The Birth and Early Years of Parameterized Complexity ⋆

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Abstract. Through the hazy lens of (my) memory, I will try to reconstruct how Mike Fellows and I, together with some co-authors in some cases, came up with the basic papers in parameterized complexity.

1 Introduction

When I agreed to do this archaeological exercise, I was rather enthusiastic until I tried to remember dates and places and what we did and where. Through cunning questions of Mike, without disclosing the existence of the Festschrift, I have confirmed many of the "facts" below. Also, I have e-mails from December 1990, and letters from 1991 and 1990. Unfortunately, many discuss "following our phone conversation" so in many cases it's back to memory.

I will not try to say who did what, since mostly it was a happy coalition of two friends. Anyway, my policy has always been that "joint work" is joint work. In our case it was definitely so : a nice interaction of complementary skills. Only as an illustration of this complementarity, I will say what happened with the very first papers [DF91,DF92a,DF92b]. It is both an interesting study in serendipity and an interesting study in complementary skills.

Before I begin, I take this opportunity to salute my longtime friend, my favourite co-author, and certainly my only co-author who understands the value of surfing. Congratulations Mike on the occasion of your 60th.

2 Beginnings

I know to within 2-3 days when I first met Mike Fellows. It was at the ACCMCC (a combinatorics conference series in Australia and New Zealand) conference at Massey University. This conference was in a medium sized rural town in New Zealand called Palmerston North, December 3-7, 1990.

Palmerston North is not the most exciting place in the world. In fact, much to the ire of the locals in 2006, John Cleese (of Monty Python fame) was quoted

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as saying "If you ever do want to kill yourself, but lack the courage, I think a visit to Palmerston North will do the trick.[1]" You may then ask yourself why Mike was visiting New Zealand for this meeting? As it turns out, in his youth he had seen a famous surfing movie called "The Endless Summer" and it featured surfing in the apparently exotic location of New Zealand, particularly Raglan and Shipwreck Bay[2]. He hoped to combine a visit here for the conference with a visit to some of the local surfing locations. But here's a classic case of serendipity. I did not have too much interest in ACCMCC, but in those days there was almost no research travel money to be had, certainly not by mathematicians or computer scientists in New Zealand. Hence, if there was any conference near, you went to it. My attitude was (and remains) "Who knows, maybe you might pick up an idea or two". In those days, you might only get one chance a year[3].

I can pretty well say that I met Mike on the 4th December. I know it was the day of the conference dinner and Mike was speaking in the afternoon. After his talk, I recall meeting him outside of the seminar room, saying that I thought the material was very interesting and loved the talk. I said it reminded me of a paper I had read recently from a *Contemporary Mathematics* proceedings.

Mike said something to the effect that "it should remind me as *he* wrote that paper and this is what it was about" (Fellows [Fe89]). It's always wonderful to run across someone who has actually read one of your papers, even if they don't recall who wrote it, and perhaps this was all to the good.

Mike started telling me about his work with Langston [FL87,FL88] of using Robertson-Seymour (e.g. [RS86a]) methods to demonstrate polynomial time

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[1] See, for example, http://www.abc.net.au/am/content/2006/s1586512.htm. The silly part was that the local tourist board took it all seriously and then the locals promptly called the local rubbish tip "Mount Cleese."

[2] My understanding is that on this trip the film gave a false impression, in that everywhere Mike went was blown out and cold.

[3] This was all before the Marsden Fund for basic science was set up in New Zealand, under the initiative of Sir Ian Axford. In those halcyon days, I recall being ambitious and hungry for the most recent ideas. There were a few small grants available. I did get a seeding grant from a committee called ISAT enabling me to have one overseas trip and several American mathematicians to visit me here in New Zealand. I am forever grateful for that. To get overseas, I would write to many universities in the US, asking would they be so kind as to give me a little money if I gave a talk, and then cobble the trip together using money from giving seminars at maybe 6-7 universities. Once I asked the travel agent to get me to Haifa the cheapest way he possibly could: Wellington-Sydney-Tokyo-Amsterdam-Tel Aviv with bad connections, 54 hours. (KLM had a deal for a $50 to anywhere in "Europe" side.)

There were some grants available to people via what was called the "University Grants Committee" but any proposal needed to get out of the local university, in those days here controlled by non-mathematicians, who everyone knew were "world class" by some unknown mysterious mechanism. I once tried to get one of these grants to visit Mike Fellows for collaborative research, and received the official reply that we don't support "Research Fellows." This response showed how seriously they read the proposal. Fortunately things changed once all proposals were internationally vetted.

complexity non-constructively. Mike asked me about my background. Hearing it was in logic and computability theory/structural complexity he suggested a problem I might be interested in. He gave me a copy of his paper Abrahamson, Ellis, Fellows and Mata [AEFM89] the "PGT" ("polynomial generator tester") paper. The contents of the PGT paper are quite important for this story and I will soon discuss them in detail. We decided to sit together at the conference dinner and discovered that we also shared a love of surfing and a love of wine, having had a couple of bottles of some Villa Maria Cabernet Merlot 1989. Mike suggested I might like to work with him on trying to prove a Ladner style density argument for the PGT setting for the complexity classes for "families of relations."

Ladner's Theorem [La75] is often quoted as being that if $P \neq NP$ then there is an "intermediate" NP language, which is neither NP-complete, nor in P. But in fact, Ladner proved that if A and B and computable languages with $A <_T^P B$ then there is a computable language C with $A <_T^P C <_T^B$. In unpublished work (a proof can be found in Downey and Fortnow [DFo03]), Shinoda and Slaman proved that you can remove the hypothesis that A and B are computable, and that this result holds for any languages $A <_T^P B$.

