From end-use characterization to decentralized reuse design: Closing the data-to-system gap

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Review

From end-use characterization to decentralized reuse design: Closing the data-to-system gap

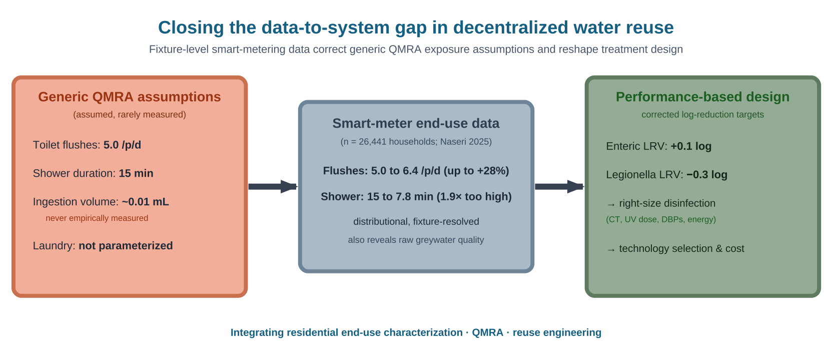
Jesper Knutsson

Water Environment Technology, Architecture and Civil Engineering, Chalmers University of Technology, Gothenburg, SE-41296, Sweden

HIGHLIGHTS

- Smart-meter data reveal QMRA exposure errors: flush up to +28 %, shower 1.9× too long.
- Accidental toilet flushing ingestion critical to QMRA but never empirically measured.
- Measured parameters shift infection risk across regulatory thresholds vs. assumptions.
- Major regulatory standards lack empirical end-use data for exposure assessment.
- Integrated framework linking smart meter data to system design and QMRA proposed.

GRAPHICAL ABSTRACT



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ABSTRACT

Decentralized water reuse systems are increasingly deployed, yet their design standards and quantitative microbial risk assessment (QMRA) frameworks rely on assumed exposure parameters rather than empirical data. Concurrently, smart metering research has generated high-resolution, fixture-level demand profiles that remain disconnected from reuse practice. This review asks whether empirical end-use data support the exposure parameters assumed in non-potable reuse risk models, bridging three siloed literatures: residential end-use characterization, decentralized reuse system design, and QMRA.

A systematic comparison of QMRA exposure assumptions against smart-meter measurements shows that the assumed toilet flush frequency (5.0/person/day) lies at the low end of smart-metering estimates (5.0–6.4/person/day), the upper value exceeding the assumption by 28 % and underestimating enteric exposure. Shower durations assumed in *Legionella* risk models (15 min) exceed the measured mean (7.8 min) by 92 % (a factor of 1.9), potentially overestimating inhalation risk. Accidental ingestion volume during toilet flushing, the parameter to which QMRA is most sensitive, has never been empirically measured, to the authors' knowledge. A sensitivity analysis shows that substituting measured for assumed values shifts modelled infection probabilities enough to cross the 10^{-4} annual infection benchmark.

Current regulatory frameworks (ISO 20426:2018, EN 16941, US EPA 2012/2025) do not require empirically grounded exposure parameters. An integrated framework is proposed, linking fixture-level data to system design and risk assessment, and priority research needs are identified, including tracer-based ingestion measurements.

E-mail address: jesper.knutsson@chalmers.se.

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The analysis is grounded in North American and European smart-meter data; applicability to regions with dual-flush toilets or absent metering infrastructure requires context-specific validation.

1. Introduction

Urban water systems worldwide face compounding pressures from population growth, climate variability, and ageing infrastructure (Al-Jayyousi, 2003; Li et al., 2009). In response, decentralized water reuse, encompassing greywater recycling, rainwater harvesting, and onsite non-potable treatment, has emerged as a complementary strategy to centralized supply, offering reduced freshwater abstraction, lower wastewater discharge, and improved local resilience (Gross et al., 2015; Nolde, 1999). Greywater from showers, handbasins, and laundry can be treated and reused for toilet flushing, irrigation, and laundry, displacing potable demand by 30–50 % in residential buildings (Friedler, 2004; Ghaitidak and Yadav, 2013).

Despite growing deployment, a fundamental disconnect persists between two bodies of knowledge that should inform decentralized reuse design. On one side, the residential water demand characterization literature has moved from aggregate metered consumption to high-resolution, fixture-level end-use profiles enabled by smart metering, flow-trace analysis, and machine learning disaggregation (Cominola et al., 2015; DeOreo et al., 2016; Mayer et al., 1999; Nguyen et al., 2013). On the other, design standards and risk assessment frameworks for decentralized reuse systems continue to rely on population-level averages and generic exposure parameters that do not reflect the stochastic, household-specific realities of water use (Boyjoo et al., 2013; Shaikh and Ahammed, 2020).

This disconnect has consequences for both system sizing and health risk estimation. System sizing based on mean daily demands rather than temporal profiles (diurnal, weekly, seasonal) can undersize storage and lead to more frequent supply shortfalls and tank overflow events, that is, periods when inflow exceeds the available storage capacity (Ward et al., 2012). But the sizing problem is secondary. More critically, quantitative microbial risk assessment (QMRA) for non-potable reuse endpoints employs exposure parameters (ingestion volumes, contact frequencies, and aerosol inhalation rates) that are largely assumed rather than measured, creating uncertainty that propagates into health risk estimates and derived log-reduction targets (Hamilton et al., 2019; Jahne et al., 2023; Schoen et al., 2017; Schoen et al., 2020).

This review focuses on non-potable reuse endpoints where treated greywater or rainwater replaces potable supply. The quantitative exposure analysis centres on toilet flushing and showering (the two scenarios for which QMRA and empirical end-use data can be directly compared), while garden irrigation and laundry are discussed more briefly. Showering is included in the exposure analysis for two reasons: first, *Legionella* inhalation during showering with reclaimed water is a standard QMRA scenario in the literature (Hamilton et al., 2019; Schoen and Ashbolt, 2011), even though showering with treated greywater is not a common reuse application; and second, shower duration data from smart metering illustrate the magnitude of assumption error that pervades the QMRA exposure parameter space. Toilet flushing also generates aerosols that represent a plausible inhalation route for *Legionella* and other respiratory pathogens in non-potable reuse systems, an exposure pathway that has received less attention than showering but is directly relevant to the most common reuse application (Barker and Jones, 2005; Johnson et al., 2013).

This review addresses this by: (1) characterizing the state of knowledge on greywater generation volumes (per-capita generation rates), quality, and temporal variability; (2) reviewing design standards, including recent international standards (ISO 20426:2018; European Committee for Standardization (CEN), 2024; European Committee for Standardization (CEN), 2021) and updated US EPA guidance (2012, 2025), and their reliance on assumed inputs; (3) inventorying QMRA exposure assumptions and comparing them systematically to empirical end-use data; (4) connecting the identified mismatches to derived log-reduction targets (Jahne et al., 2023); (5) assessing the smart metering and monitoring infrastructure available to close the gap; and (6) demonstrating through sensitivity analysis that the observed mismatches materially affect both risk estimates and the treatment-design targets derived from them. The review concludes by proposing an integrated framework and identifying research priorities.

2. Review methodology

This review follows the PRISMA 2020 guidelines for systematic reviews (Page et al., 2021) adapted for a critical review format, combining systematic database searching with targeted citation tracking to map the intersection of three distinct literatures: residential water end-use characterization, decentralized water reuse system design, and quantitative microbial risk assessment for non-potable reuse.

2.1. Search strategy

Systematic searches were conducted between 15 January and 28 February 2026 across Scopus, Web of Science, and PubMed, complemented by targeted screening of Google Scholar (first 200 results per query, sorted by relevance). Searches were performed in title, abstract, and keyword fields where supported by the database interface.

Three complementary search strings were used: (i) for greywater characterization and reuse: (greywater OR “grey water” OR graywater) AND (reuse OR recycling OR treatment OR characterization); (ii) for QMRA and non-potable reuse: (QMRA OR “quantitative microbial risk”) AND (“non-potable” OR “toilet flush*” OR “greywater reuse” OR “rainwater”); and (iii) for smart metering and end-use characterization: (“smart meter” OR “end-use” OR “fixture-level”) AND (“water demand” OR “residential water” OR “disaggregation”).

