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Separating First-Attempt Success from Recovery at Corridor Fast Chargers

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Abstract

A visit that reaches charging after an initial failure tells a different story from an immediately successful visit. Across 48,378 recorded California corridor visits in two published hardware cohorts, first-attempt success differed by 11.71 percentage points and eventual success by 8.92 points. A symmetric decomposition attributed 8.36 points of the final gap to the first-attempt component and 0.56 points to the conditional-recovery component. Among visits that initially failed, observed recovery was 27.54% in one cohort and 29.67% in the other. For the cohort reporting interruptions, allocating its interruption count across the two successful paths puts uninterrupted recovery between 21.25% and 29.67% of initial failures. Recording only the final outcome erases both the initial failure and uncertainty about whether later charging was sustained.

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Why a successful visit can still contain a failure

When a driver visits a corridor fast charger, a session may start on the first attempt, start after one or more setbacks, or never reach charging. A report of operational availability cannot distinguish these paths, since availability characterizes the port and the path also depends on the driver, vehicle, and service interface. A report of final success leaves out whether anyone had to try again. This information matters to researchers comparing fast-charging systems because a similar rate of completed visits could coexist with a different amount of initial friction. In infrastructure studies, Levinson and West (2018) model the potential effects of public charging supply, while Illmann and Kluge (2020) and White et al. (2022) consider how infrastructure availability relates to electric-vehicle adoption or adoption intent. Funke et al. (2019) distinguish corridor charging needs from those of users with routine access to residential or workplace charging. Those questions concern what users can expect from access. Our narrower question concerns what actually happens in the visits visible to a charging study.

Xu et al. (2026) study fast-charging safety diagnosis, layered control, and fault recovery for high-voltage battery systems in an interoperating charging network. Their framing makes an important measurement issue visible. A distinction between the initial condition, the recovery path, and the post-recovery condition must survive in the evidence if an investigator plans to compare how such layers perform. These public corridor counts identify the service outcome and its sequence at the visit level. They do not identify actionable safety states inside a vehicle or show which control action enabled a later charge. This study asks how much of an observed between-cohort gap in eventual success can be assigned arithmetically to first-attempt performance and how much to the transition that follows initial failure. It separately asks how coarse reporting of interruptions limits what investigators can infer about sustained recovery.

A service layer may involve gaining access, authorizing a session, making a successful connection, or observing energy delivery. Quinn et al. (2024) develop customer-centered performance indicators to distinguish observable stages of charging. The ChargeX Consortium (2023) also advocates consistent failure classification across equipment. These objectives differ from finding the underlying source of an individual fault. Several mechanisms may lead to the same recorded endpoint, including an ordinary payment problem, a communication problem, and the user's decision to stop trying. The fast-charging field evaluation of Rempel et al. (2024) documents a variety of usability and functional obstacles that can appear together in an aggregate result. Together the sources motivate retaining the two success stages in a

reporting model while leaving attribution to a particular layer unresolved when the observed variables cannot support it.

Public counts and the analysis population

The estimates use six published outcome cells, displayed in Figures 5 and 6 of Gamage et al. (2023) and reproduced numerically in Appendix A of Schell and Lopez (2026). The original study uses records from California corridor direct current fast chargers operated under Caltrans. The conference analysis spans June 2021 through November 2022. The two hardware cohorts remain anonymized as Brand B and Brand C, using the labels in that release. Brand B contributes 40,724 recorded visits with 27,617 first successes, 3,610 later successes, and 9,497 visits with no successful charge. Brand C contributes 7,654 recorded visits with 6,087 first successes, 465 later successes, and 1,102 visits with no successful charge. Counts are transcribed from the state report and independently cross-checked against the conference figures. The original conference source describes its recorded-energy criterion for screening attempts. We use the published visit classifications as given, rather than infer how a different energy cutoff would reassign visits.

For Brand C, the original study could link events within its system using anonymized user identifiers. Brand B did not have the same identifying field, and the authors used timing information to construct visits. A threshold motivated by one cohort may link attempts that actually belong to different users in a busier cohort, separate attempts of the same user when there is a delay, or assign results differently when drivers move between plugs. We cannot re-estimate those choices from the released occurrence counts. We fix the analysis to the EVS36 and state-report tabulations described above. Using later versions of the corridor analysis to supplement a released count would risk changing the sample or cohort label within a single comparison.