Since it is relevant to the story at hand, I will digress as to how I got into complexity theory at all. My background was in computable algebra and classical computability theory. Here one calibrates the universe into classes of relative computability under various reducibilities and in applications to algebra, you look at things like, e.g. computable fields and ask if every computable field has a computable algebraic closure. As with all things I study, I retain an interest and recently showed that computing whether two finitely presented groups have the same first three terms in their integral homology sequence is what is called Σ_1^1 complete. This shows that as an invariant it is as bad as it can possibly be. (Downey and Montalbán [DM08].)

At a certain point in the late 1980's, I resolved to learn some complexity theory, as it is always interesting to work in new arenas. This is something Mike is extremely keen on. He reads a lot. He can *see* many elegant applications into new areas of, for example, biology, VLSI design, linguistics, learning theory etc[4]. Branching out into a new area is something you can do at a small department like mine, and I did this by teaching complexity theory (as per Garey and Johnson [GJ79], and Balcazaar, Diaz and Gabarro [BDG87]). I wrote some papers on classical "structural complexity" such as the structure of the polynomial-time degrees (for example [DGHM89]).

Because of this background, I was familiar with Ladner's results so I guess I was a good candidate for Mike's question.

The point of all of this was that Mike left me a copy of the PGT paper, and I promised to look at it as soon as I could. I began the next day.

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[4] One thing I always liked in Mike's homes was that there were always teetering piles of books from all kinds of areas of human endeavour, and papers from many areas of science scattered everywhere.

It had long been recognized that some kinds of intractability might be better than others as mentioned specifically in Garey and Johnson [GJ79]. There had been no real quantification of what this meant, except perhaps forays into average case complexity, approximation algorithms and strong NP-completeness. There seemed no history of worrying about parameters in the input. Whilst it seems that earlier authors such as Ken Regan in [Re89] (who talked about k-DOMINATING SET in passing) and Moshe Vardi [Va82]. Vardi pointed out that the input for database-query evaluation consists of two components, query and database. For first-order queries, query evaluation is PSPACE-complete, and for fixpoint query it is EXPTIME-complete, but, if you fix the query, the complexity goes down to LOGSPACE and PTIME correspondingly. In particular, the size of the database was not the right complexity for database query complexity and the size of parameter counted. Also in the 80's were the papers Vardi and Wolper [VW86] and Lichtenstein and A. Pnueli [LP85] who pointed out that the input for LTL model checking consists of two componets, formula and trasition system. LTL model checking is PSPACE-complete, but if you fix the formula, the complexity goes down to NLOGSPACE.

So people in the database community were very aware that fixing a parameter makes an intractable problem tractable. In retrospect the key if that they did miss the BIG difference between query evaluation and model checking. In query evaluation the dependence on the formula is exponential, while in model checking it is multiplicative. Indeed, as was shown later, model checking is FPT and query evaluation is likely not FPT. All this happened in the 1980s, but in spite of the clues, these workers completely missed parameterized complexity theory. Of course other earlier work is discussed in Mike Langston's article in this volume. The first real breakthrough was the PGT paper of Abrahamson, Ellis, Fellows, and Mata [AEFM89].

In [AEFM89], they modeled parameterized problem by what they called *polynomially indexed relations*. The crude notion of (nonuniform) FPT is definitely in [AEFM89] as the notion of easiness. Specifically, Abrahamson, Ellis, Fellows and Mata defined a P-indexed family of relations $\Pi \subset \Sigma^* \times \Sigma^*$ with the n-th slice having $|x| = n$. Then this class is said to be[5]

- P-bounded if there is a polynomial p such that $(x, y) \in \Pi$ implies $|y| \leq p(|x|)$.
- P-checkable meaning that there is a polynomial time algorithm to decide if $(x, y) \in \Pi$, and
- P-indexed meaning that there are polynomial time algorithms to compute (a) $(1^n, i) \mapsto y$ such that $i_n(y) = i$ and (b) $(1^n, y) \mapsto i_n(y)$ for $y \in \mathrm{ra}(\Pi_n)$.

Then the problem kinds they considered ("PGT=polynomially generator tester pairs") were of the form as follows.

Input $x \in \Sigma^*$ and $j \in \mathbb{N}$ with $j \leq q(|x|)$.

[5] The reader should not stress too much about the details of the definition, I only give it for historical interest, and to show that it is extremely unwieldy.

Question Is there a y with $(x, y) \in \Pi$ and $i_{|x|}(y) \leq j$?

The example was Vertex Cover. In this [AEFM89] setting, deciding whether there is a vertex cover of size $\leq k$ corresponds to the input (G, J) the q bounded search problem for $\Pi = VC$ and $j = j(n) = \sum_{i=0}^{k} \binom{n}{i}$. (This is quoted straight out of [AEFM89].)

There is a similar multi-line definition of a many-one reduction from Π to Π' as a function $f : (x, i) \mapsto (x', i')$ and

- f is computable in time polynomial in $|x|$ and $\log i$.
- There are polynomials r, s, t with $|x| \leq r(|x'|)$, $x'| \leq s(|x|)$, and $i' \leq t(i)$.
- $\exists x((x, y) \in \Pi$ and $i_{|x|}(y) \leq i)$ iff $\exists y'((x', y' \in \Pi'$ and $i'_{|x'|}(y') \leq i')$.