Searches covered the period 1996–2025 and were restricted to English-language publications and peer-reviewed journal articles. A total of 512 records were retrieved (Scopus: 185; Web of Science: 148; PubMed: 97; Google Scholar: 82), yielding 312 unique records after duplicate removal using reference management software (EndNote).

Given differences in indexing, search algorithms, and database coverage, exact reproducibility of total record counts across platforms is not expected; however, search strings, inclusion criteria, and screening procedures are reported transparently to ensure methodological consistency.

2.2. Inclusion and exclusion criteria

Studies were included if they: (a) reported empirical or modelled data on greywater generation volumes, quality, or temporal patterns at household or building scale; (b) applied QMRA to non-potable reuse endpoints and reported exposure parameter values; (c) reported fixture-level water demand data from smart metering, flow-trace analysis, or automated disaggregation; or (d) described design standards, sizing methods, or regulatory frameworks for decentralized water reuse. Studies were excluded if they focused exclusively on centralized wastewater reuse, industrial water recycling, or agricultural irrigation without reference to building-scale systems. Conference abstracts without full data reporting were excluded. After title/abstract screening ($n = 312$), 87 full-text articles were assessed, and 45 publications were retained for detailed analysis and synthesis, supplemented by key regulatory documents, international standards, and technical reports identified through citation tracking (see Fig. 1).

2.3. Data extraction and quality assessment

From each retained study, the following were extracted: study design, geographic setting, sample size, monitoring duration, reported parameter values (with uncertainty where available), and the stated basis for exposure assumptions. For smart metering studies reporting fixture-level data, the disaggregation method, temporal resolution, and validation approach were additionally recorded. For QMRA studies, all exposure parameters (ingestion volumes, contact frequencies, durations) and their cited sources were extracted. Quality assessment focused on three dimensions: (i) sample representativeness (sample size, geographic scope, recruitment method); (ii) measurement validity (metering resolution, disaggregation accuracy, monitoring duration); and (iii) transparency of assumptions (whether exposure parameters were justified with empirical references or adopted from prior QMRA studies without independent verification).

2.4. Analytical approach

To quantify the disconnect between assumed and observed parameters, a comparison matrix was compiled, pairing QMRA exposure assumptions (extracted from published risk assessments) with empirical end-use values (extracted from smart metering and residential end-use studies). Where multiple studies reported the same parameter, ranges and distributional information were recorded. The identified mismatches were connected to log-reduction targets (LRTs) using the framework of Jahne et al. (2023), who demonstrated that LRTs for non-potable reuse are directly sensitive to assumed exposure frequencies and volumes. A simplified sensitivity analysis was conducted to estimate the effect of parameter substitution on annual infection probability for two illustrative scenarios: *Legionella* inhalation during showering and norovirus ingestion from toilet flushing. The full equations, parameter values, and the analysis script (including the derivation of every mismatch magnitude reported in Table 2) are provided in Supplementary Information S1.

3. Source water characterization: greywater volumes and quality

3.1. Greywater generation rates and composition

Greywater, wastewater from showers, baths, handbasins, laundry, and sometimes kitchen sinks, typically constitutes 50–80 % of total household wastewater by volume (Eriksson et al., 2002; Friedler, 2004). Christova-Boal et al. (1996) documented greywater quality from bathrooms, laundry, and kitchens in Melbourne, establishing that bathroom sources produce the least heavily polluted fraction in terms of organic and particulate content, while kitchen greywater carries the highest organic and particulate loads. This distinction between 'light' greywater (bathroom sources) and 'dark' greywater (kitchen, sometimes laundry) has become foundational in the literature (Boyjoo et al., 2013; Li et al., 2009).

Reported generation rates vary widely with geography, household size, and water use culture. In Europe, per capita greywater production ranges from approximately 60–120 L/person/day (Eriksson et al., 2002; Nolde, 1999). In water-scarce regions, lower overall consumption yields proportionally less greywater, though the percentage of total wastewater remains similar (Al-Jayyousi, 2003; Oteng-Peprah et al., 2018). Shaikh and Ahammed (2020) conducted a comprehensive review confirming substantial inter-household and inter-regional variability in generation rates. Table 1 summarizes reported greywater quality by source stream.

Xenobiotic organic compounds, including fragrances, preservatives, and UV filters, are increasingly documented as emerging contaminants of concern in greywater reuse contexts (Donner et al., 2010; Palmquist and Hanaeus, 2005). The quality of greywater is not static: it changes during storage due to microbial growth, with BOD and pathogen indicators increasing significantly within 24–48 hours if untreated (Gross et al., 2015).

3.2. Microbial quality and pathogen occurrence

Early assumptions that greywater would be largely pathogen-free have been consistently refuted. Ottoson and Stenstrom (2003) investigated faecal contamination of greywater and found that while faecal

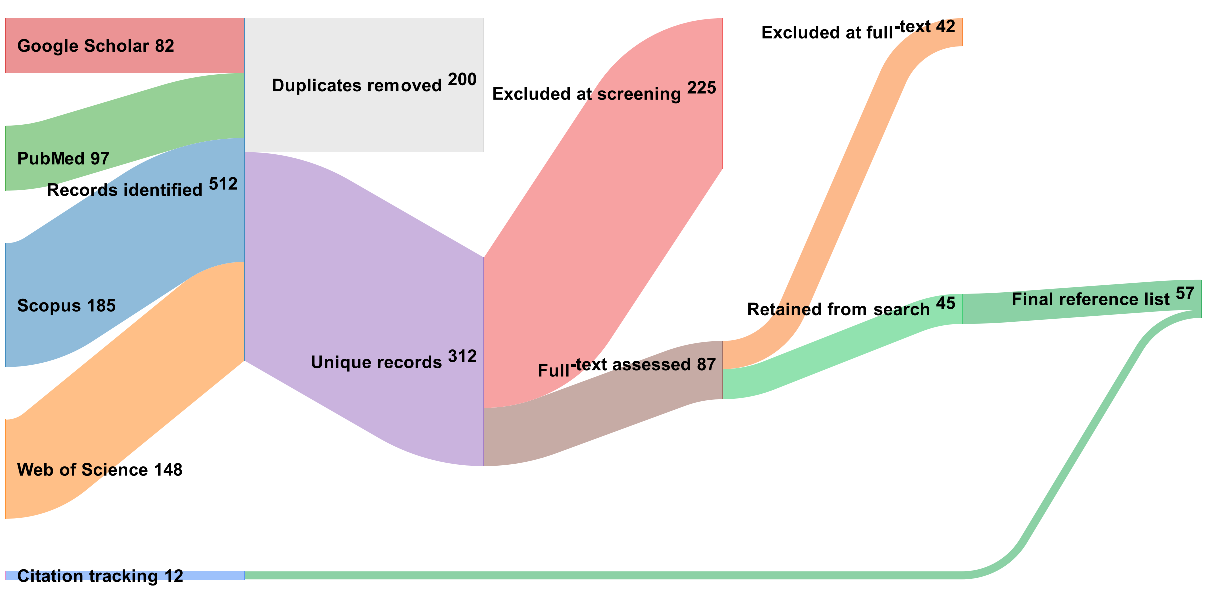

Fig. 1. PRISMA 2020 flow diagram illustrating the literature identification and screening process. A total of 512 records were retrieved from four databases, yielding 312 unique records after de-duplication. Following title/abstract screening and full-text assessment, 45 studies were retained from the systematic search. An additional 15 records, comprising international standards (ISO 20426, EN 16941-1/-2), regulatory guidance documents (US EPA, 2012, 2025), and key methodological references, were identified through citation tracking, resulting in a final corpus of 60 references.

Table 1
Typical greywater quality ranges by source stream (compiled from Eriksson et al., 2002; Friedler, 2004; Ghaitidak and Yadav, 2013; Li et al., 2009). BOD, biochemical oxygen demand; TSS, total suspended solids; NTU, nephelometric turbidity units; CFU, colony-forming units.

