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# The integration plan spends what the deal bought.

Integration depth buys cost savings by spending market position. The measured record prices that trade, and says its sign is not fixed.

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## MANAGEMENT SUMMARY

A deal model has two synergy columns and books them separately. The integration plan spends across both, and only one side gets recorded. In 232 horizontal acquisitions in German-speaking Europe, market-related performance after the deal carried a path coefficient of .67 on financial performance where cost savings carried .07; integration depth bought the second at .57 and spent the first at -.23, and the authors' own arithmetic makes the total effect negative. Two of those signs are conditional on things the integration team either decides or can read off the market, and both contrasts are tested rather than eyeballed. The evidence is association from cross-sectional surveys, which the essay states rather than buries, and it ends with the measurement a reader can run, the version of it that fires on random data, and the study that would settle it.

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The deal closed on a Friday and by Wednesday there are ninety workstreams, each with an owner and a date. Nobody in the room is against the customers. But the plan has one column for what integration saves and no column at all for what it spends, and in the measured record the thing it spends is worth more than the thing it saves. That is the finding, and the useful part is that its sign is not fixed.

What follows is an assembly, not a discovery. Every study here has been sitting in a marketing or strategy journal for two decades. What has not existed is the four of them in one place, with their conditions attached, in front of the person who owns the revenue after close, and with the numbers in the deck graded honestly against them.

## Why does post-merger integration consume both capital budgets and market position?

Open any synergy model. Cost synergies sit on one line, booked at face value because they are a subtraction you control. Revenue synergies sit on another, booked at plan because they need a customer to agree. Integration costs appear as a lump sum against the transaction. What does not appear anywhere is a price on the market position each integration decision consumes to produce the saving.

You can see why by looking at what “how deeply did you integrate” turns out to mean when somebody measures it. Homburg and Bucerius put eight items to the executives running marketing integration in 232 horizontal acquisitions between firms in Germany, Austria and Switzerland: how far were the two firms made similar on products and brand names, on new product development, on price positioning, on communication, on sales channels, partners and offices, on sales-force incentive and provision systems, on marketing and sales information systems, and on internal sales support.

Now look at how the same study measured cost savings. Nine reductions: products, services, brands, business units, sales channels, production locations, sales offices, and headcount in marketing and in sales.

These are not two independent lists. They describe the same integration workstreams from two directions: how similar two things were made, and how much of them was removed, which is precisely why one plan produces both. That is an observation about subject matter and not a claim that the two scales measure one thing; the paper reports discriminant validity between every construct pair, and I come back to the residual risk below.

## What is the empirical exchange rate between integration speed and market share?

In the same path model, on the same 232 deals: market-related performance after the acquisition: market share against the pre-deal situation, and retention of existing customers rated against competitors: carried a coefficient of .67 on post-merger financial performance. Cost savings carried .07. Both are significant. Both are completely standardised coefficients inside one model on one sample, so they are two numbers and not a ratio, and turning them into a multiplier would be the exact move this essay objects to four sections from now.

The extent of integration pushed cost savings up at .57 and market-related performance down at -.23. Homburg and Bucerius then compute the three indirect paths themselves and report the total effect of integration depth on financial performance as -.116. In their words, the positive effect through cost savings “is reversed to a negative total effect through the negative market-related consequences of integration.”

Three things about that number before anyone puts it in a deck.

It is an association. The study is a cross-sectional survey, fielded in 2002 on deals completed between 1996 and 1999, with one senior executive per deal answering on the acquirer's side. There is no experiment and no panel. The authors validated the profitability measure against objective financial data for the 43 deals where both firms were publicly traded, and got a correlation of .704; they validated customer loyalty against sixty telephone interviews with customers, and got .757 and .677 in the two industries. That is good measurement validation. It is not identification, and I will come back to what that costs.

It is a total-sample finding, and the authors say so: cost savings may still be the more important driver for some acquisitions.

And the obvious supporting evidence does not support anything. In a different sample entirely: 253 horizontal manufacturing acquisitions between 1988 and 1992: Capron and Hulland found essentially no relationship between marketing-resource redeployment and cost synergies. It would be convenient to read that as confirmation. It is not: their product-cost model does not reach significance at all,  $F = 1.72$ ,  $p = .12$ , and a model that does not reach significance licenses no conclusion in either direction. It is worth saying precisely because the convenient reading was right there.

## When does aggressive integration flip from value creator to value destroyer?

This is the part that makes the finding operable rather than gloomy, and it is the part almost nobody has been shown.

Almost, because there is one exception and it belongs here rather than in a footnote. Homburg and Bucerius published their recommendations in German in 2003: a Mannheim institute paper, still freely downloadable, aimed squarely at practitioners. If you read German, it exists and you should know it exists. What it does not contain is the conditional structure below: it predates the journal article, states the recommendations as recommendations, and reports the sample descriptively rather than reporting the coefficients or the subgroup tests. The prescription reached German readers twenty-three years ago. The evidence for it did not travel with it.

