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Annual Metabolic Monitoring and Clinical Follow-Through in Adults Prescribed Antipsychotics in Community Care: A Restricted-Search Systematic Review and Exploratory Meta-Analysis

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Abstract

Annual metabolic monitoring is a minimum component of care for many adults receiving antipsychotics, but recorded testing and subsequent clinical action are distinct outcomes. This review assessed annual monitoring in community care and examined evidence of follow-through after abnormalities. Europe PMC was searched from inception to September 8, 2026, using explicit antipsychotic, metabolic monitoring, and annual/community care concepts, supplemented by seven additional study reports available to the research team. Adult outpatient and primary care studies reporting annual or annual-equivalent monitoring were eligible. Study screening, data extraction, and quality appraisal were performed by the first author, with methodological supervision and review by the second author. Exact patient-level counts for annual coverage during ongoing treatment were synthesised using logit random-effects models, restricted maximum likelihood, and modified Hartung-Knapp confidence intervals (CIs). Treatment-initiation windows, frequency targets, paired time points, and inconsistent denominators were not pooled with annual coverage. In total, 16 reports representing 15 study programmes were included; 15 reports remained unavailable for full-text assessment after the original retrieval attempts. Five studies contributed to at least one meta-analysis. Glycaemic monitoring was 42.4% (95% CI = 18.8-70.1; five studies, 18,942 participants), lipid monitoring 33.0% (9.2-70.6; four studies, 2,188 participants), blood pressure recording 29.6% (1.7-91.2; four studies, 2,188 participants), and weight recording 31.6% (0.3-98.5; three studies, 222 participants). Heterogeneity was extreme (I^2 = 96.8-99.2%). No compatible dataset supported pooling timely clinical action among patients with newly confirmed abnormalities. Selection and documentation bias were common. Available studies show substantial gaps in documented annual monitoring, with marked variation between services. The pooled estimates are exploratory and are not reliable service benchmarks. Evidence on timely responses to abnormal results remains insufficient. Restricted searching, inaccessible reports, and the absence of independent duplicate screening and extraction limit confidence in the synthesis.

Categories: Psychiatry, Quality Improvement, Endocrinology/Diabetes/Metabolism
Keywords: community mental health, meta-analysis, metabolic monitoring, primary care, systematic review

Introduction And Background

Metabolic monitoring is an essential part of antipsychotic care because changes in weight, glycaemia, lipids, and blood pressure may require clinical review. Consensus recommendations and National Institute for Health and Care Excellence (NICE) guidance include assessment before treatment, closer surveillance during early treatment, and continued monitoring thereafter [1,2]. Annual assessment is a minimum maintenance standard for several domains; it does not replace the more frequent weight monitoring or disease-specific follow-up required in some patients [1,2].

Current guidelines recommend metabolic assessment before initiating antipsychotic treatment, during early treatment, and throughout maintenance therapy. According to NICE guidance, weight should be monitored weekly during the first six weeks, at 12 weeks, at one year, and annually thereafter. Blood pressure, glycaemic parameters (fasting glucose or HbA1c), and lipid profiles should be reassessed at 12 weeks, one year, and annually thereafter. Waist circumference should also be assessed annually. More frequent monitoring may be required according to the prescribed antipsychotic and individual metabolic risk [1,2].

Monitoring outcomes depend on what a study measures. An integrated care study distinguished laboratory orders from completed tests and showed that extending the observation window changed the proportion classified as screened [3]. Primary care studies have assessed individual laboratory components, annual testing frequencies, and specified intervals after treatment initiation [4,5]. Weight, body mass index (BMI), and waist circumference are also distinct measurements within monitoring recommendations [1,2]. These differences need to be retained when comparing reported percentages or selecting studies for pooling.

An earlier systematic review demonstrated substantial gaps in guideline-concordant metabolic screening [6]. A review of improvement interventions reported increased screening but identified weaknesses in study quality, while a more recent scoping review found continuing variation in monitoring practices and poorly defined professional responsibilities [7,8]. A further question concerns what happens after an abnormal result. NICE recommends assessment and management of identified metabolic problems, not measurement alone [2]. Monitoring coverage and clinical response therefore require different denominators: all patients eligible for a test versus patients with an abnormal finding for whom action is relevant.

This review addresses annual metabolic monitoring in adults prescribed antipsychotics in outpatient, community, and primary care services. Its objectives were to estimate documented annual coverage for individual metabolic domains, describe implementation evidence, and determine whether available studies quantify timely action after abnormalities. The review presents a restricted-search synthesis with exploratory pooling, rather than an estimate of global performance across all antipsychotic services.

Review

Methodology

Design and Protocol Amendments

Reporting was structured around the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) 2020 guidelines [9]. An existing project protocol, which had not been prospectively registered, proposed a broader all-setting review, six bibliographic databases, independent duplicate review, a generalised linear mixed model for proportions, and separate synthesis of clinical responses. The completed workflow differed materially. On 8-9 September 2026, after initial examination of the available reports, the scope was restricted to annual and annual-equivalent community care outcomes; the executable search used Europe PMC; and exact-count annual coverage was analysed on the logit scale. These amendments were made with knowledge of the initial evidence and were not prospectively registered. Their rationale and implications are described below. Table *1* summarises the departures from the original protocol.

Protocol intention	Completed method and consequence
All care settings and treatment stages	Restricted after initial source review to community/outpatient care and annual or annual-equivalent outcomes. Reduces scope and introduces post hoc selection risk
Six databases and registries	One Europe PMC search; no registry search or formal citation-chasing programme. Comprehensive coverage is not claimed
Two independent reviewers	Screening, data extraction, and appraisal were undertaken by the first author with methodological supervision and review by the second author. Independent duplicate screening, extraction, and adjudication were not performed
Full reports without language restriction; author queries	No search language limit; the Spanish-language report was translated and assessed by the research team. Fifteen reports remained unavailable for full-text assessment; no author contact was completed
GLMM proportions	REML logit proportions with modified Hartung–Knapp intervals; all primary pooled cells non-zero. Exact-count, maintenance annual coverage subset defined after source inspection
RoB 2 or ROBINS-I and GRADE for comparative outcomes	JBI-based item appraisal for descriptive outcomes and narrative appraisal of comparisons. No formal comparative certainty rating or effect pool
Monitoring and action analysed separately	Retained. Existing diagnosis treatment and all-patient referrals were not treated as post-abnormality response rates

TABLE 1: Protocol intentions and completed methods.

Amendments were finalised during 8–9 September 2026. They were not registered prospectively and should not be interpreted as prespecified in the original protocol.

REML: restricted maximum likelihood; RoB 2: Risk of Bias 2; ROBINS-I: Risk of Bias in Non-randomized Studies - of Interventions; GRADE: Grading of Recommendations, Assessment, Development, and Evaluation; JBI: Joanna Briggs Institute

Eligibility

Eligible studies concerned adults aged at least 18 years taking first- or second-generation antipsychotics for any indication in outpatient, community, or primary care services. Mixed-age samples were eligible only when adult results were separable or at least 80% of participants were demonstrably adults. Clinical audits, observational studies, and implementation evaluations were considered. Reports required an identifiable antipsychotic-exposed denominator and an annual or annual-equivalent monitoring outcome: a 12-month coverage window, a first-year or annual checkpoint assessment, or a study-defined yearly frequency measure. Such definitions were retained separately during synthesis.