Source stream	BOD (mg/L)	TSS (mg/L)	Turbidity (NTU)	Total coliforms (CFU/100 mL)	pH
Shower/Bath	50–200	25–120	15–100	10^2–10^5	6.4–8.1
Handbasin	50–150	20–100	10–80	10^2–10^5	6.5–7.8
Laundry	100–400	50–250	50–250	10^2–10^6	7.1–10.0
Kitchen	300–1500	100–500	100–800	10^3–10^8	5.9–7.4
Light greywater (combined bathroom)	50–300	25–200	15–150	10^2–10^6	6.4–8.1

indicator bacteria were consistently detected, chemical biomarkers suggested that CFU-based indicators overestimated the actual faecal load, complicating risk interpretation. Faecal contamination routes include hand washing after toilet use, bathing of young children, and laundering of soiled textiles. Subsequent studies have confirmed the presence of enteric pathogens including norovirus, rotavirus, *Salmonella*, and *Cryptosporidium* in untreated greywater, albeit at lower concentrations than in blackwater (Gross et al., 2015; Jahne et al., 2017).

Legionella pneumophila has emerged as a pathogen of particular concern for greywater reuse systems, given the warm, nutrient-rich conditions that favour its proliferation. *Legionella* is, in this respect, a notable exception: most enteric pathogens and faecal indicator organisms do not replicate outside a host and instead tend to decay during storage, so indicator regrowth and pathogen growth in greywater are concerns largely confined to opportunistic premise-plumbing organisms such as *Legionella*. Blanky et al. (2015) quantified *Legionella* in greywater treatment trains and found that conventional treatment processes achieved variable removal. Blanky et al. (2017) demonstrated that the health risk from *Legionella* exposure during showering with inadequately treated greywater could exceed acceptable thresholds, underscoring the need for pathogen-specific treatment targets rather than reliance on indicator organisms alone.

The variability of greywater microbial quality, both between households and over time within a single household, remains poorly quantified. Most characterization studies report grab samples or short-duration composites, which fail to capture the stochastic nature of contamination events. This temporal variability has direct implications for treatment system design and QMRA, yet it is rarely propagated into either domain.

4. Design standards and sizing models for decentralized reuse systems

4.1. Regulatory frameworks and treatment standards

Decentralized water reuse systems are governed by a patchwork of national and sub-national regulations that have evolved substantially in recent years. The Australian Guidelines for Water Recycling (NRMMC, EPHC and NHMRC, 2006) employ a 12-step risk management approach requiring health-based targets expressed as disability-adjusted life years (DALYs). NSF/ANSI 350 (NSF International, 2011) establishes performance criteria for onsite water reuse in North America. In the United Kingdom, BS 8525 covers greywater systems, while the EU has adopted minimum requirements for agricultural irrigation reuse (European Union, 2020), though building-scale non-potable reuse remains largely unregulated at EU level.

At the international level, ISO 20426:2018 (Guidelines for health risk assessment and management for non-potable water reuse) provides a harmonized QMRA-based framework for assessing health risks from non-potable reuse, including specification of reference pathogens, exposure scenarios, and acceptable risk thresholds. Notably, ISO 20426 adopts the WHO benchmark of 10^{-6} DALY/person/year but does not prescribe specific exposure parameter values, leaving practitioners to select assumptions from the literature, the very literature whose assumptions this review questions.

For system design, on-site non-potable water systems, Part 1: Systems for the use of rainwater (European Committee for Standardization (CEN), 2024) and Part 2: Systems for the use of treated greywater (European Committee for Standardization (CEN), 2021) establish European performance requirements for rainwater harvesting and greywater recycling systems, respectively. These standards specify water quality criteria for treated water, system component requirements, and maintenance schedules, but rely on simplified demand assumptions (e.g., fixed per-capita flush volumes and frequencies) that do not account for fixture-level variability.

In the United States, the EPA has progressively updated its non-potable reuse guidance. The 2012 Guidelines for Water Reuse (US EPA, 2012) provided a comprehensive framework including treatment technologies, monitoring requirements, and risk assessment approaches. More recently, the 2025 Risk-Based Framework for Developing Microbial Treatment Targets for Water Reuse (US EPA, 2025) provides a comprehensive QMRA-based approach to deriving fit-for-purpose log-reduction targets, a direction aligned with the framework proposed in this review. However, neither document requires that QMRA exposure parameters be derived from empirical end-use data rather than assumed from regulatory exposure factor handbooks. Indeed, none of the regulatory frameworks reviewed in this section, ISO 20426, EN 16941-1/-2, US EPA (2012, 2025), or the Australian Guidelines, mandate empirically grounded exposure parameters for non-potable reuse risk assessment, a gap considered structurally problematic rather than merely inconvenient.

A common feature across all of these frameworks is that design inputs, source water volumes, demand patterns, and occupancy profiles, are specified as fixed values or simple ranges derived from census-level data. Greywater generation is typically assumed at 100–150 L/person/day or as a fixed percentage of total water consumption, without reference to fixture-specific end-use profiles (Boyjoo et al., 2013). This approach may be adequate for large centralized systems where statistical averaging reduces uncertainty but becomes problematic at the building scale where small user populations amplify variability.

4.2. System sizing and supply–demand matching

The design of decentralized reuse systems requires matching a variable supply with a variable demand. Ward et al. (2012) demonstrated that standard design tools significantly underestimated overflow frequency in a large building rainwater harvesting system because they relied on average daily yields rather than event-based patterns and diurnal demand profiles.

For greywater systems, supply–demand timing mismatch is particularly acute: generation peaks in morning and evening (showers and laundry), while toilet flushing demand is more evenly distributed. Treatment technology selection, from simple filtration to membrane bioreactors and constructed wetlands, interacts with sizing through its effect on treatment rate (the hydraulic throughput, or flux, that the system must sustain), footprint, and influent variability sensitivity (Li et al., 2009; Stefanakis, 2020). Winward et al. (2008) found chlorine disinfection efficacy declined at elevated chemical oxygen demand (COD) concentrations that standard design parameters did not anticipate. Oron et al. (2014) reviewed greywater reuse standards internationally and concluded that most guidelines lack the granularity needed for site-specific optimization.

5. Quantitative microbial risk assessment for non-potable reuse

5.1. Framework and application

QMRA provides the formal structure for estimating health risks from microbial hazards in water reuse systems, following four steps: hazard identification, exposure assessment, dose–response modelling, and risk characterization (Haas et al., 2014). For non-potable reuse, QMRA has been applied to toilet flushing, garden irrigation, clothes washing, and car washing, with reference pathogens typically including norovirus, *Cryptosporidium, Campylobacter*, and *Legionella* (Hamilton et al., 2017; Schoen et al., 2017).

The WHO (2006) Guidelines established a benchmark of 10^{-6} DALY per person per year. Schoen et al. (2017) operationalized this for decentralized reuse by deriving pathogen log-reduction targets (LRTs) for greywater, roof runoff, and stormwater applied to toilet flushing and irrigation. Reynaert et al. (2024) extended this to diverse collection scales and multiple appliances, deriving pathogen-specific log-removal targets mapped to treatment train configurations. Jahne et al. (2023) provided a critical evaluation of LRT derivation from QMRA, demonstrating that target values are a direct function of source pathogen concentrations, exposure frequencies, exposure volumes, and the chosen risk benchmark, meaning that errors in any exposure parameter propagate linearly into the required treatment performance. Earlier, Jahne et al. (2020) had provided the first quantitative report of viral enteric pathogens (norovirus, adenovirus) in decentralized greywater and wastewater collections using ddPCR, establishing the empirical basis for source-water pathogen concentrations on which Jahne et al. (2023) later drew.

5.2. Exposure parameters: the assumption problem

The exposure assessment step requires specification of the ingested water volume per event (for ingestion routes); the aerosol concentration and inhalation rate combined with exposure duration (for inhalation routes); and event frequencies. For toilet flushing, the standard assumption is accidental ingestion of approximately 10^{-5} L per flush via hand-to-mouth contact or aerosol inhalation, with 5 flushes per person per day (Schoen et al., 2017). For showering, *Legionella* risk models assume aerosol inhalation over a 15-minute shower (Schoen and Ashbolt, 2011). For garden irrigation, ingestion volumes of 1–10 mL per event are assumed. For laundry, dermal contact and residual water on clothing define the exposure route (Busgang et al., 2018; Reynolds et al., 2022).