The results also cannot represent failed arrivals that never reached a logged attempt. An occupied bay, inaccessible equipment, or a physical obstruction can keep a driver outside the dataset entirely. Schell and Lopez (2026) distinguish their separate statewide field-test measures of physical access, port operation, and charge attempts. The California Air Resources Board (2022) reports the scope of the state legislation that motivated reliability reporting, and the Federal Highway Administration (2023) sets out a federal infrastructure framework addressing interoperability and reporting alongside installation. Those documents help explain why different measurements exist. None supplies missing corridor outcomes. The two cohorts here are deliberately treated as historical, observed services rather than a representative statistical sample of every port or driver covered by such programs.

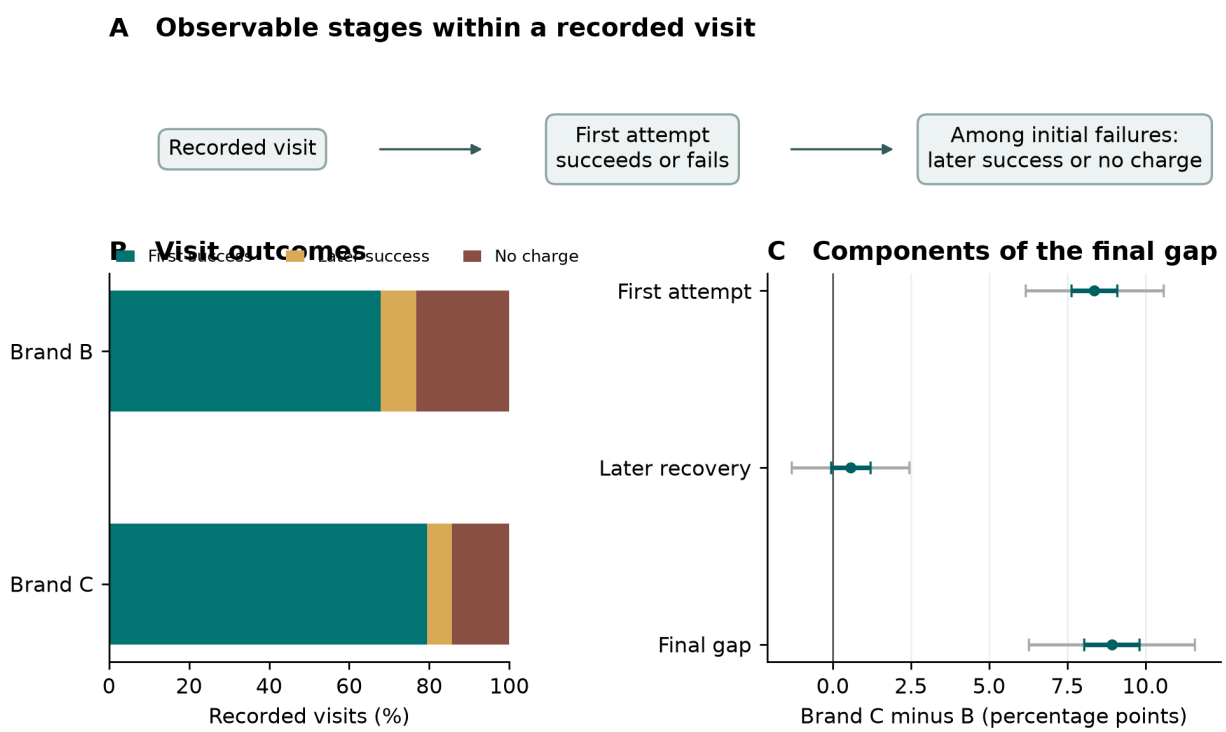


Figure 1. *Panel A shows the observation sequence, Panel B shows the three released visit outcomes, and Panel C compares the estimated first-attempt and conditional-recovery components of the between-cohort difference. Dark intervals in Panel C take visits as independent. Light intervals multiply the working variance by nine as a sensitivity case.*

A two-stage description of the difference

Define p as the number of first successes divided by all recorded visits in a cohort. Define q as the number of later successes divided by visits that did not succeed initially. Eventual success is p plus the fraction failing initially multiplied by q. This identity treats the three published terminal outcomes as mutually exclusive and exhausts the visit categories given in the source. The two q denominators explicitly include initial failures that were never followed by a recorded retry. Consequently, q is an observed resolution share among initial failures. It cannot estimate a success probability conditional on deliberately trying again, because the number of attempted retries among the unresolved visits is unavailable in the tabulation. It also cannot identify the effect of an intervention that would cause more users to stay and retry.