Inpatient-only studies, research measurements without an assessment of routine monitoring, reports limited to treatment initiation or early follow-up without a yearly outcome, and reports without verifiable adult eligibility were excluded. A report could be included narratively when its yearly outcome was relevant but insufficiently comparable or internally consistent for pooling. No language restriction was applied in searching; the eligible Spanish-language report was translated and assessed for eligibility and extraction by the research team.

Information Sources and Selection

The final search was run through the Europe PMC REST API on 8 September 2026, from 1 January 1900 to that date. All 241 returned records were retrieved in one response. Search reporting follows PRISMA-S [10]. Europe PMC was one search platform; MEDLINE, Embase, PsycINFO, CINAHL, Web of Science, and CENTRAL were not searched independently. Preliminary broad queries and targeted bibliographic lookups informed scope and source verification but were not counted as additional systematic searches. Trial registries, formal forward citation searches, librarian peer review, and author contact were not completed.

Seven additional reports available to the research team were checked against the search output. Preprint and journal versions of the same report were linked and the journal version retained. Companion publications reporting the same programme were linked at the study level. Titles and abstracts were screened, and potentially relevant reports were sought through the available study files, Europe PMC, publisher pages, and available author-hosted copies. Reports whose full content could not be retrieved remained unresolved and were not treated as ineligible studies. Screening, data extraction, count checking, and study appraisal were undertaken by the first author, with methodological supervision and review by the second author; independent duplicate screening and extraction were not performed.

Exact Search

The final search was conducted using the Europe PMC REST API on 8 September 2026 and covered publications from 1 January 1900 through 8 September 2026, without a language restriction. All search terms were applied to the title and abstract fields using TITLE_ABS. Four concept groups were combined with AND, while terms within each group were combined with OR. The antipsychotic group comprised antipsychotic* and neuroleptic*; the metabolic group comprised metabolic and cardiometabolic; the monitoring group comprised monitor*, screen*, and audit*; and the annual or community care group comprised annual*, yearly, community, outpatient*, the phrase “primary care,” and ambulatory. Asterisks were retained as truncation operators, and quotation marks specified the phrase “primary care.” The publication date restriction was applied using FIRST_PDATE:[1900-01-01 TO 2026-09-08].

The search was submitted to https://www.ebi.ac.uk/europepmc/webservices/rest/search through the query parameter with standard URL encoding. The API parameters were format=json, pageSize=1000, and resultType=core. The response reported a hitCount of 241, and all 241 returned records were retained in a single response. These details are reported in accordance with PRISMA-S [10].

A preliminary, broader scoping query returned 1,155 records, of which 1,000 were downloaded. Because that retrieval was incomplete and was used only to inform the scope, it was not treated as a second formal search, and its records were not added to the PRISMA counts.

Deduplication and Report Linkage

Two preprints were linked to their corresponding journal articles and removed before screening. The preprint of the Ali et al. two-practice study (doi: 10.21203/rs.3.rs-2972912/v1) was linked to its journal article (doi:10.1007/s11096-023-01642-5) [4]. The Woodall et al. preprint (doi: 10.1101/2024.06.18.24309122) was linked to its journal article (doi: 10.3399/BJGP.2024.0367) [11]. Of the seven supplied full reports, six were already represented in the search: Al-Saati et al. [12], the two Ali et al. reports [4,5], Mackin et al. [13], Barnes et al. (2007) [14], and Soda et al. [15]. Michael et al. was the sole additional supplied full report [16]. Barnes et al. (2007) and Barnes et al. (2015) were linked as one study programme [14,17]. Barnes et al. (2008) was identified as a companion publication describing the same baseline programme and a one-year re-audit [18]; it was linked to the included Barnes programme [14,17] rather than retained as a separate unresolved report. Linked publications were not counted as independent cohorts.

Outcomes and Extraction

The primary quantitative estimand was the proportion of eligible patients with a documented completed measurement during a defined year of ongoing antipsychotic care, using the earliest reported preintervention or routine care period per study. Domains were study-defined glycaemic monitoring, lipid monitoring, blood pressure, and weight. Glycaemic assays included glucose and/or HbA1c as explicitly defined by each study. Lipid definitions were recorded without assuming that any test represented a full panel. BMI, waist circumference, obesity composites, and complete monitoring composites were kept separate.

Exact numerators and denominators were preferred over printed percentages. Counts were not reconstructed from rounded percentages, summed across overlapping assays, or multiplied from percentages using an incompatible sample size. First-year initiation windows, baseline plus follow-up requirements, frequency adherence, orders, person-year averages, and repeated monthly denominators were not pooled with annual patient-level coverage. In before-after cohorts, the baseline contributed once to the prevalence meta-analysis; follow-up observations were described separately. No independent-sample effect was calculated from paired marginal counts without paired transition data. Source locations and count reconciliation decisions for the pooled studies are presented in Table *2*.

Primary study	Source location	Extraction decision
Barnes et al. (2007) [14]	Figure 2, journal page 1401; Methods	Only documented result/measurement counts used. References to screening without a recorded result excluded from the numerator. Obesity composite kept separate from weight
Mangurian et al. (2018) [3]	Results and screening comparison table; Population and Measures	9,247 completed tests among 16,754 eligible adults in 2014; no known diabetes. Orders and two-year testing excluded
Keenan et al. (2020) [19]	Table 1; study period in Methods	Exact overall counts used: HbA1c: 82, lipids: 77, blood pressure: 101, weight: 95; N = 117. Sex-stratum sums confirm 101 and 95 despite inconsistent printed percentages
Kelly et al. (2022) [20]	Chronic psychosis Methods and Results; baseline and re-audit monitoring data	Baseline annual record counts for 41 adults only. Do not combine with first-episode cohort or re-audit. Exact counts take priority over rounded percentages
Al-Saati et al. (2025) [12]	Table 1, journal page 2	2020 preintervention counts only; same 64 patients in 2021. Glucose uses reported union, not fasting glucose plus HbA1c. Any fasting lipid test uses reported union

TABLE 2: Source locations and extraction decisions for pooled studies.

Non-pooled discrepancies were retained explicitly. Al-Saati et al. classified three baseline patients as fully monitored despite zero recorded BMI and waist measurements; the composite was therefore not interpreted as complete recording of all core measures [12]. The IMRD report contains discordant aggregate entries, and its yearly category concerns 85-365 days after initiation [5]. The Ali et al. two-practice study's available value counts describe record availability, rather than the fraction monitored within each year [4]. Mohammed and Al Naeem's yearly BMI completion differs between the missing value summary and the paired analysis sample: 91 of 123 patients were described as having incomplete one-year BMI data, whereas 33 patients were reported in the baseline to one-year paired analysis [21]. These discrepancies were not resolved by inventing a denominator, multiplying rounded percentages, or assuming that missing values represented completed measurements.

For clinical follow-through, the intended outcome was any documented action among unique patients with a newly confirmed metabolic abnormality within an explicit follow-up interval. Established diagnosis pharmacotherapy, all-patient referrals, and reported treatment without temporal linkage were extracted as related outcomes, not substituted for this response estimand. Missing documentation was interpreted as absence of a recorded measurement, not proof that no care occurred.

Appraisal and Statistical Analysis

Descriptive monitoring outcomes were assessed with the nine-domain Joanna Briggs Institute (JBI) prevalence checklist [22], interpreted for record-based monitoring rather than disease prevalence. Item-level

judgements and rationales are presented in Table 3 and the appraisal findings; no summed quality score was used. The small primary pools did not support stratification by appraisal result. Implementation comparisons were examined separately for confounding, changing samples, repeated observations, and outcome ascertainment. Formal Risk of Bias in Non-randomized Studies - of Interventions (ROBINS-I) and Grading of Recommendations, Assessment, Development, and Evaluation (GRADE) assessments were not completed, so no validated comparative risk-of-bias category or GRADE certainty rating is claimed.