Using this very cumbersome definition, Abrahamson, Ellis, Fellows and Mata showed that things like k-Linear Inequalities had the same complexity as k-Short Sat (for arbitrary formulas), and asked the question of how things like k-Dominating Set fell into this classification. (It does not seem to as the classes are really somehow concerned with nonuniform $W[P]$.) Most of the reductions, not surprisingly, look akin to recycled LOGSPACE completeness results, and all resurface later in our paper [ADF95] with Abrahamson where we look at $W[P]$, and alternation. There is an error in the paper as it purports to show that weighted sat is (essentially) $W[P]$ complete, where in the proof of Theorem 6.5 it is claimed that unravelling of the formula is equivalent to to asking if it is satisfiable, (which is true) but the point is that the process makes more variables true so the reduction cannot be parametric. However, we were able to rescue a lot of results in [ADF95], and discover a further collection of $W[P]$-complete problems (this happened in 1993).

As you all know, Mike's personal background is pretty colourful. It involves the armed forces, jumping out of planes in parachutes, later fishing in dangerous waters, colourful living in San Diego and then finally getting into graduate school at UCSD due to the intervention of Michael Freedman of Fields Medal fame[6]. I think that this eclectic background has enabled him to be very creative, and I think you can see this in the [AEFM89] paper, as well as his earlier work with Langston [FL87,FL88] (article included in this volume). Both he and I are from relatively modest childhoods, and I believe that this often makes you quite ambitious. Maybe this is where the initial drive came from.

The story continues as follows[7]. I sat and stared at the [AEFM89] paper for a few days and thought two things. First, there is the kernel (no pun intended) of a *very exciting idea* in this paper buried under layers of ugliness, which does not get to the key issue; and second I will have a lot of trouble proving a Ladner Theorem with such an unwieldy definition. *So maybe there is a simpler formulation.*

At a certain moment I recall thinking why not simply study languages $L \subseteq \Sigma^* \times \Sigma^*$ or $\Sigma^* \times \mathbb{N}$, and have reductions as what we now see as parametric connections $(x, k) \mapsto (x', k')$. The rest of the [AEFM89] hides the core issue. I

[6] The occasion prompting Freedman to intervene being present in one of Mike's plays.
[7] This is the only point I will say who did what.

am sure that this all came out of my logic training. I then figured out the density theorem (eventually being published in [DF93] and announced in the abstract with Karl Abrahamson [ADF93]) and wrote and maybe faxed Mike several letters including a 26 page handwritten letter with a lot of proofs. We were very excited and we spoke on the phone for long periods. Since our mathematical backgrounds are quite distinct and Mike had not seen priority arguments before, likely he had trouble following my scrawls. *But* after we talked through the issues on the phone and he clearly saw many ways in which the simple definitions could be applied to Dominating Set. It must have been by the 18th December 1990 as I have an e-mail where Mike summarizes all the definitions. In that e-mail he sets down his ideas towards using logical depth, in the format of weft, (and hence the W-hierarchy) as the basis of classifying parameterized complexity. So you can see this all happened very quickly, maybe a couple of weeks. I thought weft was a terrific idea but had seen no proofs.

Where did Mike get this nice idea from? Certainly he had been thinking about Dominating Set and Independent Set as well as Vertex Cover. If you look at the logical form of the first two, you can see the form of $W[2]$ and $W[1]$ respectively, provided that you are prepared to use boolean circuits as a platform for the intractability "core problems". Only later with Liming and Jainer were we able to get nondeterministic Turing machines into the picture. ([CCDF96].)

Even now we don't have Turing machines in the picture for the miniatures $M[1]$, and this is a great open question. (Is $k \log n$ Turing machine acceptance in $M[1]$?)

It is also interesting that at the bottom of the e-mail of the 18th December are the questions (i) if $P \neq NP$ does this hierarchy separate (ii) If $W[t] \neq W[t+1]$ are there infinitely many equivalence classes, and (iii) Relationship between the $\cup W[t]$ and $W[P]$. At the time (i) has us thinking about oracle separations, (ii) relates to density and (iii) remains interesting even now.

At this stage, Mike had a lot of grant money, and asked me to visit him. He thought we could work well together and work out the details of this very attractive material. I was very excited as I could see that there was a lot of potential. Clearly I thought that what we were doing might be important, as I have kept some of these early e-mails[8].

In early 1991, I visited Mike for the first time[9]. I stayed at Mike's house in Victoria on Vancouver Island. I recall getting there and him telling me that they were practicing ecological front lawns (which seemed to equate to not mowing ever). The house was occupied by Mike's family (two nice kids Max[10] and Hanna) and his then wife Roberta. I recall that it usually smelled of coffee, and was

[8] Or the other interpretation might be that I am a horder of such things. Only in 1992 did I keep some kind of e-mail file deliberately.

[9] Of course, one hero here was my poor wife Kristin who had to deal with two children under three by herself. On this trip I think she tripped carrying a baby car capsule, and sprained her knee.

[10] One great memory I have of Max was that, on a later trip, we were going over to a place called Cox Bay on the west coast of Vancouver Island for a few days surfing

covered with piles of books and papers, as I mentioned earlier. There I discovered that Mike would get irritable if he did not eat enough eggs, and we would eat a lot of Mexican food. I would stay in a room in the basement, next to bathroom that Mike "fixed" with a technique which inevitably involved vast amounts of silicone being squeezed in a large ungainly mess.

Mike essayed to me details of his ideas of using circuit weft as a basis of classifying parameterized complexities. In particular, Mike had the *key reduction* for CNF Sat, which is used throughout [DF92a,DF92b,DF95a]. During that visit, I well remember working each day on a little white-board or maybe flip-chart at his home where many of the details of the first paper(s) [DF91,DF92a,DF92b] were worked out. Already, Mike had the fundamental idea here for the $W[t]$ classes, and for $t \geq 2$ we figured out the details on this trip.

Aside from the project we talked about lots of other things ranging from poetry to mathematics education. I had been involved in mathematics education in New Zealand, once organizing a conference for teachers at my home university. I found this something that totally sucked up your energy. I gave Mike the "sage advice", don't do it, it will ruin your research. Well how wrong I was on both counts! I am glad that there are nice accounts of Mike's initiatives with Neil Koblitz ("The Mathematics Liberation Front"), Tim Bell and Nancy Casey.