The same study split its sample on the customer orientation of the integration: three plain survey items about whether customer needs were decisive for integration activities, were the central focus of the process, and whether activities were aimed at increasing customer benefit. Where customer orientation was low, integration depth hit market-related performance at  $-.55$ . Where it was high, the coefficient was  $.10$  and not statistically distinguishable from zero.

That comparison is tested rather than eyeballed. The difference between the two subgroups is a nested chi-square difference of 4.83, significant at the five per cent level. This matters more than it sounds: comparing two coefficients by looking at which one has stars is a standard and expensive error, and it is not what happened here.

Speed behaves the same way, on something you cannot choose. Faster integration helped market performance overall, at  $.20$ . Split by market growth before the deal, the sign flips: in low-growth markets faster integration was associated with worse market performance, at  $-.13$ , and in high-growth markets with better, at  $.42$ , with a chi-square difference of 31.04. So the hundred-day instinct is not wrong everywhere. It is wrong somewhere specific, and the somewhere is the market you are actually in.

Three caveats travel with all of that, and they belong here rather than in a footnote. The same executive rated both the integration's customer orientation and the market outcome, and the paper contains no test for common-method bias, so part of the moderation could be one person telling a coherent story. A third moderator the authors predicted, the relative size of the target, did not survive its own difference test on this link. And market growth moved the depth effect in the opposite direction to their hypothesis: the damage was weaker under high growth, not stronger. Growth is not a tidy story and this is not one.

The decision this changes is not "deep or shallow, fast or slow." It is: on what basis was this sequence set, and is there anybody in the integration meeting whose job is the customer's experience of it? That is the variable the measured record says moves the sign.

**Table 1** The sign is a decision: the tested splits

| The split                                                            | Where it is low                  | Where it is high     | The difference, tested                             |
|----------------------------------------------------------------------|----------------------------------|----------------------|----------------------------------------------------|
| Customer orientation of the integration (depth → market performance) | -.55 (t = -8.42)                 | .10 (t = 1.61, n.s.) | chi-square difference 4.83, p < .05                |
| Market growth (speed → market performance)                           | -.13 (t = -3.77)                 | .42 (t = 6.69)       | chi-square difference 31.04, p < .01               |
| Relative size of the target (depth → market performance)             | the moderation was not supported |                      | chi-square difference 1.80, n.s.: licenses nothing |

Homburg & Bucerius (2005), Table 4, p. 105, completely standardized coefficients; the overall speed path (.20) is Fig. 2, p. 104. One cross-sectional survey, 232 deals, one acquirer-side executive each, no common-method test in the paper; the licence in each row is the difference test, never the stars.

## Why does the structural direction of integration govern commercial survival?

Depth and speed say how much and how fast. Neither says which way, and a merger of two commercial organisations has a direction whether or not anyone writes it down.

Capron and Hullan measured that directly, and the pattern is uncomfortable in a useful way. Moving the target's sales force into the acquirer was associated with lower market share (-.17) and lower profitability (-.21). Moving the target's brands into the acquirer was associated with lower geographic coverage (-.21) and lower market share (-.18). But moving general marketing management expertise in either direction was associated with better outcomes on both measures: market share (.33) and profitability (.23) in one direction, and market share (.18) and profitability (.20) in the other.

The resource an acquirer most wants is the one whose absorption tracks damage. The one nobody schedules a workstream for is the one that tracks gain. This is a firm-level result about structure, not a claim about individuals: the separate question of what happens when the people themselves leave has its own evidence and its own cures. Their own summary is worth quoting with its hedge intact: "Although the reasons underlying these results are not clear, they suggest that firms should be careful to not overanticipate performance benefits from postmerger sales force reorganizations. Furthermore, they suggest that a merger predicated primarily on the desire to acquire another firm's sales force resources is unlikely to succeed in the longer run."