Programme	D1	D2	D3	D4	D5	D6	D7	D8	D9
Barnes et al. [14,17]	Y	N	Y	Y	U	Y	U	U	U
Mangurian et al. [3]	Y	Y	Y	Y	Y	Y	Y	Y	Y
Keenan et al. [19]	Y	N	N	Y	Y	Y	U	U	U
Kelly et al. [20]	Y	Y	N	Y	Y	Y	U	U	U
Al-Saati et al. [12]	Y	N	N	Y	N	Y	U	Y	N
Kaoser et al. [23]	Y	Y	Y	Y	Y	Y	Y	Y	Y
Ali et al. practices [4]	Y	N	Y	Y	U	Y	U	U	U
Ali et al. IMRD [5]	Y	Y	Y	Y	U	Y	U	N	U
Al-Awad et al. [24]	Y	Y	N	Y	Y	Y	U	U	U
Schneiderhan et al. [25]	Y	N	N	Y	U	Y	U	Y	U
Breslau et al. [26]	Y	N	Y	Y	Y	Y	Y	Y	Y
Mohammed and Al Naeem [21]	N	N	N	Y	N	Y	U	N	N
Vivacqua et al. [27]	Y	Y	Y	Y	U	Y	U	U	U
Nicol et al. [28]	Y	Y	Y	Y	Y	Y	Y	Y	Y
Morrato et al. [29]	Y	Y	Y	Y	U	Y	Y	U	U

TABLE 3: Item-level appraisal of included study programmes.

The JBI prevalence checklist was applied to the descriptive monitoring component of each programme [22]. Domain codes (D1–D9): D1 appropriate frame for the stated population; D2 appropriate sampling; D3 adequate sample size; D4 subjects and setting described; D5 adequate coverage of the identified sample; D6 valid definition of recorded monitoring; D7 consistent ascertainment; D8 appropriate analysis of the reported descriptive outcome; D9 adequate response or missing-record handling. D3 used a conservative precision rule for a single proportion: about 385 independent observations for a 95% interval with ±5 percentage-point precision at 50%; this operational rule was added during appraisal and is not a JBI scoring instruction. No numerical total or validated overall category was assigned. Judgements were made by the first author with methodological review by the second author. Data sources: Barnes et al. [14,17], Mangurian et al. [3], Keenan et al. [19], Kelly et al. [20], Al-Saati et al. [12], Kaoser et al. [23], Ali et al. two practices [4], Ali et al. IMRD [5], Al-Awad et al. [24], Schneiderhan et al. [25], Breslau et al. [26], Mohammed and Al Naeem [21], Vivacqua et al. [27], Nicol et al. [28], and Morrato et al. [29]. IMRD, IQVIA Medical Research Data. These are the review authors' appraisal judgements based on the cited reports.

JBI: Joanna Briggs Institute; Y: yes; N: no; U: unclear

For at least three clinically related cohorts with exact counts, the monitored proportion in study i was $p_i = n_i/N_i$, where n_i is the number with a recorded completed measurement, and N_i is the eligible sample. The logit-transformed proportion y_i and its estimated within-study sampling variance v_i were calculated as follows:

$$y_i = \ln[p_i/(1 - p_i)] = \ln[n_i/(N_i - n_i)] \quad (1)$$

$$v_i = \mathrm{Var}(y_i) \approx 1/n_i + 1/(N_i - n_i) \quad (2)$$

Here, ln denotes the natural logarithm. Equation 2 is the binomial delta-method variance on the logit scale and requires $0 < n_i < N_i$. It measures within-study sampling uncertainty, separately from the between-study variance τ^2.

Between-study variance was estimated by restricted maximum likelihood (REML). The modified Hartung-Knapp standard error used the larger of one and the residual scaling factor, with t-based 95% confidence intervals (CIs) on k−1 degrees of freedom [30]. Estimates were back-transformed to probabilities. The summary represents an average on the logit scale, not the crude fraction of all pooled participants and not a

population-weighted global prevalence. No primary outcome contained a zero or all-event cell; no continuity correction was needed.

Study-level CIs used the Wilson score method [31]. Heterogeneity was described with Cochran's Q statistic, attributed to Cochran [32], together with I^2 and logit-scale τ^2. Approximate prediction intervals used t quantiles on k−2 degrees of freedom and incorporated between-study variance plus uncertainty in the summary. With only three to five studies, prediction intervals are especially unstable. Sensitivity analyses used DerSimonian-Laird variance estimation with the same modified Hartung-Knapp interval and leave-one-study-out calculations. Excluding the diabetes-free integrated care cohort was a specified sensitivity within the revised analysis, after study identification. Cluster-adjusted variances were unavailable; binomial variances do not account for patients nested within services. Funnel plots, asymmetry tests, meta-regression, and a comparative intervention meta-analysis were not undertaken because the relevant evidence was too sparse or incompatible. Python, NumPy and SciPy were used.

Results

Study Selection and Characteristics

The search identified 241 records. Removing two preprint duplicates left 239 records for title and abstract screening; 204 were excluded, and 35 full reports were sought. In total, 20 were retrieved. Six of the seven supplied reports were already represented in the search; the remaining inpatient report added one full-text assessment. Of 21 full reports assessed, five were excluded, leaving 16 reports representing 15 study programmes (Figure *1*, Table *4*). Overall, 15 reports remained unavailable for complete full-text assessment after the original retrieval attempts [33-47]. The five studies contributing to quantitative synthesis comprised 18,942 participants in the glycaemic analysis; denominators were smaller for other outcomes. Participant totals across all narrative studies were not summed because repeated cohorts and potential database overlap could not be excluded.

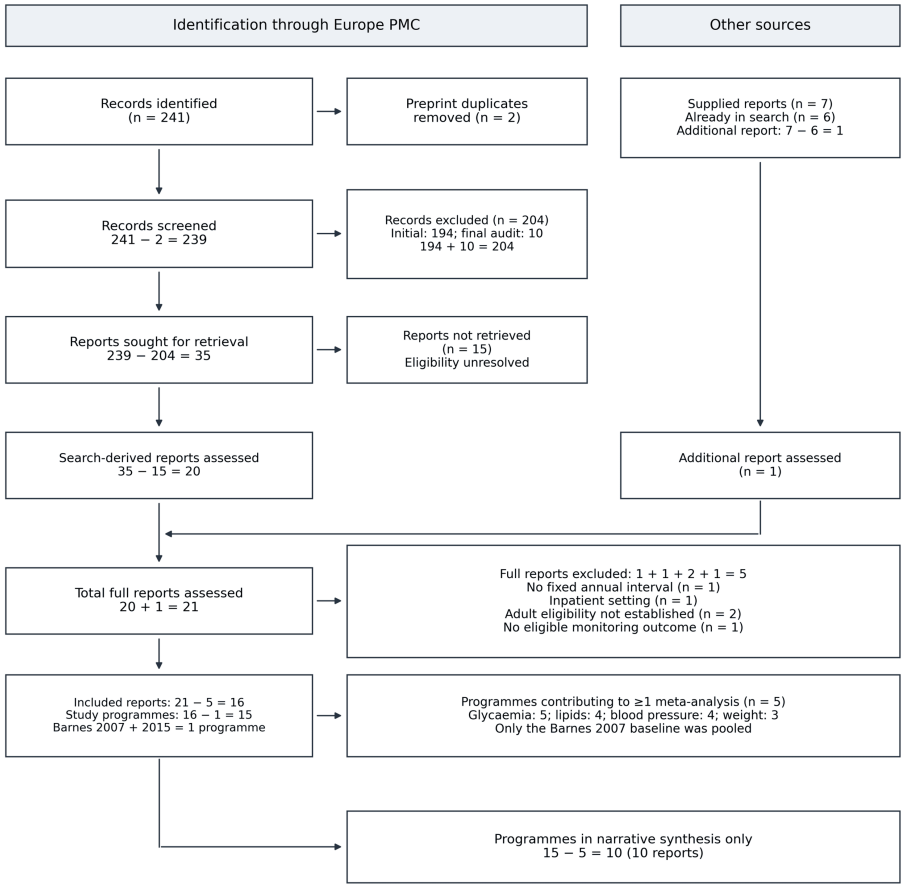


FIGURE 1: Study identification and selection.