We use symmetric weights so that the contribution assigned to a stage does not depend on selecting one hardware cohort as the sole reference. The first-attempt component is the difference between the two p values multiplied by one minus the arithmetic mean of the two q values. The recovery component is the difference between the two q values multiplied by one minus the arithmetic mean of the two p values. The two terms add exactly to the difference in eventual success. The symmetric decomposition is an adaptation of the two-rate comparison in Kitagawa (1955) to the algebra of sequential outcomes. It expresses a contrast between the two observed cohorts. It does not estimate what would actually occur if the vehicle mix, service policies, or charger behavior of one cohort were altered to match the other.

For individual cohort proportions we compute Wilson (1927) score intervals using the observed denominators. Brown et al. (2001) evaluate the behavior of proportion intervals and provide motivation for separating that calculation from a simple symmetric Wald interval. For contrasts we apply the delta method described by Oehlert (1992) to the working binomial-branch likelihood. The likelihood treats first successes as binomial given the visit count and later successes as binomial within the observed initial

failures. Its first-order covariance between the two branch parameters is zero. These calculations give well-defined reference intervals under working independence assumptions. Because repeated visits and stations cannot be grouped in the published table, we also report variance multipliers of four and nine. Kish et al. (1995) explain why clustering can change the precision of comparisons between proportions. Our multipliers are explicit sensitivity cases rather than measured design effects, and neither corrects systematic error in how records were grouped as visits.

Results before and after an initial failure

Table 1 presents the proportion experiencing immediate success, the portion of initial failures with later success, and the eventual-success proportion. Brand B recovers 3,610 of its 13,107 initially unsuccessful visits, and Brand C recovers 465 of 1,567. Although Brand B has a slightly lower conditional recovery estimate, later successes account for a greater proportion of its entire set of visits. More visits encounter the stage in which recovery can occur. A report showing only the marginal contribution of later success would say it occurs more often in Brand B, at 8.86% of all recorded visits versus 6.08% in Brand C. A report showing recovery conditional on the initial failure would reverse that ordering. Both statements describe the same released counts, but they use different opportunities to recover.

Table 1. *Outcomes by stage in observed DC fast-charging visits.*

Cohort	All visits	Initial failures	First success % [95%]	Later success among initial failures % [95%]	Eventual success % [95%]
Brand B	40,724	13,107	67.82 [67.36, 68.27]	27.54 [26.78, 28.31]	76.68 [76.27, 77.09]
Brand C	7,654	1,567	79.53 [78.61, 80.42]	29.67 [27.46, 31.98]	85.60 [84.80, 86.37]

Table 1 note. *Wilson intervals use the numerator and denominator attached to each stage and treat visits as independent. They do not model station dependence or grouping errors.*

The first-attempt gap is 11.71 percentage points in the direction of higher success in Brand C. Later successes narrow the eventual gap to 8.92 points because the lower first-attempt cohort has more initially unsuccessful visits eligible for recovery. In Table 2, the symmetric decomposition attributes approximately 94% of the final gap to the first-attempt component. With a variance multiplier of nine the 95% reference range around the first-attempt component still runs from 6.15 to 10.57 points. The equivalent range around the recovery component runs from −1.32 to +2.45. The conditional-recovery contrast itself is 2.13 points, and even its working independent-visits interval straddles zero. These calculations support greater emphasis on the initiation difference in describing the observed output gap. They do not identify a persistent equality of recovery capacity across brands or an intrinsic superiority of their components.