Mike also knew of my interest in surfing as we had spoken of this in Palmerston North and on the telephone. I brought my wetsuit to Canada and we went to this place called Sombrio. In the early 1990's this was a place occupied by a few guys who seemed to live like some kind of refugees from the 1960's in old shacks. In those days, Sombrio was somewhat difficult to get to. A couple of hours drive, then a walk for half a kilometer through knee deep mud. Boy, was that all fun. In 2008, Mike and I went there for old times, when STOC/IWPEC was in Victoria. Now, it is all made up track, part of the West Coast Trail, and there were 50 guys there surfing![11]

As with all the trips, Mike and I spoke about maths most of the way. It's a great model for research.

I have a draft manuscript ([DF91]), dated March 25, 1991 called "A Completeness Theory for Fixed Parameter Problems" which has the new definitions, weft ideas, some hardness proofs, and many of the basic results from [DF92a,DF92b]. It is not overly well written but has problems considered like k-perfect Code, k-Not All Equal Sat, k-CNF Sat, k-Dominating Set, k-Independent Dominating Set, and the like. It also has a collection of FTP examples like Feedback Vertex Set, Planar Face Cover Number, Min Cut Linear Arrangement, Graph Genus, known mainly to Mike at that

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and work. Mike said to Max we had to leave soon and was he packed? Max grabs a jacket, jumps in the car and says something to the effect of "Let's go!" That was the trip where Mike forgot Roberta's clothes, something we discovered half way there.

[11] On another trip, in 1992, we took Mike Hallett, one of Mike's PhD students, and we put him in a dive suit. He fell off a wave and was washed across the reef (without touching it) looking like the gingerbread man as he could not bend too well in 9-18 mm of rubber.

time. It has the basic weft reductions, though I think it slurs over the "obliviousness" of the k-CNF Sat reduction needed for the induction for the higher wefts. I also have some copies of some old slides of Mike's where he spoke at a meeting at Manitoba on this material a little later. (This was the basis of the paper [DF92a].) I recall that we submitted [DF91] to FOCS, and it was rejected[12].

It was quite early on that we noticed the issue of uniformity in the reductions. However, it was not really till the March that we refined this to the three definitions we now have. This occurred after a debate as to whether Graph Genus was strongly uniformly FPT. Graph Genus is only nonuniformly FPT on the face of it. Mike pointed out that it was uniformly FPT by Fellows and Langston [FL89b]. It was only shown to be strongly uniformly FPT in 1999 by Mohar [Mo99]. I think to some extent this shows where our "head space" was at the time, as we were still fascinated by the Robertson-Seymour material.

Incidentally, I still think that there is a very interesting project with this material. The hypothesis $FPT \neq W[1]$, say, has two very different meanings depending on whether we mean uniform or nonuniform. In the *nonuniform* case it says that determining whether a 3CNF formula has a weight k satisfying assignment is not in DTIME(n^c) for some fixed c, and hence from some slice onwards, deterministic algorithmic must take more time. In the uniform case it is apparently weaker, and apparently it "could" be that all the slices are in DTIME(n^c), but only nonuniformly. This is because the uniform case asks for a *single* algorithm to witness this inclusion. When I think of the issue I think that $W[1] \neq FPT$ *nonuniformly*, as the spirit of the programme.

3 Precursors

Before we move on, I guess we should look at where these ideas germinated. Yes, it is true that the relevant event was the [AEFM89] paper but also, in retrospect, you can see the ideas crystallizing out from earlier considerations; especially of the work of the two Mikes, Fellows and Langston, and their co-authors.

Even though we evolved to think of this as addressing practical computing, a real inspiration was the theorem of Robertson and Seymour which stated that finite graphs were well quasi-ordered under the minor relation, and immersions. Furthermore for a fixed graph H, $H \leq_{\text{minor}} G$ is $O(|G|^3)$. Hence any minor closed class had a polynomial time (FPT anyway) recognition algorithm, in spite of the fact that we did not know what it was. This is a stunning theorem, and it has yielded a revolution in graph theory in the last 30 years. The algorithms stemming from applications of Robertson-Seymour wqo theory are, or course, wildly impractical. Mike tells me of a famous computer scientist saying "This is not computer science, it is mathematical curiosity!" *But there are so many practical, nearly practical, and fascinating spin-offs.*

[12] I have only ever been involved in two submissions to FOCS/STOC. One was [DF91], and the other was the recent one on kernel lower bounds [BDFH08]. Both were rejected.

I have something else Mike gave me. An old proposal by him and Langston to use wqo methods to design VLSI circuits. And it was funded by the NSF, and later the Office of Naval Research! Wow, is that blue-sky research. But look what it yielded : If nothing else, parameterized complexity.

The point here is maybe Mike was sensitized by his work with Langston (such as [FL87,FL88]) on applying and effectivizing Robertson-Seymour wqo theory. It focuses us on the issue of the parameter, *then* the algorithm once the parameter is known. In the Robertson Seymour case, the parameter is the obstruction set.

Also surely related here is the paper on cutset regularity (=finite index) by Fellows and Langston [FL89] that was quite important I believe. Fellows and Langston used an analogue of the Myhill-Nerode Theorem combined with a parsing language for graphs of bounded treewidth to establish a general methodology for fixed parameter tractability (where the parameter is treewidth) as per Courcelle's Theorem (as discussed here in Downey [**?**]), and a method of establishing that something is likely hard by showing that it is not of finite index.