The PRISMA 2020 flow diagram is adapted to the completed restricted search [9]. The counts reconcile as follows: 241 − 2 = 239 records screened; 239 − 204 = 35 reports sought; 35 − 15 = 20 search-derived reports assessed; 20 + 1 additional supplied report = 21 reports assessed; and 21 − 5 = 16 included reports. The five full-report exclusions comprise one report without a fixed annual interval [13], one inpatient report [16], two reports without established adult eligibility [15,48], and one without an eligible monitoring outcome[49]. The 16 included reports represent 15 study programmes because Barnes et al. 2007 and 2015 describe one programme [14,17]. Five programmes contributed to at least one meta-analysis [3,12,14,19,20], and 10 contributed to narrative synthesis only [4,5,21,23-29]. The Barnes et al. (2015) follow-up report did not contribute an additional pooled cohort [17]. Fifteen reports remained unavailable and unresolved [33-47].

PRISMA: Preferred Reporting Items for Systematic Reviews and Meta-Analyses

Report	Reason for exclusion
Mackin et al. (2007) [13]	No fixed annual interval: mean follow-up of 599 days, with variable follow-up and some antipsychotic discontinuation. Research measurements not treated as routine annual care
Michael et al. (2020) [16]	Inpatient-only setting, outside amended scope; whole-ward outcomes also lack an antipsychotic-specific denominator
Soda et al. (2021) [15]	Adult-only or ≥80% adult eligibility could not be verified from reported age information. Repeated monthly visiting denominators also unsuitable for the primary pool
Hardoon et al. (2022) [49]	Antipsychotic prescribing patterns in personality disorder; no eligible annual metabolic monitoring or abnormality-linked response outcome
Hall and Shah (2016) [48]	Monitoring in a community learning disability service, but a separable adult sample or ≥80% adult proportion was not established in the full report

TABLE 4: Full reports excluded after eligibility assessment.

Included evidence comprised community audits, primary-care record studies, insurance or integrated-system cohorts and quality-improvement evaluations [3-5,12,14,17,19-21,23-29]. Most studies were observational. Definitions varied from any result during a year to a specific yearly checkpoint, multiple required tests, or completed testing both before and after treatment. The UK programme reported by Barnes et al. in 2007 and 2015 was counted as one study programme; only its original baseline contributed to pooling [14,17].

Full Reports Not Retrieved

In total, 15 reports [33-47] were sought during the review workflow but were not available for complete full-text eligibility assessment and extraction (Table *5*). They therefore remain unresolved rather than being classified as scientifically ineligible. A final title/abstract audit did not support further reclassification of the remaining records because several abstracts explicitly described annual, past-year, or yearly frequency monitoring outcomes. Titles and numbered references are retained to permit subsequent retrieval without silently discarding these records.

Report	Title
Guo et al. (2026) [33]	Metabolic monitoring and control among patients with type 2 diabetes prescribed second generation antipsychotics
Hackett et al. (2020) [34]	Improving and standardizing metabolic screening for people prescribed antipsychotic medication who are at risk of developing metabolic syndrome within the community mental health setting
Hibner et al. (2020) [35]	Metabolic monitoring of second-generation antipsychotics: Evaluation of a pharmacist- and nurse-driven protocol
Poojari et al. (2020) [36]	Identification of risk factors and metabolic monitoring practices in patients on antipsychotic drugs in South India
Morrato et al. (2016) [37]	Metabolic testing for adults in a state Medicaid program receiving antipsychotics: remaining barriers to achieving population health prevention goals
Pereira et al. (2019) [38]	Monitoring of metabolic adverse effects associated with atypical antipsychotics use in an outpatient psychiatric clinic
Cotes et al. (2015) [39]	Antipsychotic cardiometabolic side effect monitoring in a state community mental health system
Wiechers et al. (2012) [40]	Impact of a metabolic screening bundle on rates of screening for metabolic syndrome in a psychiatry resident outpatient clinic
Hor et al. (2016) [41]	Improving metabolic monitoring in patients maintained on antipsychotics in Penang, Malaysia
Uzal et al. (2018) [42]	Monitoring of recommended metabolic laboratory parameters among Medicaid recipients on second-generation antipsychotics in federally qualified health centers
Ruiz et al. (2016) [43]	Antipsychotic use and metabolic monitoring in individuals with developmental disabilities served in a Medicaid medical home
Gallagher et al. (2013) [44]	A health screening and promotion clinic to improve metabolic monitoring for patients prescribed antipsychotic medication
Teeluckdharry et al. (2013) [45]	Monitoring metabolic side effects of atypical antipsychotics in people with an intellectual disability
Dhamane et al. (2013) [46]	Metabolic monitoring of patients prescribed second-generation antipsychotics
Moeller et al. (2011) [47]	Quality of monitoring for metabolic effects associated with second generation antipsychotics in patients with schizophrenia on public insurance

TABLE 5: Reports not retrieved for full-text assessment.

Records Reclassified at the Final Title/Abstract Verification

During the final screening audit on 9 September 2026, 10 records initially carried forward for full-report retrieval were reclassified using the amended eligibility criteria and information available in their titles and abstracts [18,50-58] (Table *6*). These decisions did not classify a report as ineligible solely because its full text was inaccessible. Nine records lacked the required annual or annual-equivalent completed monitoring outcome [50-58]; the remaining record was the Barnes et al. (2008) companion report [18], linked to the already included Barnes et al. programme [14,17]. Accordingly, title/abstract exclusions increased from 194 to 204, reports sought for retrieval decreased from 45 to 35, and reports not retrieved decreased from 25 to 15.

Report	Reason for reclassification
Singhal et al. (2026) [50]	Any glucose/HbA1c/lipid monitoring was assessed across a multiyear 2019–2024 record, not as annual or annual-equivalent patient coverage
Bomboy et al. (2021) [51]	Primary outcome was metabolic laboratory ordering 8 weeks after implementation; short-interval orders were not completed annual measurements
Cohen et al. (2020) [52]	Primary outcomes were laboratory order and alert response rates; no fixed annual completed measurement outcome was reported
Severi et al. (2018) [53]	Cross-sectional cardiovascular risk assessment without a defined annual or yearly monitoring interval
Lai et al. (2015) [54]	Visit-level laboratory adherence used a 6-month interval, not annual patient-level coverage
Mittal et al. (2014) [55]	Monitoring was assessed at baseline and 3 months after a new antipsychotic prescription; no yearly endpoint
Barnes et al. (2008) [18]	Companion report of the Barnes programme; same baseline cohort and one-year re-audit were linked to the included Barnes programme rather than treated as an independent unresolved report
Mittal et al. (2013) [56]	Monitoring was assessed at baseline and 60–120 days after a new antipsychotic prescription; no yearly endpoint
Millar et al. (2010) [57]	Descriptive health screening clinic report without an identifiable annual antipsychotic-exposed monitoring denominator
Jennex et al. (2008) [58]	Cross-sectional chart documentation and disease monitoring among long-term users without a fixed annual/yearly coverage interval

TABLE 6: Records reclassified during final title and abstract verification.

