A CV OF FAILURES

Selected negative results from a career in experimentation

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Most CVs report the surviving hypotheses. This one documents some of the hypotheses that did not survive.

EXECUTIVE SUMMARY: A BRIEF HISTORY OF BEING WRONG

Economist by training, data scientist by profession, and enthusiastic generalist by temperament. A career spent moving between research, analytics, machine learning, product-like experiments and creative projects. The common thread is an unusually high tolerance for starting things before knowing exactly how they will end.

FAILED INTERVENTIONS

The Perfect Career Plan

Attempted to optimise career direction as if it were a tractable decision problem. Discovered that labour markets contain unobserved variables, delayed feedback and approximately zero clean counterfactuals.

Post-mortem: Result: stopped waiting for a perfectly specified career path; started treating careers as iterative experiments.

Being Good at Everything

Accumulated economics, statistics, ML, NLP, computer vision, visualisation, GenAI, research and assorted creative skills. Useful for solving unusual problems; less useful for answering the question 'what exactly are you?' in one sentence.

Post-mortem: Result: learning to present breadth as a coherent systems skill rather than a collection of unrelated hobbies.

The 'I'll Build It Properly From the Start' Hypothesis

Repeatedly discovered that the imagined final architecture is usually less useful than the ugly first prototype.

Post-mortem: Result: prototype first, architecture second.

Optimising Before Starting

Spent considerable cognitive energy designing the ideal project, portfolio, workflow or learning plan before producing version 0.1.

Post-mortem: Result: increasingly suspicious of plans that contain more detail than the thing they are planning.

Assuming a Good Model Solves a Business Problem

Learned that predictive performance is only one component of value. A model can be statistically respectable and operationally useless.

Post-mortem: Result: define the decision, prediction point, costs and business KPI before becoming emotionally attached to the algorithm.

NEGATIVE RESULTS FROM RESEARCH & DATA SCIENCE

Invoice dispute modelling

Discovered that class imbalance is not merely a modelling nuisance. The definition of the positive class, leakage risk, threshold choice and cost of false negatives can matter more than squeezing another few percentage points from the model.

Model comparison

Learned the uncomfortable lesson that a more sophisticated algorithm is not automatically a better model. Baselines are annoyingly useful.

Interactive analytical tools

Built tools and dashboards that taught a more general lesson: users do not experience your data pipeline. They experience the interface, the question it answers and the friction between those two things.

Causal inference

Studied problems where observational data can look persuasive while quietly refusing to identify causality. Developed a lasting distrust of sentences beginning with 'X increased because…' when no identification strategy follows.

PROJECTS THAT DID NOT BECOME WHAT THEY STARTED AS

Buddy / de-escalation bot

Started as an idea around rephrasing inflammatory messages and evolved through questions of moderation, language models, Telegram and interaction design. The useful output was less the final product than the process of discovering what the product actually needed to be.

Delhi markets / UX + data project

A deliberately interdisciplinary idea combining mapping, market behaviour, visualisation and UX research. The failure mode: too many interesting directions. The lesson: scope is a design decision, not an administrative detail.

Creative technology experiments

Painting, carving, lino, beadwork, jewellery, interactive storytelling and assorted attempts at making things with hands. A recurring failure: expecting a first attempt to look like the thing imagined in the head.

BAD ASSUMPTIONS

If I understand the problem, I should know the answer.	No. Sometimes understanding the problem is the valuable part.
More optimisation is always better.	Only if the objective function is correct. Otherwise you become extremely efficient at the wrong thing.
A portfolio needs to look finished.	A portfolio can also demonstrate how someone thinks, investigates and changes course.
Failure means the experiment was pointless.	A failed hypothesis can still produce information. A failure with no post-mortem is much more expensive.
Being interdisciplinary means being unfocused.	Sometimes it means being the person who can connect two disciplines that normally do not speak to each other.

SKILLS ACQUIRED THE HARD WAY

• **Problem formulation:** translating vague questions into measurable targets.

• **Statistical humility:** knowing when an estimate is answering a different question than the one being asked.

• **Model skepticism:** asking what could make the result wrong before celebrating the result.

• **Prototyping:** making version 0.1 exist before designing version 10.0.

• **Communication:** translating technical work into decisions that non-technical stakeholders can act on.

• **Scope control:** recognising that an interesting project can still be the wrong project to do right now.

CURRENT UNRESOLVED PROBLEMS

How to remain ambitious without turning every interest into an optimisation problem. How to build things before they are perfect. How to make room for curiosity, play and experimentation while still being economically sensible. How to distinguish a genuine failure from an experiment that simply returned an inconvenient result.

ROBUSTNESS CHECKS

> *Would I make the same decisions again with the information I had at the time? Not always. Would I make all the same mistakes again? Probably not. Would I deliberately run some of the experiments again? Absolutely.*

REFERENCES

Available from former colleagues, collaborators, friends, unfinished side projects, abandoned notebooks, Git histories and anyone who has watched me say 'I have an idea' immediately before creating unnecessary work for myself.

Version 1.0 · Prepared as an experiment in documenting negative results