Additionally, with the wisdom of hindsight, it spotlights the notion of implicit parameters like graph width metrics. For us, however, this material was more important when the book was being written.

On the other hand, in retrospect, it might have been a bit unfortunate to tie the FPT material to the Robertson-Seymour material when we spoke. Many listeners thought that what we were doing was basically applying Robertson-Seymour. For example, it is really striking how much it is mentioned in my Dagstuhl 1992 abstract. That is, initially, we failed to focus on practicality.

There are other precursors such as Kintala-Fischer's [KF80] model of limited nondeterminism, but we were unaware of this paper at the time. Also, Kintala-Fischer approach there does not split the problem into slices, is not applied anywhere, and has trouble dealing with the issues we deal with.

4 Figuring out $W[1]$ and the great Kiwi road trip

We found that we worked well together. Mike decided to visit me in Wellington. I cannot remember when, but it was almost certainly the northern summer, so maybe May or June 1991.

At the time, we had this framework (which was submitted to FOCS), lots of enthusiasm, and the whole of the world of NP-complete (and other) problems to see if we could find other interesting applications. The relative complexity of the proof of k-Dominating Set being $W[2]$ complete showed us that the reductions likely would not be easy.

In particular, we had not figured out the situation for $W[1]$. Given the relative delicacy of the reductions, we strongly believed that $W[1]$ would stratify into an infinite collection of levels $W[1,t]$. Here $W[1,t]$ denoted the class of problems FPT-reducible to depth 2 weft 1 circuits with one large *And* gate and whose small *Or* gates above have fanin bounded by t. We decided to make a surfing road trip and work on these issues, and my wife let us go.

Thus I grabbed my "Guide to New Zealand Surfing", surfing gear, two books of poetry (Collected Poems by Michael Dransfield, and one called "Applestealers" about "new poetry" in Australia)[13], Garey and Johnson, lots of paper, two clipboards and some preprints, and off we went. We basically circumnavigated the North island of New Zealand below Auckland, traveling to New Plymouth, Raglan, Mount Maunganui and Newdick's Beach, Gisborne, Mahia, Napier, White Rock and then home. For about a week, we drove, worked, surfed and had a fantastic and incredibly productive time. Contrary to our intuition, we realized that there was *no* stratification of $W[1]$. This is the basis of the core paper [DF95b], and contains that lovely reduction for RED/BLUE NONBLOCKER and hence the completeness for CLIQUE and INDEPENDENT SET.

We also discussed another question brought up by Mike in an e-mail of February 27, 1991. Mike says he noticed this "weird thing"; which was that a certain problem whose unparameterized version was in Σ_2^P did not seem to fit the model we had. He said "Maybe the whole hierarchy is some kind of analog of the polynomial time hierarchy..." "Or maybe there is some kind of weird combinatorial reduction placing this above the current hierarchy." I don't recall that we made progress on this, but on my next visit to Mike in April or May 1992 we worked on this, eventually also with Karl Abrahamson, resulting in the paper [ADF95].

Like much of our time together, that first trip had a lot of great memories. The high point was coming over the hill at Mahia, and it looking like a surfing movie: lines stacked to the horizon. We surfed till we dropped and then drove that night into Napier. Mike slept most of the way. One the way in, we decided we needed a beer, and stumbled into this bar on the highway, not noticing the trucks and motor bikes outside. The place was full of large, tattooed and scary people, so after a quick beer we ran like rats.

5 Getting published and promoting the material

Some time late in 1991 we received the news that the abstract [DF91] had been rejected from FOCS. We were really annoyed. Later people I spoke to like Stuart Kurtz and Lance Fortnow said that the abstract should have been accepted. I had been told by a number of people that this is why there are so many spin-off conferences like CCC as STOC/FOCS has strong opinions as to what constitutes an advance.[14]

Although it anticipates things somewhat, later we had reviews of some of our papers which said really revealing things like: "What this subject really needs is

[13] Mike can read while in a moving car, and with me driving, we would alternate between a little poetry and a lot of maths.

[14] Whilst I serve on many CS conference committees I don't like the method. I always think of the art contests in Paris in the late 19th Century, and think of the paintings rejected; Van Gogh, Rembrandt, Rousseau, etc. Who do we remember now? I think that these institutions are intrinsically conservative. Moshe Vardi had a very interesting article about this in a recent Communications of the ACM. (Vardi [Va09].)

for it to be developed by someone like *here unnamed famous CS professor* and their students." Hardly interacting with the *science* or *merit* of the *work*.

Having found out about the rejection, we decided to try for the 1992 *Conference on Computational Complexity* or *Structure in Complexity Theory* as it was then known. I have a version of the abstract we submitted dated December 9th 1991. Certainly it was much stronger than the earlier abstract, and includes the $W[1]$ collapse.

Mike was already doing what he does so well. Traveling around interacting and spreading ideas. He had had some excellent feedback both informally and about the material when he gave seminars.

The first time I spoke on this material was at Schloss Dagstuhl 9.00 am on Monday the 3rd of February 1992. I was the first speaker in the whole *Complexity Theory Seminar*. I was luckily invited for my early work in structural complexity. I had flown in from Wellington the day before. I could see in the eyes of the audience that this was a "good idea." It does give me pleasure to go back and read the relevant abstract from the book they have at Dagstuhl.

For some naive reason I had expected lots of workers at that meeting to stop what they were doing and launch into the new theory. I particularly thought would happen this after Mike and Hans Bodlaender [BF95] showed that k-Processor Scheduling would likely not be in polynomial tme, using our technology, *without* showing that it was NP-complete. More specifically, it had long been known that if k was part of the input, then k-Processor Scheduling is NP-complete, but for a fixed k, the NP-completeness or otherwise of k-Processor Scheduling is a prominent problem in the back of Garey and Johnson [GJ79]. What Hans and Mike did was to show that it is $W[2]$-hard. This means that, assuming $FPT \neq W[2]$, there should be no feasible algorithm for large k. The hidden message of the Bodlaender-Fellows breakthrough is that it is possible to prove hardness without establishing NP-completeness[15].