Critically, Schoen et al. (2020) explicitly acknowledged that non-potable reuse exposure data remain 'limited' and that treatment targets are highly sensitive to assumed ingestion volumes. Hamilton et al. (2019) showed that risk-based critical concentrations of *Legionella* for indoor water uses varied by more than an order of magnitude depending on assumed aerosol generation and inhalation volume. Jahne et al. (2023) further demonstrated that LRTs for non-potable reuse are sensitive to both exposure frequency and volume assumptions, with changes of the magnitude documented in the mismatch analysis (Section 6) sufficient to shift required log-reduction values by 0.1–0.5 log units, a difference that can determine treatment technology selection.

Taken together, these findings suggest that, in the absence of empirically grounded exposure data, QMRA for decentralized reuse functions less as a conservative approximation than as structured assumption-making: key parameters are neither reliably conservative nor representative of observed behaviour, yet plausible variations among them can propagate into material differences in estimated risk. This reflects inadvertent bias rather than any deliberate misrepresentation; the problem is systemic. Using weakly substantiated values has been acceptable for so long that the collection of contextually representative exposure data has become undervalued and is seldom demanded by modellers, regulators, or funding agencies. This is not to argue that QMRA without data is illegitimate; it is indispensable for scoping a problem and, above all, for identifying the data most needed. The point is that ethical practice requires modellers to state plainly when the available data are too weak to support the conclusions drawn, a judgement reached far less often than the evidence warrants.

6. The exposure parameter mismatch: QMRA assumptions versus empirical end-use data

Arguably, the central issue is not simply uncertainty in QMRA inputs, but a systematic misalignment between assumed and observed exposure conditions in everyday water use. High-resolution end-use data show that key parameters, particularly ingestion volumes and event characteristics, are not only poorly constrained, but often inconsistent with empirical behaviour. Because annual dose scales directly with the product of pathogen concentration, ingestion volume, and event frequency, and because the dose–response models in common use are approximately linear over the low-dose region relevant to treated reuse water, biases in these exposure terms propagate proportionally into estimated risk; a bias in a single parameter is carried through directly rather than cancelling, and biases in several parameters in the same direction compound, so the combined effect on estimated risk, and on the log-reduction targets derived from it, can reach an order of magnitude. The divergence thus originates in the estimation of dose, not in the form of the dose–response model. It is therefore argued that aligning QMRA inputs with observed behaviour is not a refinement, but a prerequisite for credible risk assessment in decentralized reuse systems.

To test whether the exposure assumptions embedded in QMRA for non-potable reuse are consistent with what residential end-use studies have measured, a systematic comparison was compiled across four key parameters: toilet flush frequency, shower duration and frequency, accidental ingestion volume associated with toilet flushing, and laundry frequency. Table 2 presents this comparison with distributional information where available. Table 3 extends the analysis across multiple QMRA studies to show how exposure assumptions vary within the risk assessment literature itself.

6.1. Interpretation of the mismatch

The comparison in Table 2 reveals what can be considered a structural misalignment between QMRA practice and observable household behaviour, not merely parameter uncertainty but a systematic disconnection between the models and the reality they purport to represent. Three patterns stand out. First, for toilet flush frequency, smart metering data from recent large-scale studies (Naseri et al., 2025, reporting on 26,441 US households) yield a mean of 6.4 flushes/person/day, 28 % above the 5.0 flushes/person/day assumed in QMRA, while the foundational end-use studies (DeOreo et al., 2016; Mayer et al., 1999) report 5.0; the assumption thus lies at the low end of the measured 5.0–6.4/person/day range rather than at its centre. The distribution is right-skewed, with a standard deviation of approximately 1.8 flushes/person/day, meaning that the QMRA point assumption falls below the 25th percentile of measured values in some household segments. Because enteric pathogen risk from toilet flushing scales linearly with event frequency, this underestimate translates directly into a proportional underestimate of annual infection probability. Using the LRT framework of Jahne et al. (2023), a 28 % increase in flush frequency corresponds to an increase in required log-reduction of approximately 0.1 log units, a shift that may not change treatment technology category but erodes the safety margin within a given technology tier. Put differently: the assumed flush rate is not conservative. It is optimistic.

Second, for shower duration, the divergence is more dramatic and operates in the opposite direction. The 15-minute shower assumed by Schoen and Ashbolt (2011) in their *Legionella* inhalation model exceeds the measured mean of 7.8 minutes (SD ±3.1) reported by Mayer et al. (1999) and the median of 7.1 minutes reported by DeOreo et al. (2016). The QMRA assumption thus falls above the 95th percentile of

Table 2
Exposure parameter mismatch: QMRA assumptions versus empirically measured end-use values. Mismatch magnitude is the percentage by which the QMRA assumption deviates from the measured value(s): for an assumption below the measured value, (measured − assumed)/assumed; for an assumption above it, (assumed − measured)/measured. Full derivation is given in Supplementary Information S1.

Parameter	QMRA assumption	Empirical measurement	Mismatch direction	Mismatch magnitude	Risk implication
Toilet flush frequency	5.0/p/d (Schoen et al., 2017)	5.0/p/d (DeOreo et al., 2016) to mean 6.4/p/d (SD ±1.8; Naseri et al., 2025, n = 26,441)	Assumption at/ below measured	0 to +28 %	Enteric exposure underestimated
Shower duration (min)	15 (Schoen and Ashbolt, 2011)	Mean 7.8 (SD ±3.1); median 7.1; range 5–12 (DeOreo et al., 2016; Mayer et al., 1999)	Assumption 1.9× measured	+92 % (1.9×)	*Legionella* inhalation risk overestimated
Shower frequency	1.0/p/d (assumed)	Mean 0.9–1.2/p/d (Naseri et al., 2025)	Broadly consistent	0–20 %	Minor; slight underestimate in some populations
Accidental ingestion (toilet)	~10^{-5} L/flush (Schoen et al., 2017; 2020)	No empirical measurement exists (to the authors' knowledge)	Unknown	Unknown	Most sensitive QMRA parameter; no empirical basis
Laundry frequency	Not separately parameterized in most QMRA	≈0.3 loads/person/day (~2/week) (DeOreo et al., 2016)	N/A	N/A	Exposure pathway unresolved in most QMRA models

Table 3
Exposure assumptions used across selected QMRA studies for non-potable reuse. N/R, not reported.

Study	Flush freq. (per person per day)	Shower dur. (min)	Ingestion vol. (L per event)	Basis for assumptions
Schoen et al. (2017)	5.0	N/R	1×10^{-5}	EPA Exposure Factors Handbook; expert judgement
Schoen and Ashbolt (2011)	N/R	15	Aerosol model	Assumed; no empirical reference cited
Hamilton et al. (2019)	N/R	7–15 (range)	Aerosol model	Literature range; sensitivity tested
Blanky et al. (2017)	N/R	8	Aerosol model	Based on Israeli water use data
Busgang et al. (2018)	N/R	N/R	$1–10 \times 10^{-3}$ (irrigation)	Regulatory guidance; expert elicitation
Reynaert et al. (2024)	5.0	8	Various by end use	WHO guidelines; adopted from Schoen et al.
Jahne et al. (2023)	5.0	N/R	1×10^{-5}	Adopted from Schoen et al. (2017); sensitivity demonstrated

the measured distribution. Because inhaled *Legionella* dose is roughly proportional to shower duration (via cumulative aerosol exposure), this assumption inflates the modelled risk accordingly. One caveat tempers this comparison, however: inhalation exposure to shower-generated aerosols need not cease when the water is turned off, as residual airborne droplets persist in the enclosed space, so the effective exposure time can exceed the metered flow duration. Notably, Table 3 shows that more recent QMRA studies have adopted shorter durations: Blanky et al. (2017) used 8 minutes, and Reynaert et al. (2024) also used 8 minutes, suggesting partial convergence with empirical data, though without explicit acknowledgement of the smart metering literature as the basis. The correction happened, but the reasoning was not documented. This is not a criticism of those authors: contextually representative end-use data were not easy to locate or synthesise when the earlier models were built, and the convergence toward shorter, more realistic durations is itself encouraging. The point is methodological (the basis for the change went unstated) rather than an imputation of complacency.