Title/abstract exclusions (n = 204) encompassed reviews and protocols, prescribing or metabolic disease studies without routine annual monitoring outcomes, ineligible populations or settings, initiation or short-interval outcomes without a yearly endpoint, order-only outcomes, and linked companion reports without an independent study contribution. The 10 reclassified records and their reasons are listed in Table *6* [18,50-58]. This re-screening changed the PRISMA screening counts but did not alter the 21 full reports assessed, comprising 16 included reports [3-5,12,14,17,19-21,23-29] and five excluded reports [13,15,16,48,49], or any quantitative meta-analysis based on the five pooled studies [3,12,14,19,20] (Table *7*).

Study	Setting and denominator	Outcome and synthesis
Barnes et al. (2007) and Barnes et al. (2015) [14,17]	UK assertive outreach; baseline 1,966; repeated national audits	Previous-year recorded results; earliest baseline pooled. Follow-up programme linked to the same study
Mangurian et al. (2018) [3]	US integrated system; 16,754 adults without known diabetes	Completed glucose or HbA1c during 2014; glycaemic pool only
Keenan et al. (2020) [19]	New Zealand primary care; 117 adults in eight practices	Recorded measurements in April 2016–March 2017; four domains pooled
Kelly et al. (2022) [20]	Ireland chronic-psychosis LAI clinic; 41 adults	Annual record baseline and re-audit; chronic baseline pooled. First-episode subgroup excluded
Al-Saati et al. (2025) [12]	Canada outpatient psychosis; same 64 records in two years	2020 baseline pooled; 2021 described separately. Form-based record exclusions.
Kaoser et al. (2026) [23]	British Columbia population cohorts; pre-pandemic maintenance subgroup 66,138	Marker-specific annual counts; no glycaemic test union. Narrative; excludes clozapine and long-term care
Ali et al. (2023) two practices [4]	UK general practices; 497 adults	Mean yearly frequency over a multiyear period. Record availability counts are not annual coverage
Ali et al. (2023) IMRD [5]	UK primary-care database; 11,435 adults starting antipsychotics	First-year window 85–365 days; inconsistent aggregate entries. Narrative only
Al-Awad et al. (2024) [24]	Saudi outpatient clinics; 350 adults	Yearly checkpoint composite complete in 6/350; not any-test yearly coverage. Narrative only
Schneiderhan et al. (2023) [25]	US community mental health and family medicine; 175 adults	Two 15-month observation periods with frequency/gap criteria. Narrative only
Breslau et al. (2018) [26]	US Medicaid community clinics; antipsychotic subgroup 2,994 versus 4,692 in wave 1; 793 versus 3,921 in wave 2	Repeated annual person-years and adjusted programme comparisons. No independent patient count pool
Mohammed and Al Naeem (2022) [21]	UAE psychiatry clinics; 123 adults selected from 4,123 records	Yearly BMI follow-up; extensive exclusions and inconsistent completion counts. Narrative only
Vivacqua et al. (2021) [27]	Argentina health-system records; 3,702 adults	Completion at baseline and 12 months jointly; different estimand. Narrative only
Nicol 2016 [28]	US Medicaid; 5,202 adults, with a separate youth sample	Testing 31–365 days after initiation; adult data only. Narrative only
Morrato 2009 [29]	US commercial plans; annual persistent-user subgroup 3,140	Testing 31–365 days after initiation; yearly aggregate rates rounded. Narrative only

TABLE 7: Characteristics of included study programmes and quantitative eligibility.

The journal issue year is used in study labels: Kelly et al. (2022) was published online in March 2021 [20], and Nicol et al. (2016) was published online in September 2015 [28]. Sample sizes are study- or subgroup-specific and must not be summed across rows.

LAI: long-acting injectable; IMRD: IQVIA Medical Research Data; BMI: body mass index

Annual Coverage

The exact counts are shown in Table *8*. Across the primary pools, recorded glycaemic monitoring ranged from 17.1% to 70.1%, lipid monitoring from 17.1% to 65.8%, blood pressure recording from 4.7% to 86.3%, and weight recording from 9.4% to 81.2%. The meta-analytic summaries and prediction intervals appear in Table *9* and Figures *2*-*5*. I^2 exceeded 96% for every outcome. The very broad intervals, especially for blood pressure and weight, show that a single summary does not describe a consistent level of performance. Given the small number of contributing studies and substantial differences in clinical settings, populations, and monitoring definitions, individual-study findings are prioritised for clinical interpretation. The pooled estimates are retained only as exploratory statistical summaries and should not be considered representative monitoring rates or clinical benchmarks. Blood pressure and weight monitoring results require particular

caution because of their exceptionally wide confidence and prediction intervals. For these outcomes, the variation between individual studies is more clinically informative than the pooled estimates.

Study	Glycaemia	Lipids	Blood pressure	Weight
Barnes et al. (2007) [14]	558/1,966	428/1,966	517/1,966	Not pooled
Mangurian et al. (2018) [3]	9,247/16,754	Not reported	Not reported	Not reported
Keenan et al. (2020) [19]	82/117	77/117	101/117	95/117
Kelly et al. (2022) [20]	7/41	7/41	8/41	7/41
Al-Saati et al. (2025) [12]	28/64	21/64	3/64	6/64

TABLE 8: Exact annual monitoring counts included in the primary meta-analyses.

Cells contain monitored patients/eligible patients. Glycaemia means glucose or HbA1c in Barnes et al. [14], Mangurian et al. [3], and Al-Saati et al. [12]; HbA1c in Keenan et al. [19]; and fasting glucose in Kelly et al. [20]. Lipids are study-defined lipid monitoring; Al-Saati et al. explicitly count any fasting lipid test [12]. Barnes et al.'s 341/1,966 obesity measure accepted body mass index or another obesity measure and was not entered as weight alone [14]. Exact counts take priority over rounded or inconsistent printed percentages; Keenan et al.'s blood pressure and weight counts are independently supported by the sums of its sex strata [19].

Outcome	k	N	Percent and 95% CI	I^2 %	τ^2	95% prediction interval %
Glycaemic monitoring	5	18,942	42.4 (18.8–70.1)	99.2	0.787	3.2–94.3
Lipid monitoring	4	2,188	33.0 (9.2–70.6)	96.8	0.923	0.5–98.1
Blood pressure	4	2,188	29.6 (1.7–91.2)	97.6	3.889	<0.1 to >99.9
Weight	3	222	31.6 (0.3–98.5)	97.5	3.880	<0.1 to >99.9

TABLE 9: Exploratory random-effects summaries of annual recorded monitoring.