The mass parameterized migration did not happen with the exception of a few cases like Ken Regan. In fact, I think the majority of workers in complexity theory at the time remained and possibly remain unaware of the definitions, which is kind of a shame since it seems one of the few successful coping strategies.[16] With the exception of some people such as Bill Gasarch, Alan Selman and Eric Allender have kept track. Even for those who seemed to like it, it was quite different from what they were doing at the time[17].

[15] Later this methodology was taken up by Aleknovich and Razborov [AR01] who recognized the value of complexity classes sensitive to isues within polynomial time.

[16] Notably, most texts on computational complexity don't even mention it. Perhaps this is because complexity theory sees itself as being concerned with showing something *can't* be done, whereas a nice aspect of parameterized complexity is the focus on *trying to serve practical computation.* I am willing to believe $P \neq NP$, and as a consequence we need a complexity theory that "serves mankind" in the form of practicioners.

[17] But at least both Mike and I were invited to the next *Complexity Theory* seminar at Dagstuhl in 1993.

In general, I think the majority of people keep doing what they do, but a little more on this later. At that Dagstuhl meeting the big thing was Ogiwara-Watanabe [OW91] and the leftset technique for looking at constrained reductions from sparse sets. The other thing was to define new complexity classes. There was an edict put out in this meeting that it should be illegal to define a complexity class and not populate it with a concrete problem. Fortunately for us ours had members!

Dagstuhl was very interesting in those days, as it was only the "castle" and did not include the lovely new "ring" building joined to the castle by the bridge. It had the worst Internet in the universe upstairs where the games rooms are now. I recall that loading a single page over the Internet via the dial-up modem would take an hour. I remember learning a lot about GRAPH ISOMORPHISM at that meeting. Some of us also went for a run as a group in some old tracks, with totally out of date maps and got totally lost. An hour run turned into a 2.5 hour one.

In 1992, I had a sabbatical and was to spend May-December at Cornell University. I recall dropping in to Mike on the way. I did not know much about treewidth nor about wqo theory, and Mike set about teaching me that. He showed me Bodlaender's (and his student's) work on algorithms for graphs of bounded treewidth and explained his work with Langston such as [FL89]. I remembered that we tried to prove that graphs of bounded treewidth were well quasi-ordered by the minor ordering using methods from automata theory. This is a cool project that has never worked out, but is still a fascinating possibility. We also worked on expanding our repertoire of hardness results into other combinatorial problems. In particular, at that time we worked on applying this to Angluin-type learning complexity with Mike's student Patricia Evans [DEF93]. I recall meeting Neil Koblitz with whom Mike had been working on parameterized versions of cryptography [FK93]. I think it was in a car trip to Sombrio with Neil in the car that we realized that all problems are kernelizable iff FPT, whereas we had been trying to show that there were some problems in FPT which were not kernelizable. Thinking about this lead to the paper with Liming Cai and Jianer Chen (who I had not met at the time) on advice classes of parameterized complexity. (That is, FPT=Polynomial time with slicewise advice.) This eventually appeared in [CCDF97] due to the enormous backlog in the journal. The recent *WorKer*[18] Leiden talk by Dániel Marx has a lot to say on the issue of Kernelizable=FPT vs e.g. search trees. Dániel has expanded this talk into a contribution to the present volume (Marx [Ma12]).

Of course at the time we had several problems which are still with us. Collapse propagation of the W-hierarchy, approximation (e.g. $2k$) FPT approximation of DOMINATING SET[19], how to deal with space.

[18] A very nice workshop at the Leiden conference center based on topics around the theory of kernelization. `http://www.lorentzcenter.nl/lc/web/2010/418/info.php3?wsid=418`.

[19] To wit, is there an FPT algorithm which either says no size k domintaing set or gives a size $2k$ dominating set.

I think at that time Mike's interest had moved into computational biology and string matching, such as LCS. He had the very clever student Mike Hallett, and had connections with Tandy Warnow. I know he was also working with Hans Bodlaender. (Certainly he and I later talked on pattern matching and the material which resulted in [BDFW95] and [BDFHW95].)

During that visit, I began writing [DF93] and did not *Latex* at the time. Mike gave me his source for [DF92a], and said "just do this". Little did I know that I was about to learn *Latex* from someone who was learning disabled in the area. It took me a few year to discover the command "\begin{theorem}" or that Latex would automatically number things (like theorems, bibitems, etc) or even that there were things called bibitem, cite or ref.

We talked a lot about the programme of getting the work to penetrate. We decided that *if the work was of value then it would be seen to be so at some stage, history would be the judge.* There was no reason that some person working on their own stuff should want to change, *but* if we had a source for the material (like a book) then graduate students would maybe pick it up. After all, writing a book would "surely take only a year or two to write!" Little did we know how wrong that timeframe was to be. I have an e-mail from July 14, 1992 indicating that I was writing a chapter on the Abrahamson-Fellows [AF93] methods, and asking if Mike knew of any FPT applications of the minimization of a submodular function, or of combinatorial optimization. I also mention the beginning of Appendix 1. Clearly the book must have been started by then. So I guess it only took seven years. I also note that around that time Liming Cai and Jianer Chen were definitely in the frame. Almost certainly Mike had visited Jainer, and Liming was a student at the time. They had told us of the Papadimitriou and Yannakakis [PY91] work on MAX SNP and their work showing how it relates to FPT. By the 16th July we were reading Kintala-Fischer and could see how many convolutions are needed if you try to address limited nondeterminism nonparametrically. At the time we were also wondering is there was a decent stratification of the classes using FPT reductions polynomial in both the parameter and the input. It is nice to see this concept resurface in Dániel Marx's material at *WorKer*' 10 (and present in Marx [Ma12]) on bounded search trees where they are called "Polynomial parameter transformations".