Third, and most consequentially, accidental ingestion volumes during toilet flushing, the parameter to which QMRA outputs are most sensitive (Schoen et al., 2020), have, to the authors' knowledge, never been empirically measured under realistic conditions. The standard value of approximately 10^{-5} L per flush is derived from regulatory exposure factor guidance, not from observational data. Arguably, this is the most consequential gap in the entire QMRA framework for non-potable reuse: the parameter to which risk estimates are most sensitive has no empirical foundation whatsoever.

6.2. Illustrative sensitivity analysis

To demonstrate the practical significance of these mismatches, simplified sensitivity calculations are presented for two exposure scenarios: (a) *Legionella pneumophila* inhalation during showering with treated greywater, and (b) norovirus ingestion from toilet flushing with treated greywater.

6.2.1. Scenario A: Legionella inhalation during showering

Following the framework of Schoen and Ashbolt (2011), the inhaled dose of *Legionella* during a shower is proportional to the product of aerosol concentration, respirable fraction, pulmonary ventilation rate, and shower duration. Using the exponential dose–response model, the single-exposure infection probability is $P_{\text{single}} = 1 - \exp(-k \times d)$, where k is the pathogen-specific infectivity constant ($k = 0.06$ for *L. pneumophila*; Haas et al., 2014) and d is the inhaled dose (CFU). Annual infection probability is then$P = 1 - (exp(-k \times \text{dose}))^n$, where n is the number of annual exposure events (e.g., 365 showers/year). Holding all other parameters constant, a reduction in assumed shower duration from 15 minutes to 7.8 minutes (the measured mean from Mayer et al., 1999) reduces the inhaled dose by a factor of 1.92. In the low-dose region where P is approximately linear with dose, annual infection probability shifts by a comparable factor. For a system operating near the 10^{-4} annual infection benchmark, this difference may determine whether the system passes or fails the health target. In terms of log-reduction targets, the reduction in assumed shower duration would lower the required LRT for *Legionella* by approximately 0.3 log units, potentially allowing a less intensive treatment technology to meet the same health benchmark. This annual formulation assumes an identical per-event dose across the n events; relaxing that assumption requires integrating the single-event probability over the per-event dose distribution, which the Monte Carlo analysis in Section 6.2 approximates.

6.2.2. Scenario B: Norovirus ingestion from toilet flushing

For enteric pathogen exposure via toilet flushing, annual dose scales with the product of pathogen concentration in treated greywater, ingestion volume per flush, and number of flushes per year. Substituting the measured flush frequency of 6.4/person/day (Naseri et al., 2025) for the assumed 5.0/person/day increases the annual number of exposure events from 1,825 to 2,336, a 28 % increase. For norovirus, which follows a hypergeometric dose–response model with a steep curve at low doses (Teunis et al., 2008), a 28 % increase in cumulative annual dose shifts annual infection probability by a comparable or smaller percentage: comparable in the low-dose, near-linear region and progressively smaller as the dose–response curve saturates; for any single-hit model it cannot exceed the proportional increase in dose. The corresponding increase in required LRT is approximately 0.1 log units.

Toilet flushing also generates aerosols (Barker and Jones, 2005; Johnson et al., 2013), representing a plausible inhalation pathway for *Legionella* and other respiratory pathogens in non-potable reuse systems. This toilet-plume aerosol exposure route has received less attention than showering in the QMRA literature but is directly relevant to the most common non-potable reuse application. The aerosol generation rate depends on flush energy, bowl geometry, and lid position, parameters that, like accidental ingestion volume, lack standardized measurement under realistic conditions.

The relative importance of the aerosol inhalation and faecal-oral ingestion pathways during toilet flushing depends on the reference pathogen. For *Legionella pneumophila*, which causes infection exclusively via the respiratory route, toilet-plume aerosols represent the sole relevant exposure pathway; here, aerosol particle size distribution and respirable fraction are the critical determinants (Hamilton et al., 2019). A relevant distinction, however, is that toilet cisterns hold unheated water and therefore offer far less favourable conditions for the growth of *Legionella* and other opportunistic premise-plumbing pathogens than the warm water delivered to showers; all else being equal, the *Legionella* risk from toilet-plume aerosols is expected to be inherently lower than that from shower aerosols. For enteric pathogens such as norovirus, the faecal-oral route via hand-to-mouth contact is conventionally assumed to dominate, but the hyperinfectivity of norovirus at very low doses (Teunis et al., 2008) means that even minor aerosol ingestion during a flush event could contribute meaningfully to total dose. The Teunis et al. (2008) model has itself been the subject of subsequent debate, with later analyses variously challenging and reaffirming its parameterization; consistent with the central argument of this review, it is treated here as a further input warranting re-evaluation against more recent challenge-study data rather than uncritical reuse, and is retained here only because it remains the most widely applied norovirus dose-response model and therefore the appropriate basis for comparison with existing assessments. In the absence of empirical ingestion data, risk assessors face a structural dilemma: conservative practice demands assuming the higher-risk pathway, yet doing so without measurement data risks compounding conservatism across multiple parameters. Future exposure measurement campaigns should address both pathways concurrently, combining aerosol sampling with hand-rinse and oral-swab protocols during toilet flush events, to enable pathway-specific dose estimation rather than reliance on aggregate assumptions.

A further consideration applies specifically to toilet flushing and laundry. In both, a substantial fraction of the pathogen load originates not from the supply water but from the user: faecal material introduced during defecation, or soiled items placed in the wash. Where this user-contributed load dominates, stringent microbial standards imposed on the treated supply water deliver limited marginal protection and may be over-conservative for these particular endpoints. At the same time, the incremental risk to an individual from re-exposure to their own faecal organisms is generally lower than the risk those same organisms would pose to the wider population, so household-level and population-level risk framings can legitimately diverge. Both effects argue for endpoint-specific exposure characterization rather than a single supply-water standard applied uniformly across reuse applications.

These calculations are deliberately simplified; a full Monte Carlo QMRA would propagate distributional uncertainty across all parameters simultaneously. Moving from point-estimate to stochastic inputs using the variance data now available from smart metering studies (e.g., SD ±1.8 for flush frequency, SD ±3.1 min for shower duration) would be expected to widen the confidence intervals of the estimated annual infection probability substantially, because the right tail of the exposure distribution includes high-use individuals whose risk may exceed the population mean by an order of magnitude or more. The direction in which the central estimate moves, however, is not arbitrary. For norovirus, whose hypergeometric dose–response function is concave in the low-dose region, Jensen's inequality runs counter to a common intuition: holding the mean dose fixed, adding exposure variability lowers the expected infection probability relative to the point-estimate calculation rather than raising it (Nilsen and Wyller, 2016). The principal value of distributional data therefore lies less in shifting the central estimate than in resolving the high-use right tail of the exposure distribution, where a minority of intensive users may individually exceed the population-mean risk by an order of magnitude and thereby govern whether a target fraction of the population breaches the health benchmark. This reinforces the value of empirical distributional data, not merely updated point values, as inputs to QMRA. The point is not to replace existing risk assessments but to demonstrate that the assumed-versus-measured parameter discrepancies identified in Table 2 are not merely academic. Although each individual correction is modest in log terms (typically 0.1–0.3 log; Section 6.2), these shifts are large enough to move a marginal system across the regulatory pass/fail boundary at the 10^{-4} annual-infection benchmark and, as Section 6.4 sets out, to change the treatment-technology selection that follows from it. This is strong evidence that QMRA exposure parameters must be grounded in empirical end-use data.