Data sources are Mangurian et al., Barnes et al., Keenan et al., Kelly et al., and Al-Saati et al. for glycaemic monitoring [3,12,14,19,20]; Barnes et al., Keenan et al., Kelly et al., and Al-Saati et al. for lipids and blood pressure [12,14,19,20]; and Keenan et al., Kelly et al., and Al-Saati et al. for weight [12,19,20]. Estimates use REML with modified Hartung–Knapp t-based CIs [30]. Prediction intervals were calculated on the logit scale using t quantiles with k−2 degrees of freedom and then back-transformed to percentages. The notation "<0.1 to >99.9" means that the calculated lower limit is below 0.1% and the upper limit exceeds 99.9%; it does not indicate a negative percentage or a value above 100%. These extremely wide intervals approach the full 0–100% range and reflect substantial heterogeneity and very few studies. They are approximate and particularly unstable. Outcomes overlap within studies; the values in the N column must not be added.

k: number of studies; N: total eligible participants within each outcome; CI: confidence interval; I^2: percentage of observed variation attributed to between-study heterogeneity; τ^2: estimated between-study variance on the logit scale; REML: restricted maximum likelihood

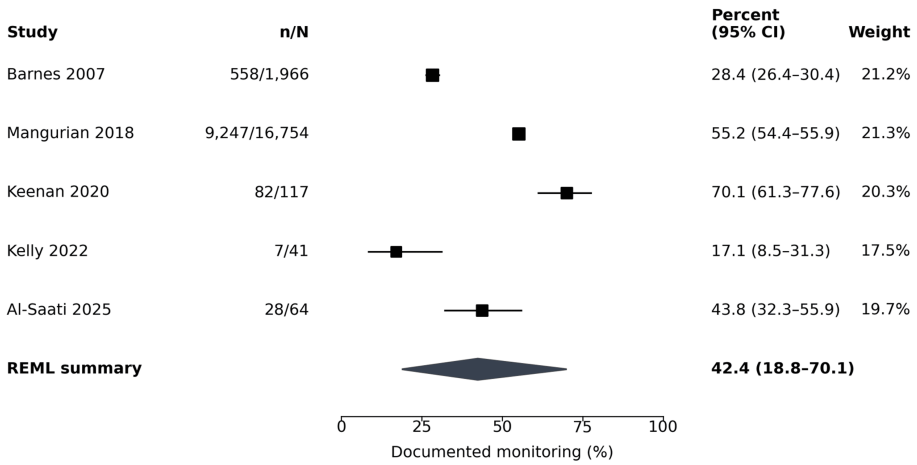


FIGURE 2: Annual glycaemic monitoring.

Data sources: Five studies contributed to this analysis. Mangurian et al. [3], Barnes et al. [14], Keenan et al. [19], Kelly et al. [20], and Al-Saati et al. [12]. Squares show study proportions and are scaled by random-effects weight; horizontal lines show 95% CIs calculated using the Wilson score method [31]. The diamond shows the REML logit summary with a modified Hartung–Knapp 95% CI. Exact definitions are in Table 8. Extreme heterogeneity limits interpretation as a common service rate.

n: patients with recorded monitoring; N: eligible patients; CI: confidence interval; REML: restricted maximum likelihood

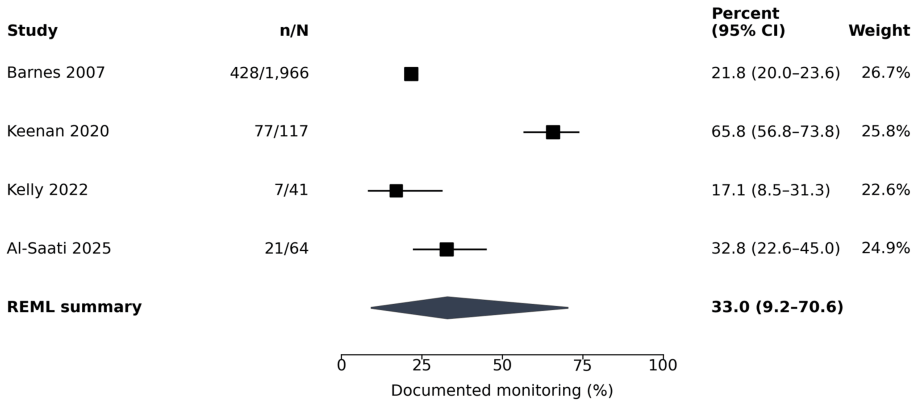


FIGURE 3: Annual lipid monitoring.

Data sources: Four studies contributed to this analysis. Barnes et al. [14], Keenan et al. [19], Kelly et al. [20], and Al-Saati et al. [12]. Squares show study proportions and are scaled by random-effects weight; horizontal lines show 95% CIs calculated using the Wilson score method [31]. The diamond shows the REML logit summary with a modified Hartung–Knapp 95% CI. Exact definitions are in Table 8. Extreme heterogeneity limits interpretation as a common service rate.

n: patients with recorded monitoring; N: eligible patients; CI: confidence interval; REML: restricted maximum likelihood

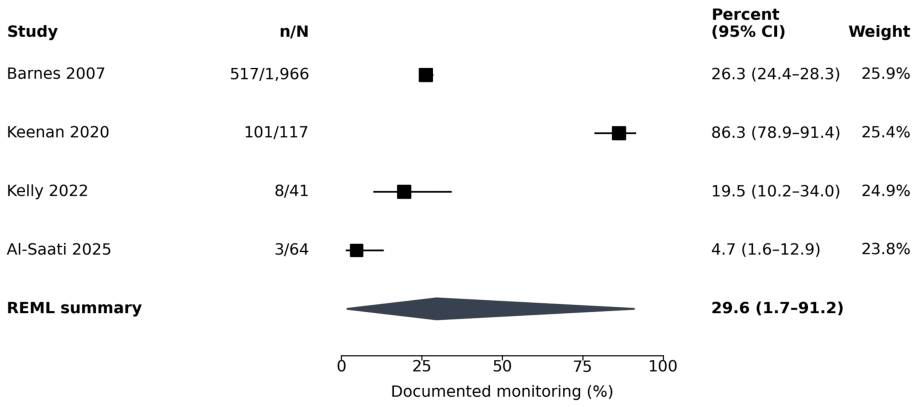


FIGURE 4: Annual blood pressure recording.

Data sources: Four studies contributed to this analysis. Barnes et al. [14], Keenan et al. [19], Kelly et al. [20], and Al-Saati et al. [12]. Squares show study proportions and are scaled by random-effects weight; horizontal lines show 95% CIs calculated using the Wilson score method [31]. The diamond shows the REML logit summary with a modified Hartung–Knapp 95% CI. Exact definitions are in Table 8. Extreme heterogeneity limits interpretation as a common service rate.

n: patients with recorded monitoring; N: eligible patients; CI: confidence interval; REML: restricted maximum likelihood

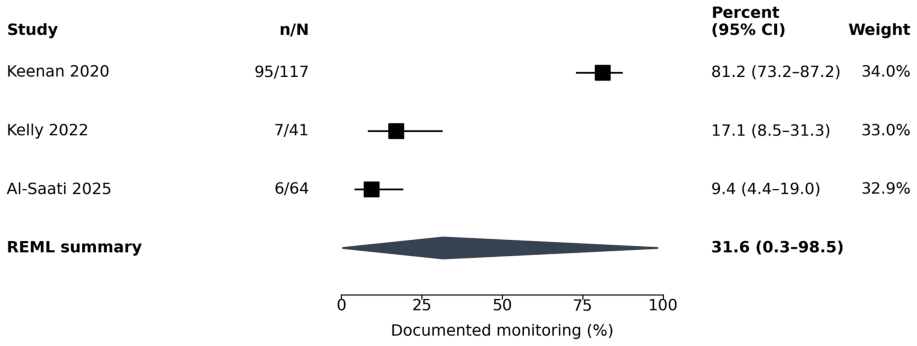


FIGURE 5: Annual weight recording.