Sometime around then, we heard that our paper was accepted by *Structures in Complexity*. We also decided to write up the two papers from that abstract [DF95a,DF95b]. I think Mike wrote the first one and I did the second one.

When I got to Cornell I spoke at the meeting for Anil Nerode's 60th Birthday in maybe May or June. My old advisor John Crossley, co-author Jeff Remmel, and Anil Nerode were in the audience and immediately said what exciting stuff this was. It could be "really important." Nerode said he would support the project in any way he could, especially as an editor. This certainly made Mike (when I told him later) and I very happy as Anil has excellent taste in mathematics and computer science. One of the first papers to appear [DF95c] appears in the volume coming from this meeting.

Both Mike and I were talking about the material all over the place. I spoke in Cornell, Chicago, Urbana, Maryland, George Washington, and a number of other places. Were we pushing Robertson-Seymour too much? Bill Gasarch told me that the W-hierarchy was viewed a bit as the "wierdness hierarchy."

It is of course natural to view problem classification as a function of logical depth. Perhaps it is the fact that NP is a syntactic class whereas ours were closure under FPT reductions, making them more semantic. There is no difference with respect to the intractability issue. (i.e. if, for instance, $P \neq NP$ then co-NP is not in P so in terms of hardness co-NP hardness shows things hard just as well as NP hardness.) But the difference does surface if you try to, for example, prove an analog of Toda's Theorem, where there is real trouble with "$BP \cdot W[P]$". This remains a great set of problems. How do you do randomized computation with only, for example, $k \log n$ bits of nondeterminism? It is also possible to show something like $FPT^{\#W[P]} \supseteq \cup_k AW_k[W[P]]$ where this is the k-th level of the AW-hierarchy, or just $FPT^{\#W[P]} \supseteq AW[*]$. To do this without routing through an analog of BP would entail a new technique to prove $P^{\#P} \supseteq PH$. (That is, proving it or $P^{\oplus P} \supseteq PH$, by counting, but using no probabilistic amplification.) The one parameterized version of Valiant-Vazirani [**?**] by Downey, Fellows and Regan [DFR98] hides things in the reductions. Recent papers by Moritz Müller [Mu08a,Mu08b] describes the issues and are the state of the art.

6 Mr. Feasible

What is the value of Cook's Theorem? (Or perhaps the "Cook-Levin Theorem") Why is it significant? The proof that predicate logic is undecidable by using predicates to emulate Turing machines had been around since the 1930's. By Herbrand's Theorem, we know that quantifiers can be emulated by infinitary propositional formulas, so in some sense it is hardly surprising that a miniaturization of these ideas can show that CNF Sat is NP-complete. The proof of Cook's Theorem is hardly difficult, *but is an seminal result.*

The crucial value of Cook's Theorem is that it tells us *why* things like Sat are hard, assuming NTM Acceptance is hard. Karp then shows us that hardness is everywhere amongst natural practical problems. In terms of *practical* computing, these two papers must also tell us that, if we are confronted with an NP-complete problem, and we *actually need to solve it*, we should seek other coping strategies. One of the problems with NP-completeness theory is that it really only tells us what *won't* work, not *how* to tackle the problem.

On the 25th February, 1991, Mike said "As for practical, I don't know. It's a bad new theory. Apart from completeness there are some fun positive results..." So whilst we discussed the practicality of the material, at the beginning we did not see the utility of thinking parametrically for *practical computation*[20].

I cannot recall when we began to realize that FPT gave more than NP. At some stage, we realized that it could be used as a *systematic method of attacking*

[20] Even as late as Downey, Fellows and Stege [DFS99], we were saying "the extent to which FPT is really useful us unclear." We now know that it really is.

intractability as later articulated in the paper with Mike's student Ulrike Stege [DFS98,DFS99]. I believe that it was then that we moved away from Robertson-Seymour and began focusing on kernelization, bounded search trees and the like. Of course, Mike had known of a number of examples before this. (Witness [DF92a].) *But* it was sometime during 1992 that we had kind of an epiphany in this direction.

Personally I believe that this is what is really cool about the area, and why it has flourished. The emphasis went directly towards applications[21]. Our statement of purpose and the delineation of these techniques is clearly found in the paper [DF95c], which we called "Mr Feasible". We referred to [DFS98] and [DFS99] as "Sons of Feasible."

When we write the revised teaching version ([DFta]) of the book [DF99], the emphasis will be towards practical considerations. Industrial strength FPT is where we should look. This was in the "manifesto" we called the story of Dr O, in the beginning of the book.

7 1993 and beyond

In 1993 we continued to develop both the theory and some co-authors. Papadimitriou and Yannakakis [PY93] wrote their paper on VC-dimension and $NP[\log n]$ which is NP with logarithmically many bits of nondeterminism. Later we showed how this fits into our setting. In that paper they mention our work, as they later do in [PY97] as a basis for complexity in database theory, where of course parameterized complexity is a totally natural method of analysis. The real story here was later clarified by Grohe and others as, for example, [Gr01a,Gr01b,Gr02][22]

One key step that remained was to show that the k-Step Halting Problem was $W[1]$-complete. We finally figured the $W[1]$-completeness in the paper [CCDF96] with Liming and Jianer, I presented to the Sacks Conference in MIT in 1993. Whilst circuits were a nice basis and were likely a good enough basis for an intractability theory, the k-step halting problem was so traditional that it completed the picture of $W[1]$ as a gold standard for hardness. It is unfortunate that we don't know the situation for $M[1]$ and the acceptance problem for Turing machines of size $k \log n$.

