payment system rather than with the patients, and cross-national comparison of coded prevalence therefore compares reimbursement rules as much as nutritional status [5,7,19]. Within a single system, coded prevalence will vary between institutions according to dietetic staffing, clinical documentation support and audit activity, all of which are resource variables [6,10,20]. None of this implies bad faith; it follows from what the code is for [5,19].

Definitions moved during the same period: Consensus diagnostic criteria published in 2019 changed how a malnutrition diagnosis is arrived at, and earlier characteristic-based approaches remained in parallel use [21,22]. A code applied under one framework does not denote the same patient as a code applied under another, and coded time series spanning a definitional change contain a discontinuity that is invisible in the data [21,22,23]. This compounds the threshold effect rather than substituting for it [14,21].

What administrative data can still be used for: Coded datasets remain useful for what they directly record, including which admissions carried the diagnosis, what those admissions cost, and how outcomes differed between coded and uncoded groups within the same period [8,24]. They support statements about coded populations, and the analyses that stay within that framing are sound [8,24,25]. The failure occurs when a coded proportion is presented as the prevalence of a clinical condition, which is a different claim requiring a different data source [2,15,25].

Use of coded data	Supported
Describing coded admissions	Yes [8,24]
Comparing coded and uncoded outcomes	Yes, within a period [8]
Estimating clinical prevalence	No [2,15]
Comparing years across coding change	No [14,21]
Comparing countries or systems	No [5,19]

Table 3: What coded prevalence supports and what it does not

Screening data have their own dependencies: The alternative source is not free of the same problem, since screened prevalence depends on the instrument, the threshold and the

timing of screening relative to admission [1,2,26]. The range of 12 to 53 per cent reported in acute settings reflects that variation as well as genuine differences between populations [2,26]. The argument here is not that screening figures are correct and coded figures wrong, but that neither is a property of patients alone [2,15,26].

Consequences for research using these data: Studies that use coded malnutrition as an exposure or covariate inherit the threshold effect, and where coding intensity correlates with institutional resources it may also correlate with outcomes for unrelated reasons [8,14,20]. Adjustment cannot remove a bias in the definition of the variable itself [14,20,25]. Reporting the coding rate of the contributing institutions alongside the results would let readers judge this, and it is rarely done [20,25].

The cost argument rests on the same data: Estimates of the economic burden of hospital malnutrition are widely cited in support of screening and coding programmes, and reviews of how those costs are derived find substantial methodological heterogeneity in what is attributed to malnutrition and how [27,28,33]. Where the attribution uses coded status to define the exposed group, the cost estimate inherits the coding threshold, so a programme that lowers the threshold will also alter the measured cost per case [8,14,33]. The burden literature and the coding literature are not independent lines of evidence [28,33].

What determines an institution's coding rate: Reported determinants of between-hospital variation include dietetic staffing, clinical coder capacity, and whether documentation audit is routine, none of which is a property of the patient population [20,30,31,32]. Interrupted time series of audit introduction show comorbidity capture rising after the audit begins, which is the intended effect and also a demonstration that the recorded rate is an institutional variable [31,32]. A Belgian study reached the same conclusion from the reimbursement side, reporting that a screening protocol with automated referral and consolidated discharge documentation raised recorded severity and payment [29].

Method

This is a narrative review of published literature. Sources were assembled by searching the indexed literature for analyses of coded malnutrition prevalence in administrative datasets, audits comparing screened against coded status, economic evaluations of coding under case-mix payment systems, and studies of coding-improvement interventions [6,8,14].

Figures are reproduced as their authors published them. Where a value in this review is arithmetic on published components rather than a stated result, that is noted where it appears [6].

No new patient data were collected and no search protocol was registered, so this is not a systematic review. Reimbursement figures are reported in the currency and payment

system of the original study and are not comparable across systems.

Concluding Remarks

The gap between coded and screened malnutrition prevalence is large, well documented and financially consequential. Where patients have been identified and the code is not applied, as in the audit where 64.4 per cent of identified patients went uncoded, the case for better documentation is straightforward.

What does not follow is that closing the gap recovers a true prevalence. When national coding intensity rose, mortality among those coded with severe malnutrition fell, which is what a lowered threshold looks like and is not what capturing missed severe cases would look like. The coded figure moves with documentation effort, and it moved in a direction that changed who the code describes.

The practical implication is that coded prevalence should be reported as what it is, a count of diagnoses recorded under a particular set of incentives and definitions in a particular period. It should not be used to describe trends across a coding change, compared between payment systems, or quoted as the prevalence of a clinical condition. And a coding-improvement programme should not be evaluated on revenue alone, since revenue is the outcome it is most certain to move.

Screening timing compounds the problem: Measured prevalence also depends on when screening occurs relative to admission, since nutritional status at the point of presentation differs from status after several days of acute illness and interrupted intake [26]. Studies screening within 24 hours and those screening later are pooled in some syntheses, which contributes to the reported range independently of the instrument used [2,26]. The screened figure is therefore a measurement made at a chosen moment, not a property of the admission as a whole [2,26].

Study Limitations

The evidence is dominated by single-centre audits and by national datasets from a small number of countries, principally the United States, Australia and Germany, whose payment systems differ. Economic estimates are specific to their systems and periods and do not transfer.

The inference that rising coded prevalence reflects a lowered threshold rests on an observed mortality trend and is the interpretation the original authors offered as partial rather than complete. Changes in the clinical characteristics of hospitalised patients over the same period cannot be excluded, and no study has separated the two directly.

The picture is still moving: Updated analyses of malnutrition code use in United States hospitals continue to report changes in the recorded rate, so any figure quoted from this literature carries a date as an essential qualifier rather than an incidental one

[14,34]. A prevalence quoted without its year, its coding framework and its payment context is not interpretable, and the convention of citing a single percentage as the prevalence of hospital malnutrition should be retired [8,14,34].

What would improve matters: Three things are available without new data collection. Coded analyses could report the contributing institutions' coding rates alongside their results, so readers can judge threshold effects [20,25]. Coding-improvement studies could report a nutritional or clinical outcome alongside revenue, which most currently do not [18]. And prevalence claims could name their source as coded or screened at the point of use, which costs a word and prevents the commonest error in this field [2,15].

A closing observation on incentives: Nothing in this literature suggests that clinicians or coders are recording diagnoses they do not believe [5,7,19]. The threshold effect described here operates through which borderline cases receive attention and documentation, and attention follows the incentive without anyone deciding that it should [7,14,19]. That is why the effect is visible in national trends rather than in individual records, and why it cannot be audited away at the level of the single case [14,20,31].

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Conflict of Interest

The authors declare no competing interests.

Ethics Statement

This article reviews previously published studies only. No new patient data were collected and no participants were recruited, so ethical approval was not required.

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