To illustrate the effect of distributional inputs, a simplified Monte Carlo analysis was conducted (n = 10,000 iterations) for the norovirus toilet-flushing scenario. Flush frequency was sampled from a lognormal distribution fitted to the Naseri et al. (2025) data (mean 6.4, SD 1.8 flushes/person/day); ingestion volume was sampled log-uniformly over the range 10^{-6} to 10^{-4} L/flush to reflect the complete absence of empirical constraint on this parameter; and norovirus concentration in treated greywater was held constant at 1 genomic copy/L (representing a well-functioning treatment system). Using the Teunis et al. (2008) hypergeometric dose–response model, the resulting distribution of annual infection probability spanned roughly two orders of magnitude (5th–95th percentile: 1×10^{-3} to 8×10^{-2}; median 9×10^{-3}), with every percentile lying above the 10^{-4} annual-infection benchmark; the corresponding point estimate using mean inputs was 2×10^{-2}. Two features of this result merit emphasis. First, the median lies slightly below the point estimate rather than above it, which is the expected consequence of the concavity of the dose–response function (Jensen's inequality; Nilsen and Wyller, 2016) and confirms that a single point estimate does not by itself understate central risk. Second, and more consequentially, because the analysis holds the dose–response model and its parameters fixed and varies only the exposure terms, the spread reported here reflects exposure uncertainty alone. Even so constrained, almost the entire spread is generated by the log-uniform ingestion-volume term, the one parameter for which no empirical measurement exists. The dominant source of exposure-side uncertainty in this endpoint is therefore the complete absence of measured ingestion data: even a well-functioning treatment system (1 copy/L) places high-use individuals an order of magnitude above the benchmark through this single unconstrained exposure term. The full calculation, including equations and the analysis script, is provided in Supplementary Information S1.

6.3. Geographic and contextual limitations of the mismatch analysis

An important caveat applies to the mismatch matrix: the empirical end-use data synthesized in Table 2 derive predominantly from studies conducted in North America, Australia, and Europe. Toilet flush frequency data from Naseri et al. (2025) reflect US households with predominantly single-flush toilets; in regions where dual-flush toilets dominate (e.g., Australia, parts of Europe, and much of East Asia), the relationship between flush frequency and water volume per event becomes more complex. Dual-flush systems offer a reduced-volume option (typically 3–4 L) for liquid waste and a full-volume flush (6–9 L) for solids. In practice, users sometimes perform double-flushes when the half-flush is insufficient for solids removal, which could increase the total number of flush events per day beyond what single-flush frequency data suggest. Conversely, the lower per-event volume in half-flush mode reduces the aerosol generation energy and potentially the accidental ingestion volume per event.

Whether these effects offset or compound is, frankly, an open question that cannot be resolved with the data currently available. The net impact on QMRA exposure depends on the joint distribution of flush type selection, per-event volume, and aerosol generation, none of which have been characterised for dual-flush systems in a QMRA context. The 6.4 flushes/person/day figure from Naseri et al. (2025) should therefore be interpreted as representative of the US single-flush context rather than a universal value. Shower durations measured by Mayer et al. (1999) and DeOreo et al. (2016) reflect North American norms; in water-scarce regions with higher tariffs or cultural norms favouring shorter showers, measured durations may be lower, narrowing the mismatch with QMRA assumptions. Conversely, in contexts with unrestricted hot water availability and no water pricing signals, showers may be longer. A further, largely unexamined determinant of shower-aerosol exposure is the showerhead itself: spray pattern, droplet-size distribution, and flow setting can alter the respirable aerosol fraction substantially, yet, unlike duration, this factor is essentially absent from QMRA exposure assessment and warrants explicit characterisation.

These geographic dependencies do not invalidate the mismatch analysis but delimit its direct applicability. The key finding, that QMRA studies do not cite or reference empirical end-use data when specifying exposure parameters, holds regardless of geography. What the mismatch matrix demonstrates is a methodological gap (assumptions not grounded in measurement), not a universal numerical correction. For site-specific application, the framework proposed in Section 8 would require locally measured end-use data, reinforcing the need for smart metering infrastructure in diverse settings.

The data gap is particularly acute in low- and middle-income countries (LMICs), where decentralized greywater reuse is often most critical for water security yet smart metering infrastructure is largely absent. In such contexts, practitioners cannot rely on the fixture-level disaggregation datasets available from North America and Australia. Alternative approaches to deriving empirically grounded exposure parameters include structured time-use diary studies (recording self-reported fixture use events over multi-day periods), short-duration flow logging with portable ultrasonic meters deployed during targeted monitoring campaigns, and leveraging mobile-phone-based survey platforms to collect behavioural data at low cost. While these methods yield lower-resolution data than automated smart metering, they can provide region-specific distributional estimates of flush frequency, shower duration, and water contact patterns that are substantially more informative than the generic assumptions currently embedded in QMRA models. International guidance documents (e.g., ISO 20426, WHO Guidelines) should explicitly acknowledge the need for context-appropriate exposure measurement methods and provide tiered protocols scaled to available monitoring infrastructure.

In practical terms, these differences are not academic. Changes in estimated exposure can translate directly into shifts in required log-reduction targets, potentially altering system configuration, treatment intensity, and cost. In this sense, mis-specified exposure assumptions propagate through the entire design chain.

6.4. Implications for system design and regulation

The mismatches documented above have concrete implications for decentralized reuse system design and regulatory compliance. Consider a household-scale greywater recycling system designed for toilet flushing in a four-person household. Using the standard QMRA assumption of 5.0 flushes/person/day, the system must treat and supply 20 flush events daily. Using the measured value of 6.4 flushes/person/day (Naseri et al., 2025), the demand increases to 25.6 events per day, a 28 % increase that affects both treatment throughput and storage sizing. If the storage tank was sized for average daily balance based on the lower flush rate, the system will experience more frequent potable water top-ups, reducing effective potable savings below design predictions (a system sized for 5.0 flushes/person/day meets only about 78 % of the measured flush demand, so realized potable savings fall short of the design value by a comparable margin).

Conversely, the shower duration mismatch suggests that some current design requirements for *Legionella* control may embed safety margins that are real but unacknowledged. If the assumed 15-minute shower overestimates actual exposure by a factor of approximately two, then log-reduction targets derived from this assumption incorporate conservatism that, while potentially desirable, is not transparently documented. Making this safety margin explicit, by recalculating targets using measured shower durations, would allow regulators to distinguish between intentional conservatism and artefactual overprotection, potentially enabling less costly treatment trains for scenarios where measured exposure is demonstrably lower than assumed. These remarks align with a broader argument against unquantified conservatism in risk-based water management (Chik et al., 2018; Parkhurst and Stern, 1998): rather than relying on conservatism that may be excessive in some cases and insufficient in others, it is preferable to apply an explicit, well-reasoned, and equitable safety factor.

For accidental ingestion, the absence of empirical data prevents any design adjustment. Future measurement campaigns should prioritise this parameter using tracer-based methods, for example, fluorescent dye tracers added to non-potable supply lines, combined with wipe sampling of hands and oral mucosa after toilet use events, could provide the first empirical estimates of accidental ingestion volumes under realistic residential conditions. Such tracer-based studies involve human subjects research with distinct ethical and practical challenges: informed consent protocols for household toilet-use monitoring, the logistical difficulty of capturing representative samples of rare stochastic events (splash, aerosol contact), and the need for institutional review board approval for protocols involving intentional exposure characterization. Pilot studies in controlled laboratory settings with simulated flush events may provide initial bounding estimates before progressing to in-home campaigns. Until such data exist, QMRA practitioners should at minimum report the sensitivity of their results to the assumed ingestion volume, making the embedded uncertainty transparent to regulators and designers.

The issue is not the absence of perfect measurements, but the continued reliance on point estimates that are misaligned with observed variability. Even coarse empirical distributions would represent a substantial improvement over current practice, particularly given the sensitivity of QMRA outputs to upper-tail exposure events.