I think the final really fundamental paper was [ADF95] which began with trying to populate $W[P]$ and also have a decent parameterized treatment of space. We also included some connections with subexponential time, anticipating later developments by Impagliazzo, Paturi and Zane [IPZ01] by half a decade.

[21] Mike Langston is an amazing study in this area. From Robertson-Seymour outer space algorithms to concrete biological computations on supercomputers!

[22] I remember being contacted by Martin by e-mail in the mid-90's. He had worked in bounded-variable logics and canonization. Perhaps because of this he became interested in parameterized complexity. The original investigation of bounded variable logics was in Vardi [**?**]. That paper shows that if you bound the number of queries you get tractability.

Immediately when you consider space, you find that the nondeterministic guessing in the proof that QBFSAT is $PSPACE$ complete is far from parametric. What to do? The idea was to use QBFSAT parameterized as the basis for those combinatorial problems usually found to be $PSPACE$-complete. I am not sure that we have a nice treatment of space yet.

Other early authors who picked up the methodology were H. Kaplan, R. Shamir and R. E. Tarjan, [KST94] P. Goldberg, M. Golumbic, H. Kaplan, and R. Shamir [GGKS95], Leizhen Cai [LeC96], and the nice thesis of Bazgan [Baz95], who was the first to connect FPT with approximation.

Later the work penetrated India through Venkatesh Raman, who had independently constructed a parametric reduction for a version of DOMINATING SET and then stumbled on to our papers. He e-mailed Mike and was enthused by Mike's response. Also in the mid-90's, Phokion Kolaitis suggested to Martin Grohe to have a look at our papers.

There were all of Mike's students who were around at the time, and working on aspects and applications of parameterized complexity. Mike Hallett, Mike Dinneen, Ulrike Stege, Patricia Evans, Todd Wareham, Marco Cesati and others.

I am not completely sure why the work did not penetrate as quickly as it might have. The charitable view is that the basic framework was already there, and many of the basic questions were solved, at least those that were accessible. I recall when I first heard of NP-completeness from John Stillwell at Monash University in the late 1970's it was felt that there were "too many" NP-completeness proofs. Perhaps the same held for $W[1]$ and FPT, when the real initiative in the early years was finding all kinds of novel applications of the framework in areas like biology, VLSI, linguistics, pattern matching, robot motion planning and the like. At the same time, the small but growing community began to see the simple practical FPT methods being easily codable and widely applicable. *Mr Feasible* was really important. Perhaps this is why the *applications people* took up the ideas.

Unfortunately, Mike and I think there were some casualties because of the lack of penetration into the computational complexity community. This meant that work in the area could be difficult to publish. We believe that a few young people working in the area in its infancy had trouble getting positions. I know that some well-respected authors would not put "parameterized complexity" in the title of their papers if they wanted them to be published in certain conferences/journals *even though what they were working on was parameterized complexity.* Fortunately this situation has all changed in the last decade.

I visited Mike a couple of more times and he visited me in Wellington two more times also, once with Ulrike. We worked on the material on coding theory (appearing later as [DFVW99],) the structural question about the W^*-hierarchy ([DFT96,DF98]) where the parameter gives the depth of the circuits as part of the input[23], the mission statement [DFS98], and of course the book [DF99].

[23] Notably it is still open if $W^*[t] = W[t]$ for $t \geq 3$.

8 Epilogue

By about 1994-1995, the basic papers were done and we had clear paths to develop. The insight that we could have this extended conversation with a problem parametrically, and the development of the distinctive tools such as those in Mr Feasible and his son, but also later ones like Colour Coding, crown reductions, iterative compression have all enriched the subject. We planned to include all the basic material around at the time on implicit parameters like treewidth, and particularly the work of Bodlaender and his co-authors, such as Bodlaender [Bod93,Bod96].

By the time the book [DF99] was published, I felt somewhat bruised. I was ready for an affair with another siren to obsess over and fell into working in Algorithmic Randomness culminating in yet another book (after having vowed never to write one again) [DH10][24]. Actually, Kolmogorov complexity is a reasonable parameter to look at in graph theory. It would seem that graphs of high Kolmogorov complexity ought to have some kind of 0-1 law for their algorithmic behaviour. This has not been explored.

Whilst I have kept a running interest in our child's development, serving on PC's, reading new papers and the most excellent books of Niedermeier [Nie06], and Flum and Grohe [FG06], writing one or two papers a year, particularly with new developments like online parameterization, parameterized inapproximability, $M[1]$ and the kernel lower bound project, Mike has really been the beating heart of the subject, spreading the word.

For me the speed and depth of the mathematical developments of the 2010 *WorKer* meeting in Leiden was kind of scary. I had been finishing the randomness book, and looked away from the subject for a year, and arrived to find that some amazingly dramatic progress had occurred. Mainly through the activities of a number of very talented young people. Maybe I am just getting old.

On the other hand, as a community maybe we should take Mr Feasible as a lesson, and not get too obsessed with the beauty of complicated mathematics, so as to lose sight of the *practicality* of what we are trying to do.

It was incredibly fun and rewarding to be involved in all those papers long ago. I like to think the idea of parameterized complexity will be of lasting value to practical computation. Looking at all the young and clever people at the Leiden conference I think the subject is now in excellent hands.

And to finish "Congratulations Mike!"

[24] By a strange twist of fate, the motivating question for my working in algorithmic randomness was a Ladner-style density question for another degree structure about the "degree of randomness" called Solovay reducibility, as I articulated in the preface to [DH10].

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