Data sources: Three studies contributed to this analysis. Keenan et al. [19], Kelly et al. [20] and Al-Saati et al. [12]. Squares show study proportions and are scaled by random-effects weight; horizontal lines show 95% CIs calculated using the Wilson score method [31]. The diamond shows the REML logit summary with a modified Hartung–Knapp 95% CI. Exact definitions are in Table 8. Extreme heterogeneity limits interpretation as a common service rate.

n: patients with recorded monitoring; N: eligible patients; CI: confidence interval; REML: restricted maximum likelihood

Sensitivity Results

The large integrated care cohort contributed only to the glycaemic pool and excluded people with known diabetes [3]. Removing it reduced the summary from 42.4% to 38.9%. Across leave-one-out analyses, summary estimates ranged from 35.8% to 49.0% for glycaemia, 23.7% to 38.8% for lipids, and 15.4% to 45.1% for blood pressure. Weight sensitivity analyses contained only two studies and were unstable. DerSimonian-Laird point estimates were similar to REML estimates, which does not resolve clinical heterogeneity or limitations in study selection (Table *10*).

Outcome	DerSimonian–Laird percent and 95% CI	Leave-one-out point estimate range %
Glycaemic monitoring [3,12,14,19,20]	42.6 (19.0–70.1)	35.8–49.0
Lipid monitoring [12,14,19,20]	32.9 (8.1–73.1)	23.7–38.8
Blood pressure [12,14,19,20]	29.9 (1.7–91.3)	15.4–45.1
Weight [12,19,20]	31.5 (0.2–99.2)	12.8–49.0

TABLE 10: Sensitivity analyses of annual monitoring estimates.

All intervals retain modified Hartung–Knapp scaling. Weight leave-one-out calculations have only two studies and are numerical sensitivity checks, not a separate eligible primary meta-analysis. Excluding Mangurian gives glycaemic monitoring 38.9% (95% CI = 11.0–76.6).

CI: confidence interval

Only two primary studies reported directly comparable BMI counts, and obesity composites were not treated as weight alone [12,14,20]. Waist circumference was recorded in 3/117 patients in Keenan et al., 0/41 in Kelly et al., and 0/64 at baseline in Al-Saati et al. [12,19,20]; these sparse data were not pooled. A study's definition of complete monitoring materially changed its interpretation. Consequently, no pooled complete monitoring estimate was calculated.

Evidence Outside the Primary Pools

The British Columbia study reported separate annual laboratory markers in a large maintenance cohort, including total cholesterol in 25,261/66,138 and HbA1c in 27,534/66,138 before the pandemic [23]. These marker-specific counts were not assumed to equal the union of all lipid or glycaemic tests. The two UK primary care reports described annual frequencies or a broad first-year initiation window; several denominators or printed summaries could not be reconciled sufficiently for the primary pools [4,5]. Other studies used annual checkpoints, baseline plus 12-month completion, frequency targets, or first-year monitoring, which remained narrative outcomes (Table *7*) [21,24-29].

Implementation and Clinical Follow-Through

Annual monitoring improved over successive audits in the UK programme, but voluntary participation and changing cohorts limit causal attribution [17]. In the 41-patient chronic psychosis subgroup in Kelly et al., blood pressure documentation increased from 8 to 17 and weight documentation from 7 to 14 [20]. In Al-Saati et al., glucose monitoring decreased from 28/64 to 24/64 while any fasting lipid monitoring increased from 21/64 to 24/64; referrals increased from 13/64 to 18/64 [12]. These paired marginal counts were not analysed as independent observations. The primary care integration evaluation found improved laboratory monitoring in one implementation wave but no clear improvement in the second [26]. Taken together, these reports do not identify a single effective component or establish a general intervention effect.

No compatible dataset supported a pooled rate of timely action after a newly abnormal result. Barnes et al. documented pharmacotherapy in 75/121 patients with known diabetes, 56/116 with known hypertension, and 44/120 with known dyslipidaemia, calculated from the reported counts without recorded medication [14]. In Kelly et al., two patients had documented diabetes, and none had recorded diabetes medication; four had hypertension, and one had antihypertensive medication [20]. These are cross-sectional treatment documentation findings, not incident abnormality response rates. Mohammed and Al Naeem reported management for nine identified metabolic syndrome cases, but extreme cohort selection and absence of a standard response interval limit interpretation [21]. Condition groups overlap and cannot be added to count unique patients receiving action. Table *11* shows why these outcomes do not estimate timely action after a newly confirmed abnormality.

Study	Verified related outcome	Why it does not estimate timely action after a new abnormality
Barnes et al. (2007) [14]	Diabetes medication 75/121; hypertension medication 56/116; lipid medication 44/120	Established diagnoses; no incident abnormality date or common response interval. Conditions overlap
Kelly et al. (2022) [20]	Diabetes treatment 0/2; hypertension treatment 1/4; GP notification described	Cross-sectional treatment documentation. Timing and appropriateness of action cannot be established
Al-Saati et al. (2025) [12]	Primary-care referral 13/64 before and 18/64 after implementation	Denominator includes all sampled patients, not only those with an abnormal finding
Mohammed and Al Naeem (2022) [21]	Management reported for nine identified metabolic-syndrome cases	Highly selected cohort, inconsistent monitoring completion and no standard interval to action

TABLE 11: Clinical action outcomes and denominator limitations.

No patient-level “any response” numerator was created by adding medication, referral, or lifestyle categories. Absence of recorded medication does not establish inappropriate treatment, because the indication, severity, previous therapy, and non-pharmacological care may differ.

Appraisal Findings

The primary studies described their settings and monitoring outcomes, but several used convenience or voluntary service samples, small cohorts, or incomplete capture of care delivered elsewhere. Al-Saati et al. excluded records without an initiated monitoring form, creating a particular selection concern [12]. Mangurian et al. had a well-defined integrated system denominator and assessed external testing, but its diabetes-free insured population differs from general community caseloads [3]. Other narrative studies had important restrictions, inconsistent counts, or measures combining more than one time point [4,5,21,25-29]. These limitations affect interpretation independently of the statistical heterogeneity (Table *3*).

Reasons Informing Interpretation

Barnes et al.: Services volunteered, and some teams sampled records; care outside psychiatric notes and service clustering were incompletely addressed [14,17]. Mangurian et al.: a defined integrated system cohort, explicit completed test ascertainment, and checks for external tests support internal validity; excluding diabetes, restricting age to 18-64 and insured membership limit applicability beyond the defined population [3]. Keenan et al.: convenience practice selection, modest sample size, uncertain external capture, and mismatched printed percentages; exact stratified counts support extraction [19]. Kelly et al.: a complete small clinic subgroup with consistent count denominators, but no independent extraction reliability and inadequate precision [20]. Al-Saati et al.: exclusion of patients without a form can omit those least likely to be monitored; the full monitoring composite is inconsistent with individual domains, although the pooled individual counts are coherent [12].

Kaoser et al.: Population data and defined exposure windows strengthen ascertainment within the eligible population; exclusions and marker-specific outcomes limit transfer to general community caseloads [23]. Ali et al. two practices: selected sites and a frequency-based estimand [4]. Ali et al. IMRD: a large defined database cohort, but discordant aggregate entries constrain quantitative reuse [5]. Al-Awad et al.: random sampling from outpatient records, with a small sample under the stated precision criterion and yearly checkpoint definitions [24]. Schneiderhan et al.: selected comanaged patients, non-blinded implementation and frequency criteria; the analysis recognises paired observations [25]. Breslau et al.: selected grant-funded clinics and residual confounding, despite claims-based outcomes, propensity weighting, and repeated-measure methods [26].