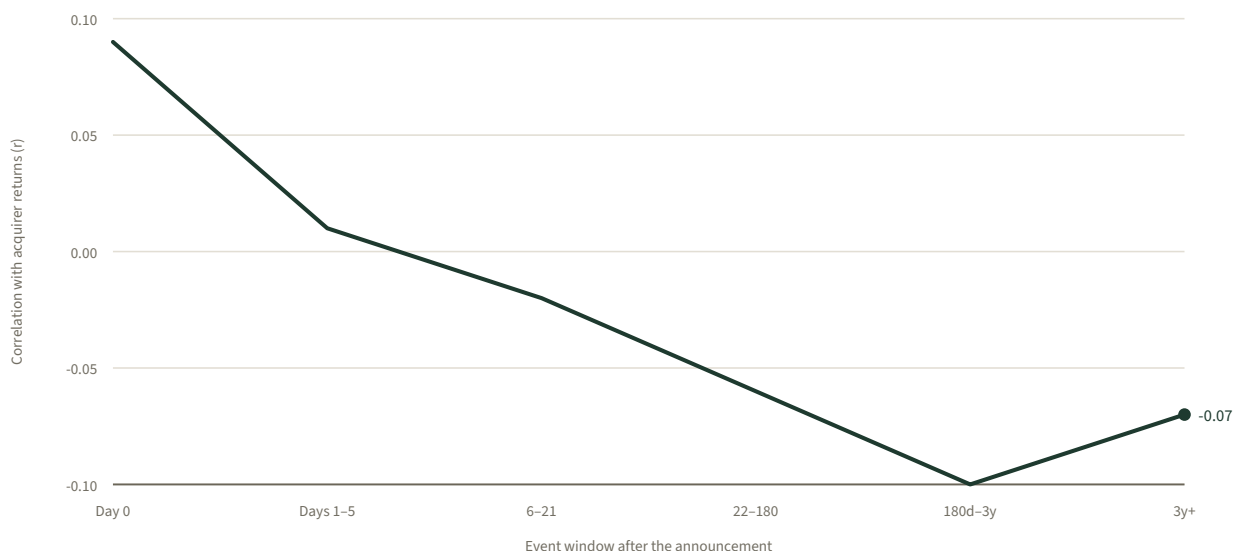
Scope, because it matters: these are mid-market manufacturing deals, closed between 1988 and 1992, with a median purchase price of 42 million on the 13 percent of deals where a price was recoverable. That is a different population from the 2-billion-plus transactions the consultancy surveys describe, and the two should not be stacked.

The decision this changes: “who reports to whom” is on the org-chart slide, and “whose systems, whose accounts, whose comp plan” is usually treated as its administrative consequence. The measured record says the second one is the decision and the first one is the paperwork. This dynamic reflects why integration savings come from what you put in, where synergies depend on active resource commitment rather than passive balance sheet reduction. The same asymmetry runs through what a proven playbook loses when it crosses a border: the sequence transfers, the position it was standing on does not. An acquired customer list transfers on the closing schedule. The interface holding those customers in place does not appear on it at all.

## Why does historical M&A base rate data fail to predict individual deal success?

The honest floor under all of this is a meta-analysis: across 93 studies, King and colleagues found that acquiring firms’ performance “does not positively change as a function of their acquisition activity, and is negatively affected to a modest extent.” Moderation was indicated for every event-window result and for return on sales, though not for return on assets at three years or return on equity, and the paper’s own conclusion is that unidentified variables explain significant variance.

Figure 1 The base rate, drawn: acquirer returns drift negative



King, Dalton, Daily & Covin (2004), Table 1: estimated population correlations with acquirer abnormal returns, 26 to 127 effect sizes per window, n up to 28,016, across 93 studies. Day 0 and the three windows from 22–180 days onward are significant; days 1–5 and 6–21 are not. Outcomes are the market’s verdict on the buyer: nothing here observed retention or cross-sell.

Read that carefully, because it licenses less than people take from it and something more useful. The outcomes are acquirer abnormal returns and accounting ratios: the market’s verdict on the buyer, and the buyer’s books. Nothing in it measured whether a cross-sell landed or a customer stayed. A base rate computed on share-price reactions cannot tell a commercial leader anything about the decisions in front of them, because it never observed

them. Whether depth, speed and direction are among the missing moderators is a guess, and I mark it as one: the two literatures measure different dependent variables and nobody has connected them.

Which leaves the numbers actually circulating in the room, and they deserve one paragraph and no more. The 23 per cent gap between revenue-synergy goal and attainment is real and it is a McKinsey survey of 200 executives who had each run synergy programmes on more than two deals over \$2 billion: a practitioner survey, useful as a description of what experienced people report, and not evidence. The companion claim that revenue synergies take five years against two for cost synergies rests on an exhibit whose cost-synergy curve is labelled, in the source, illustrative. And the hundred-day frame was tested in 2004 by Duncan Angwin, who found no significant association between the volume of changes started in the first hundred days and perceived success: with his own hedge, that an association does appear in years three and four and that hindsight and survivor bias cannot be excluded. Angwin is not the orthodoxy's advocate; he is its first critical reader.

There is an obvious objection to that paragraph and it deserves answering rather than surviving. Homburg and Bucerius surveyed 232 executives; Capron and Hulland surveyed 253. McKinsey surveyed 200. Dismissing one and building on the others looks like snobbery about the letterhead, and it would be, if the distinction were the letterhead. It is not. The two studies here publish their sampling frame, their response rate and their non-response tests; they print the wording of every question; they report the reliability of each scale; they validate their perceptual measures against objective financial data and against interviews with the customers themselves; they test their subgroup comparisons instead of pointing at stars; and they list what they got wrong and what they could not rule out. The consultancy survey publishes a number and an exhibit. That is the whole of the difference, and it is enough, but it is a difference of disclosure, not of virtue, and a survey is a survey either way. Everything above rests on people answering questions about their own deals. It was he who recorded that speed "has become a central message from consultants for post-acquisition integration, and focusing upon the First 100-Days has become de rigueur."