6.5. From corrected exposure to treatment-design and operational targets

The preceding subsections establish that the exposure corrections are real but individually modest in log terms. Their engineering consequence becomes explicit once they are mapped onto the log-reduction value (LRV), the quantity that links risk analysis to treatment design. The LRV is the base-10 logarithmic reduction in pathogen concentration across the treatment process required to meet the health target for a given exposure (a reverse-QMRA calculation; Schoen et al., 2017; US EPA, 2012). Because the log-removals of sequential barriers are additive, the total LRV of a multi-barrier train is the sum of its stages. Because annual risk tracks dose almost linearly in the low-dose region (Section 5.2), each exposure correction translates additively onto the required LRV: a parameter that raises estimated dose by a factor of 10 raises the required LRV by one log.

On the enteric side, correcting toilet-flush frequency from the assumed 5.0 to the measured 6.4 events per person per day (Naseri et al.,

2025) raises the required norovirus LRV by approximately 0.1 log, a small but systematic tightening of the treatment target. Far more consequential is the accidental-ingestion volume: because this parameter is unmeasured and plausibly spans two orders of magnitude (Table 2; Section 6.2), it alone can move the required enteric LRV by up to one log. The design implication is concrete: a barrier sized against a single assumed ingestion volume is not robust to this uncertainty, and a multi-barrier train (for example, membrane filtration followed by disinfection) is justified not by conservatism for its own sake but by the demonstrated breadth of the exposure distribution.

On the inhalation side, the shower-duration correction works in the opposite direction. An assumed 15-minute shower overestimates the measured mean (7.8 min) by a factor of about 1.9, embedding roughly 0.3 log of unacknowledged conservatism in any *Legionella* target derived from it. This margin propagates directly into disinfection design. For chlorination, inactivation follows Chick–Watson kinetics in which log-inactivation is proportional to the CT product (disinfectant concentration multiplied by contact time; Haas et al., 2014); for ultraviolet systems, log-inactivation is proportional to delivered fluence. A 0.3 log over-specification therefore inflates the required CT or UV dose proportionally, with three measurable penalties: higher chemical demand and possible disinfection by-product formation for chlorine, higher electrical energy per order removed (EE/O) for UV, and, at the margin, the selection of a more intensive barrier than the health target actually requires (a higher required CT can be met by increasing contact time, for example tank volume, as well as disinfectant concentration, so the chemical-demand and by-product penalties apply specifically where concentration is raised). Making the margin explicit, by deriving targets from measured rather than assumed shower durations, lets a designer either bank the safety factor deliberately or reallocate it to the parameters (such as ingestion volume) where the real uncertainty lies.

Taken together, these translations are what close the data-to-system gap named in the title. End-use characterization supplies the exposure distributions; QMRA converts them into LRVs; and the LRVs determine treatment-technology selection, disinfectant dosing, by-product and energy footprints, and ultimately cost. The same fixture-level data that correct the QMRA inputs also constrain supply–demand balance and storage sizing (Section 4.2), so a single monitoring investment serves the hydraulic and the microbial sides of the design simultaneously. The contribution of this review is to make that coupling explicit and to show that it changes design outcomes, not merely risk estimates.

7. Smart metering, fixture-level monitoring, and data infrastructure

7.1. Evolution of end-use demand characterization

Residential water end-use characterization has evolved from aggregate billing data to high-resolution, fixture-level profiles. The foundational Residential End Uses of Water study (Mayer et al., 1999) established the first large-scale breakdown of household consumption by fixture in North America. Its successor (DeOreo et al., 2016) updated these profiles from over 700 households, documenting shifts driven by water-efficient appliances. In Australia, the Southeast Queensland Residential End Use Study (Beal and Stewart, 2011) provided detailed end-use data linked to smart meter disaggregation. Willis et al. (2011) used the same dataset to quantify how environmental attitudes affect demand profiles, a finding relevant to reuse system sizing, where user behaviour determines both greywater supply and non-potable demand.

7.2. Automated disaggregation and its untapped potential for reuse

Manual flow-trace analysis (the visual, fixture-by-fixture classification of high-resolution flow records by a trained analyst, who matches each draw to a fixture from its volume-and-duration signature) does not scale, motivating automated disaggregation algorithms. Froehlich et al. (2009) demonstrated single-point pressure-wave sensing (HydroSense), while Nguyen et al. (2013) developed Autoflow, applying pattern recognition to classify fixture events with accuracies exceeding 85 %. Subsequent work refined these approaches using deep learning and self-organizing maps (Beal et al., 2013; Pastor-Jabaloyes et al., 2018). Cominola et al. (2015) provided a comprehensive review of smart meter analytics for residential water demand, and Boyle et al. (2013) reviewed intelligent metering for urban water more broadly.

Most recently, Naseri et al. (2025) reported fixture-level end-use statistics from 26,441 US households, providing the largest dataset to date of empirically measured toilet flush frequencies, shower events, and clothes washer cycles at the population level. This study, combined with earlier fixture-level datasets, provides a rich empirical basis that could directly inform QMRA exposure parameters. Yet QMRA studies for non-potable reuse have not drawn on this body of fixture-level evidence, neither the recent large-scale dataset of Naseri et al. (2025) nor the end-use studies available for a decade or more (DeOreo et al., 2016; Mayer et al., 1999), as a source for exposure assumptions.

The relevance of this body of work to decentralized reuse is substantial but unrealized. End-use disaggregation provides exactly the fixture-resolved, time-stamped data needed for greywater supply–demand modelling, storage sizing, diurnal treatment load estimation, and, as demonstrated in Section 6, empirically grounded QMRA exposure parameters. Because fixture-level traces identify which streams contribute to the mixed flow and when, the same data also enable reconstruction of the composition and pollutant loading of the bulk raw greywater itself, prior to any treatment, a use of end-use monitoring that has so far been largely overlooked but bears directly on treatment-train selection and influent design. The smart metering and water reuse literatures have developed in parallel with minimal cross-referencing; bridging them is the central recommendation of this review.

This disconnect matters because QMRA does not operate on mean conditions alone. Exposure frequency, duration, and volume jointly determine dose, and because risk tracks dose closely in the low-dose region, a mis-specified exposure distribution biases the risk estimate by a similar factor; where several exposure terms are biased in the same direction, the effects compound.

8. Toward an integrated framework: linking end-use data to reuse design and QMRA

The preceding sections document a three-way gap: greywater characterization studies report aggregate volumes and grab-sample quality; design standards specify fixed inputs; and QMRA frameworks assume exposure parameters that, as Table 2 demonstrates, diverge measurably from observed water use behaviour. Meanwhile, smart metering research generates the high-resolution data that could inform all three domains but is rarely connected to them.

An integrated framework is proposed, operating at three levels (Fig. 2 illustrates the conceptual linkages). This framework is structured as an adaptive management cycle consistent with the iterative plan–implement–monitor–adapt approach advocated by the US EPA (2025) Risk-Based Framework for Water Reuse. At the data level, fixture-resolved smart meter traces provide temporal profiles of both greywater-generating events (showers, handbasins, laundry) and non-potable demand events (chiefly toilet flushing, with irrigation where applicable). At the design level, these profiles feed into storage sizing models, treatment capacity specification, and overflow risk estimation, enabling probabilistic rather than deterministic system design. At the risk assessment level, the same profiles provide empirically grounded inputs for QMRA: measured flush frequencies replace assumed values, measured shower durations replace upper-bound defaults, and measured occupancy patterns replace census-level averages. Critically, by using distributional rather than point-estimate inputs, QMRA can propagate realistic parameter uncertainty through to risk outputs and