Mohammed and Al Naeem: 4,000/4,123 potentially eligible records were excluded, including patients with baseline metabolic problems or other medication exposure; missing follow-up and inconsistent BMI completion severely restrict interpretation [21]. Vivacqua et al.: defined health system population, but a joint baseline and follow-up outcome and inconsistent anthropometric summary [27]. Nicol et al.: explicit adult claims cohort and first-year outcome; results do not represent continuous maintenance coverage or uninsured patients [28]. Morrato et al.: defined commercial claims and first-year persistent exposure; inconsistencies in reported cohort totals and rounded annual summaries limit reuse of aggregate counts [29].

Implementation Comparisons

Uncontrolled before-after studies (Barnes et al., Kelly et al., Al-Saati et al., and Schneiderhan et al.) cannot

separate intervention effects from secular changes, documentation changes, or selection [12,14,17,20,25]. The UK programme changed participating teams and caseloads over time [17]. Kelly et al. and Al-Saati et al. reobserved the same chronic care patients; paired transition tables were not available for this reanalysis [12,20]. The Al-Saati et al. comparison also coincided with pandemic-era service disruption [12]. Breslau et al. used controls and adjusted difference-in-differences models, but non-random allocation, clinic selection, and possible time-varying confounding remain [26]. These observations are narrative methodological assessments, not completed ROBINS-I assessments. No high-certainty causal conclusion or formal GRADE rating is assigned.

Discussion

This review found incomplete and highly variable documentation of annual metabolic monitoring. The extreme between-study variation is a central finding. The pooled results should not be used as contemporary national rates, acceptable targets, or predictions for an individual service. Their CIs reflect substantial uncertainty, and the prediction intervals demonstrate how little the small evidence set constrains a future service's performance.

Variation has several plausible sources. Studies span different healthcare systems, years, indications, and care pathways. Integrated laboratory data may capture tests that a mental health case note audit misses. Conversely, restriction to selected clinic attendees or completed monitoring forms can exclude people with the greatest barriers to care. Known diabetes may increase testing, while a diabetes-free cohort answers a screening question. Assay choice and whether a report counts any test, a full panel, or a required frequency further alter the estimand. Random-effects modelling cannot make these definitions identical or remove selection bias.

The findings extend the concern identified in earlier monitoring reviews [6-8] while separating completed annual recording from other measures of guideline adherence. A monitoring form, education, or audit feedback may support care, but the available implementation evidence is not sufficient to attribute improvement to one component. Apparent improvements in repeated audits can also reflect changing patient mix, better transfer of existing results, wider service participation, or secular changes. Controlled designs and explicit implementation descriptions are needed to distinguish these mechanisms.

A second evidence gap concerns clinical response. Failure to report action is not equivalent to failure to act. Treatment may already be in place; lifestyle management may be appropriate; an abnormal value may need confirmation; and a prescribing change may require balancing metabolic and psychiatric outcomes. A defensible response measure therefore requires the abnormality threshold, a patient-level denominator, the action expected, and its timing. Existing diagnosis medication counts and referrals among an entire clinic population do not supply this information.

Clinical Implications for Community Psychiatric Care

Annual metabolic assessment should represent a minimum maintenance standard and should not replace more frequent monitoring during antipsychotic initiation, following medication changes, or when indicated by individual clinical risk. Clear responsibilities should be established between psychiatric and primary care services for arranging investigations, confirming test completion, reviewing abnormal results, and ensuring appropriate management and follow-up. For service evaluation, monitoring should be assessed as successive stages: eligible patients, completed tests, abnormal findings, documented clinical review, appropriate action, and reassessment. Recording the responsible clinician and agreed follow-up interval may help identify gaps in communication and continuity of care. These are practical service improvement considerations rather than evidence that a particular intervention reduces cardiovascular events. Future studies should evaluate both monitoring adherence and timely clinical responses to newly identified metabolic abnormalities.

Limitations

The most important limitations arise before meta-analysis. This review used a single restricted search engine and an annual/community care term block, which may miss studies that do not describe these features in searchable fields. Fifteen reports remained unavailable for complete full-text assessment after the original retrieval attempts [33-47]. Several abstracts described potentially eligible annual or past-year monitoring outcomes, so omission of these reports may materially affect the synthesis; the direction of any resulting bias is uncertain. No prospective registration, librarian search review, systematic registry search, or author contact was completed. Screening, data extraction, and appraisal were undertaken by the first author with co-author supervisory review rather than by two independent reviewers. These limitations prevent a claim of comprehensive ascertainment or full implementation of the original protocol.

The scope and analysis plan were amended after examination of the supplied evidence, creating a risk of outcome-driven selection. The explicit eligibility rules, unresolved report list, and exact-count dataset make those decisions visible, but do not eliminate that risk. The pools contain few studies, frequently from

selected services, and span historical periods. Unadjusted binomial variances ignore within-service clustering. Differences in outcome definitions, missing external records, and the exclusion of known diabetes in one large cohort further restrict comparability. The primary analysis does not address full guideline adherence, newly initiated treatment, inpatient practice, or treatment effectiveness.

Finally, formal comparative risk-of-bias and certainty assessments were not completed. The descriptive checklist is not a substitute for ROBINS-I or GRADE. The absence of a pooled clinical response estimate represents insufficient compatible reporting in the retrievable evidence, not evidence that clinical action is absent across practice.

Conclusions

This review found incomplete and variable documentation of annual metabolic monitoring among adults prescribed antipsychotics in community care. The exploratory pooled estimates should be interpreted cautiously because of substantial heterogeneity, differences in monitoring definitions, and limitations in study identification and retrieval. They cannot provide a dependable benchmark for an individual service. The available evidence was also insufficient to determine how consistently newly abnormal results led to timely clinical action. The key message is that documented testing captures only one part of metabolic care. A recorded measurement does not establish that an abnormal result was reviewed or managed, while an absent record does not necessarily indicate that care was omitted. These findings highlight the importance of clear communication and shared clinical responsibilities between psychiatric and primary care services. Effective metabolic care requires not only routine monitoring but also timely review of abnormal findings, appropriate management, and documented follow-up. Annual assessment should represent a minimum maintenance standard, with more frequent monitoring according to individual clinical needs. Although the available evidence does not establish the superiority of any particular service model, strengthening coordination across the complete monitoring pathway represents an important priority for improving the quality of care.

Additional Information

Author Contributions

All authors have reviewed the final version to be published and agreed to be accountable for all aspects of the work.

Concept and design: Mahnoor Fatima Shah, Ian French

Acquisition, analysis, or interpretation of data: Mahnoor Fatima Shah, Ian French

Drafting of the manuscript: Mahnoor Fatima Shah

Supervision: Mahnoor Fatima Shah, Ian French

Critical review of the manuscript for important intellectual content: Ian French

Disclosures

Conflicts of interest: In compliance with the ICMJE uniform disclosure form, all authors declare the following: **Payment/services info:** All authors have declared that no financial support was received from any organization for the submitted work. **Financial relationships:** All authors have declared that they have no financial relationships at present or within the previous three years with any organizations that might have an interest in the submitted work. **Other relationships:** All authors have declared that there are no other relationships or activities that could appear to have influenced the submitted work.

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