Table 2. *Difference in rates and decomposition, Brand C minus Brand B, in percentage points.*

Comparison	Estimate	95% reference interval, design effect 1	95% sensitivity interval, design effect 9
First-attempt success difference	11.71	[10.70, 12.72]	[8.68, 14.75]
Recovery conditional on initial failure	2.13	[−0.26, 4.52]	[−5.03, 9.29]
First-attempt component of final gap	8.36	[7.63, 9.10]	[6.15, 10.57]
Recovery component of final gap	0.56	[−0.07, 1.19]	[−1.32, 2.45]
Eventual-success difference	8.92	[8.04, 9.81]	[6.26, 11.58]

Table 2 note. *Contrasts use the delta approximation from the two-stage likelihood. The multiplier changes the working variances of the contrast estimates, and the point estimates stay fixed.*

The observable recovery path leaves a trace even among successful visits. Brand B's later successes are 11.56% of its eventual successes. Brand C's are 7.10%. Those shares would be lost if final success alone were retained. The conference figure for Brand C also reports 132 interrupted charging sessions among visits counted on the successful branch, without cross-classifying those interruptions by first or later success. At most 132 distinct successful visits can be represented by 132 reported interruptions. Thus, even if all 132 were distinct and in the later-success group, 333 later successes would remain without a recorded interruption, giving 21.25% of initial failures. If none were in that group, the rate would remain 29.67%. This range describes what is compatible with the observed interruption margin. It does not cover interruptions the source protocol failed to record. A comparable interruption count for Brand B is unavailable from these sources, so it would be misleading to use zero for it and recompute a comparative uninterrupted-service gap.

Keeping outcome and diagnosis separate

Other approaches help explain what the visit data leave out. Energetics (2024) evaluates termination codes and energy delivered in a different sample of charging sessions. ChargerHelp (2024) describes differences between field observations and status-based figures in its own reliability reporting. J.D. Power (2024) asks drivers about their charging experiences, including problems beyond what is visible in energy delivery. Liu et al. (2023) and Yu et al. (2025) study experience and reliability using user reviews. These sources provide complementary evidence about failure visibility. A driver report has a different selection process from a recorded visit, and a recorded visit has a different selection process from an equipment-availability snapshot. Bhatt et al. (2024) emphasize the role of both access and experience, which reinforces the reason to state which population forms each denominator rather than combine all these sources into a single rate.

Usage also differs across settings in ways that could affect who continues after a failure. Hecht et al. (2022) and Bayram et al. (2023) analyze patterns of usage in Germany and public AC charging in the United Kingdom, respectively. Their results concern different populations from these California fast-charging corridors, but they show why the mix of charging visits is an empirical quantity. Globisch et al. (2019) and Potoglou et al. (2023) examine user preferences and public charging choices. LaMonaca and Ryan (2022) review the distinct participants and service arrangements in charging provision. Together they motivate interpreting the recovery denominator as a selected set of unsuccessful starts that can differ in driver time, station availability, and payment or vehicle conditions. We do not observe those dimensions among the released counts. Singh et al. (2025) connect perceived infrastructure reliability to electric-vehicle market outcomes, and Zhang et al. (2018) examine the economics of providing charging

infrastructure. This analysis describes which portion of failure is visible before that economic or behavioral link is investigated.

For the layered diagnostic problem raised by Xu et al. (2026), the most informative record would preserve initial outcome, later energy transfer, and evidence about whether subsequent charging continued. Its value comes from retaining distinctions that cannot be reconstructed from an eventual-success label alone. A later successful session might follow a corrected authorization, a connection change, a vehicle request, or a move to another usable port. The public aggregates distinguish the path without identifying which of those events occurred. Linking a visit transition to the internal mechanism of a fault would require compatible service and vehicle records that observe both the transition and the proposed mechanism. The present calculations support separate first-attempt and recovery metrics, explicit eligibility rules for each, and an additional continuity measure when the data provide it.

Data and reproduction

The three-category counts were transcribed from printed Appendix A-2 of CEC staff report CEC-600-2026-034 and checked against the open EVS36 conference figures. The Brand C interruption count comes from EVS36 Figure 5. The public sources were accessed on November 23, 2026. The companion CSV documents the extract and the calculation script produces the proportions, decomposition, working intervals, sensitivity results, and within-cohort interruption bounds. It logs a hash of the count extract and the independently downloaded conference PDF. No event-level records are distributed with the analysis.

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