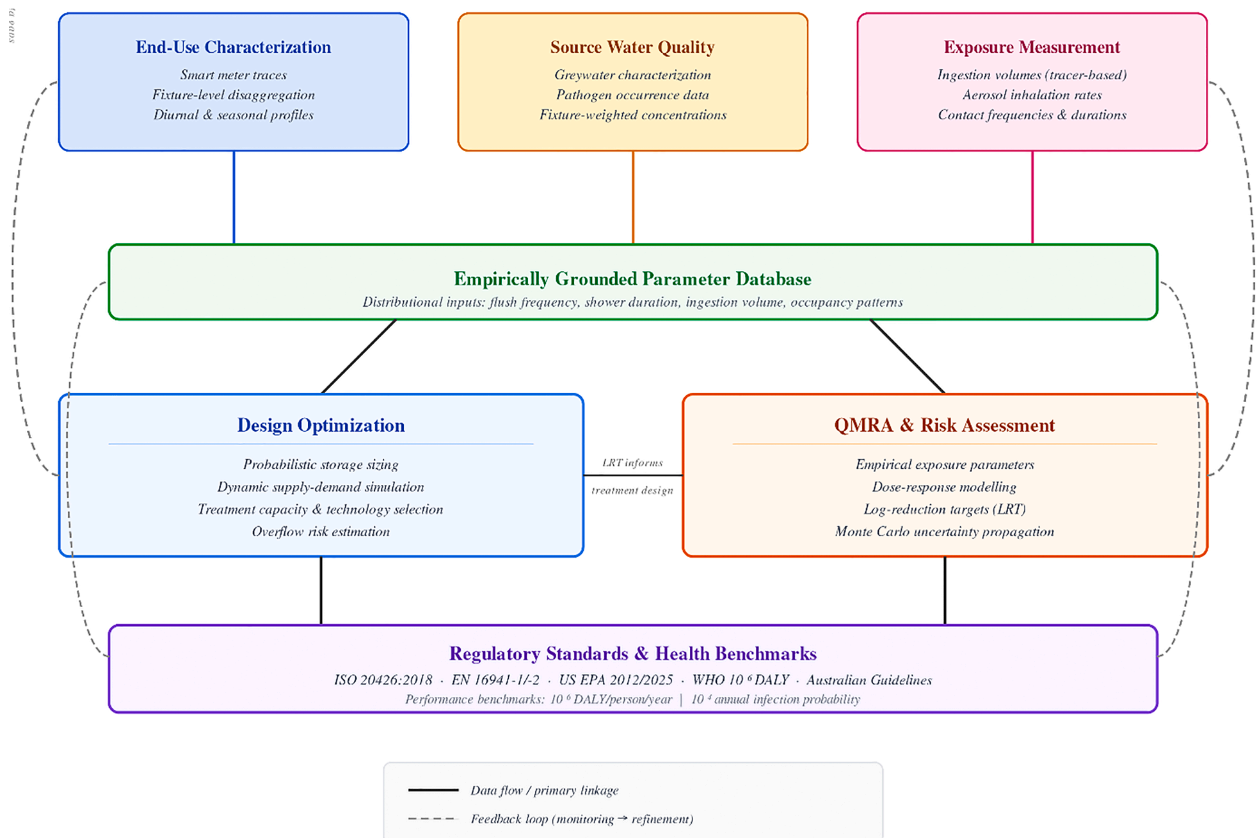

Fig. 2. Conceptual framework linking fixture-level smart meter data to decentralized reuse system design and QMRA. Three data streams (end-use characterization, source water quality, and exposure measurement) feed into design optimization and risk assessment, with regulatory standards providing the benchmark framework. Dashed arrows indicate feedback loops where monitoring data refine design parameters and risk estimates over time, consistent with the adaptive management cycle (plan–implement–monitor–adapt).

derived log-reduction targets (Jahne et al., 2023; US EPA, 2025).

The integration would also improve source water characterization for QMRA. By knowing which fixtures contribute to the greywater stream (and when), microbial source concentrations can be weighted by fixture rather than assumed as a homogeneous mixture. A greywater system collecting only from showers and handbasins will have a systematically different pathogen profile than one also collecting laundry water, a distinction captured by fixture-level data but lost in aggregate characterization (Christova-Boal et al., 1996; Friedler, 2004). For illustration only (the concentrations below are assumed, not measured), consider a greywater system collecting from showers (flow-weighted fraction $f_s = 0.65$) and handbasins ($f_h = 0.35$). If the mean norovirus concentration is $C_s = 10$ genomic copies/L in shower water and $C_h = 50$ genomic copies/L in handbasin water (reflecting hand-washing after toilet use), the flow-weighted source concentration is $C_{mix} = f_s \times C_s + f_h \times C_h = 0.65 \times 10 + 0.35 \times 50 = 24$ gc/L, compared with 30 gc/L from an unweighted arithmetic mean. This 20 % difference in assumed source concentration propagates directly into the QMRA dose calculation and derived log-reduction target, illustrating why fixture-resolved data matter for risk assessment.

Implementation requires living laboratory platforms where smart metering, water quality monitoring, and reuse system performance data are collected concurrently. Several such platforms exist, including the HSB Living Lab in Gothenburg, Sweden, where fixture-level water use in 29 apartments has been monitored alongside greywater quality since 2019 (Knutsson and Knutsson, 2021), and the Technion greywater pilot in Haifa, where greywater treatment and reuse in multi-storey buildings have been studied (Blanky et al., 2017; Friedler et al., 2005). Yet none of these platforms have systematically linked their end-use data to QMRA inputs or reuse system design optimization. This is a missed opportunity. The data exist; the integration does not.

9. Research priorities and conclusions

This review has demonstrated that decentralized water reuse design and risk assessment are built on exposure assumptions that diverge measurably from empirical end-use data. Shower durations are overestimated by approximately a factor of two in *Legionella* risk models, toilet flush frequencies are underestimated by up to approximately 28 % (relative to the largest available dataset; Naseri et al., 2025), and the most influential QMRA parameter, accidental ingestion volume, has, to the authors' knowledge, never been empirically measured. Bridging this data-to-system gap requires coordinated advances in five areas.

First, empirical exposure measurement campaigns are urgently needed. Tracer-based ingestion studies under realistic non-potable use conditions, particularly aerosol characterization during toilet flushing and showering in buildings with reclaimed water, would address the most consequential gap in the QMRA evidence base.

Second, fixture-level smart meter data should be systematically coupled to QMRA exposure models. The algorithms for end-use disaggregation are mature and the population-scale datasets now exist (Naseri et al., 2025); what is lacking is the application of their outputs to risk assessment frameworks.

Third, smart meter data should be coupled to greywater system design models, enabling dynamic supply–demand simulation, probabilistic storage sizing, and treatment load forecasting that reflect actual household behaviour rather than design averages.

Fourth, standardized living laboratory protocols are needed to generate comparable datasets across sites, climates, and building typologies. Ongoing work at the HSB Living Lab at Chalmers University of

Technology suggests that a minimum of 12 months of concurrent fixture-level metering and grab sampling at weekly intervals is required to capture seasonal variability in both demand profiles and microbial concentrations, but this tentative figure needs validation at other sites before it can serve as a general recommendation.

Fifth, regulatory frameworks, including ISO 20426, EN 16941, and US EPA guidance, should evolve from prescriptive, fixed-parameter approaches to performance-based, data-driven frameworks that accommodate site-specific variability. The Australian Guidelines for Water Recycling (NRMMC, EPHC and NHMRC, 2006) provide a model, but their implementation would benefit from the stochastic input distributions that end-use monitoring can provide. The LRT framework (Jahne et al., 2023; US EPA, 2025) offers a natural integration point, since log-reduction targets are directly computable from empirical exposure parameters.

In conclusion, the tools and data needed to close the gap between end-use characterization and decentralized reuse design largely exist, but they reside in separate disciplinary communities. The mismatch matrix presented here (Table 2) provides what is, to the authors' knowledge, the first systematic, quantitative demonstration that this disconnect has material consequences not only for health risk estimation but, through the log-reduction targets these parameters propagate into, for treatment-technology selection, disinfectant dosing, and the chemical and energy footprint of decentralized reuse systems (Section 6.5). Integrating residential water demand science, water reuse engineering, and quantitative risk assessment into a coherent, data-driven design paradigm is both feasible and overdue.

The question is therefore no longer whether better exposure data exists, but whether QMRA frameworks can continue to justify assumptions that are both unvalidated and, in some cases, inconsistent with observed behaviour.

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Jesper Knutsson: Conceptualization, Formal analysis, Investigation, Methodology, Writing – original draft, Writing – review & editing.

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Data availability

This review synthesises data from previously published studies, which are cited throughout; no new primary data were generated. The equations, parameter values and analysis script underlying the sensitivity and Monte Carlo calculations are provided as Supplementary Information S1.

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