## What can empirical transaction measurement verify, and what remains uninsurable?

Here is the version worth running, and the version that will lie to you.

The one that lies. Rank your acquired accounts by revenue in the year before close, take the top decile, and measure what happened to them afterwards. I ran this on 400 synthetic accounts with no integration effect present anywhere in the data, 2,000 times. Mean result: a 15.1 percentage point decline, reading as damage in 73 per cent of runs. That is regression to the mean. Rank on a noisy measurement and the extremes move back toward the middle whether or not anything happened to them. Your best accounts will look punished by the integration in any dataset, including one with no integration in it.

The one that works. Take every account in the acquired base. Tag each one by an integration-plan variable, which workstream touched its contract, its price, its rep or its systems, and in which month. That tag is not a field in any system, and reconstructing it from the integration office's own workstream calendar plus the account teams is an afternoon's work, which is the honest price. Then compare revenue change across accounts the plan touched early against those it touched late. Critically: check with the integration office how the sequence was set. If the answer is "biggest accounts first," the comparison is confounded with account size and everything correlated with it, and you should say so rather than publish it.

And the step that makes the answer mean something. Before you run any of that on the post-close period, run the identical comparison on the pre-close period alone: split those months in half, assign the same exposure tags,

and measure. You know the true answer there is zero. Whatever spread you get is your own noise floor, measured from your own volatility rather than borrowed from anyone's assumption. In simulation this placebo recovers the true floor closely across a wide range of volatility, which is the only property that makes it worth prescribing.

Expect that floor to be wide. At the size of a typical acquired B2B base, a difference of twenty or thirty percentage points between two halves can appear with nothing happening at all. Which is the honest end of this: your own deal probably cannot answer this question. One deal is not a sample, and that is exactly why a twenty-year-old survey of 232 acquisitions is still the best answer available to you.

It should not be. Homburg and Bucerius wrote in their own limitations section that the next study should measure integration depth, speed and market-related performance from the customer's side rather than the manager's, and should ask customers directly how customer-oriented the integration felt. Their very last suggestion was to study how prices raised as a consequence of a merger affect performance: a question with its own measured record now, and one worth holding next to this one, since price positioning is item three on the integration-depth scale. That study would resolve the common-method problem, break the reverse-causality tie, and update evidence collected on deals that closed before the euro. Twenty-one years later I cannot find it, in either language, and the searches behind that sentence were run in August 2026 rather than settled for all time.

Until somebody does, the position is this. Three decisions: how deep, how fast, which way: are trades, not schedules. The rate at which they trade has been measured once, carefully, on deals that look less like yours every year. The conditions that flip their sign are known, and one of them is who is in the room. And the numbers in your deck are a survey of two hundred people and a curve the source itself labelled illustrative.

That is a worse evidence base than the confidence in the room implies, and a better one than the room is using.

Table 2 The measurement, and the traps it must survive

| Step                                               | What it guards against                            | What the seeded simulation shows                                                     |
|----------------------------------------------------|---------------------------------------------------|--------------------------------------------------------------------------------------|
| Tag integration exposure per account, before close | Without the tag, any later comparison is folklore | Not a field in any system; an afternoon to reconstruct                               |
| Never rank on pre-close revenue                    | Regression to the mean, read as damage            | The top decile of 400 accounts reads -15.1 pp with no effect present, in 73% of runs |
| Run the placebo split first                        | A measurement that alarms on random halves        | Random halves recover the true noise floor at every spread tested (0.20, 0.45, 0.80) |
| Respect the base-size floor                        | Twenty-point swings that mean nothing             | 95% noise range: -33.1/+34.2 pp at 100 accounts, -24.2/+24.4 at 200                  |

integration-exposure-null.py: 400 accounts, 2,000 trials, seed 20260807; published beside the pair's claim ledger and reproduces bit for bit. Author's own design; the simulated numbers are the null, not a finding about any deal.

## Where are the empirical boundaries of integration performance studies?

Boundary. The cited models distinguish cost and market effects; they do not price every integration action. Use the decision questions to test direction and exposure in the specific deal.

Evidence base. The analytical frame also draws on these additional sources: Angwin 2004; Capron and Hulland 1999; Chartier et al. 2018; Homburg and Bucerius 2003. The links identify the exact works; they support the mechanisms and boundary conditions discussed here, not every claim in isolation.

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END OF ESSAY

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