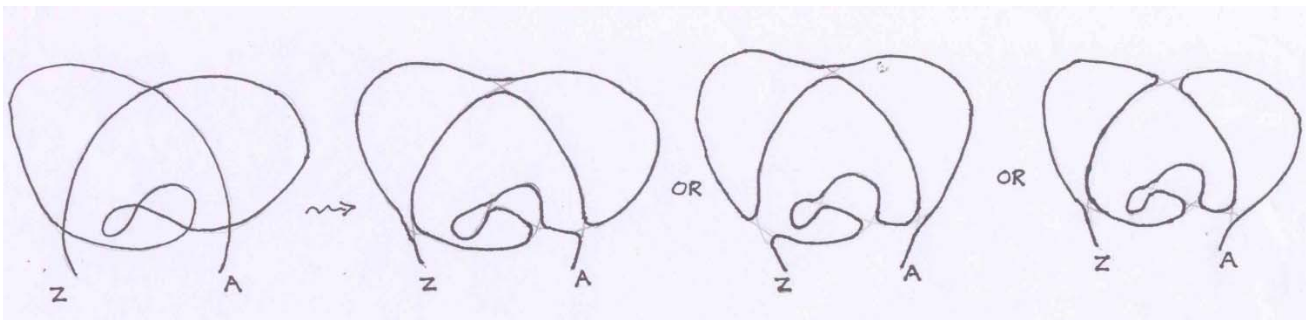
Switches and Fingers

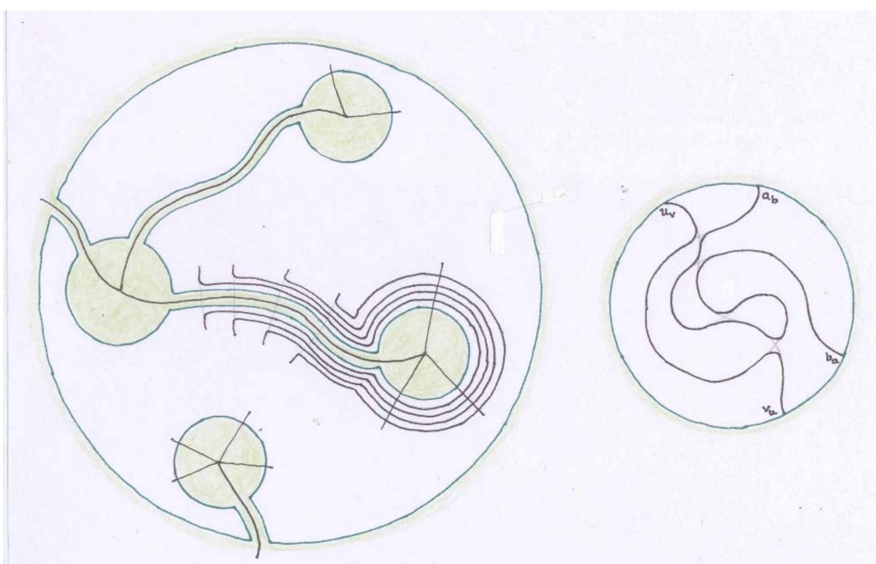
When can a given immersion f, of the space X of a finite connected simplicial complex K, into the twice dimensional sphere, be improved to an embedding ? In the text we'll assume dim(K) = 1.

(ੲ) *An immersed arc is arbitrarily close to an embedded arc.* We needn't hop into a third dimension, we can avoid any self-cut we meet -- say the one that we meet first when we start from A -- by *switching* tracks very close to it in such a way that the in-between journey gets reversed. *q.e.d.* So arbitrarily close to any immersed K we have another, whose edges have no self-cuts, but the remaining cuts are the same. However these infinitesimally close embedded arcs are *not unique* :-



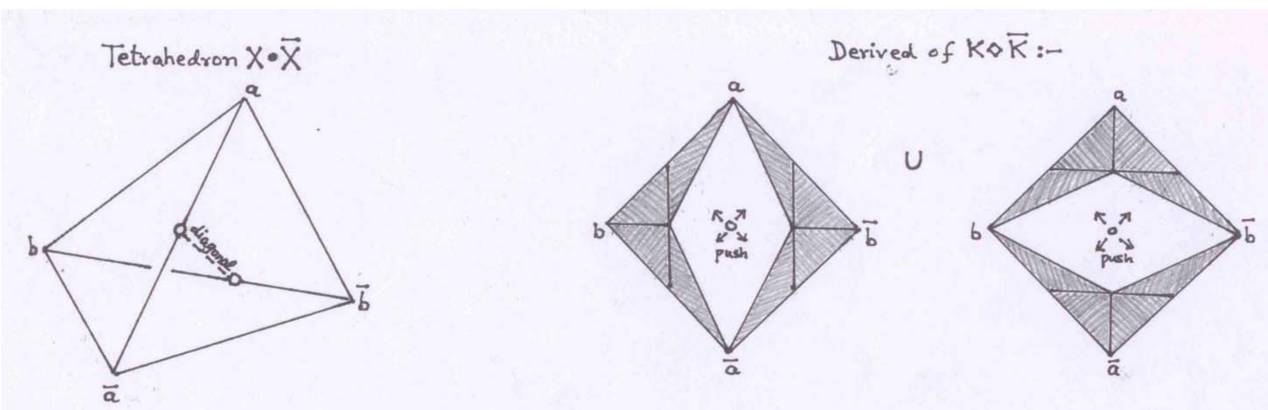
The immersed K is good in small disjoint disks around its vertices, we'll denote by v_w and w_v the points in which any immersed edge cuts the boundaries of these disks around its vertices v and w. Without changing the immersion in these disjoint pools of goodness we'll now link them up by thin channels to obtain a single topological disk. This construction depends on the choice of a vertex -- which we'll deem to be the point at infinity of our extended plane -- and more importantly the choice of a *maximal tree* T of K.

(ਸ) *Using fingers pointing away from the vertex at infinity we can successively clear the edges of* T *to obtain a new immersed* K *with no cuts in a thin closed tubular neighbourhood of this embedded tree.*



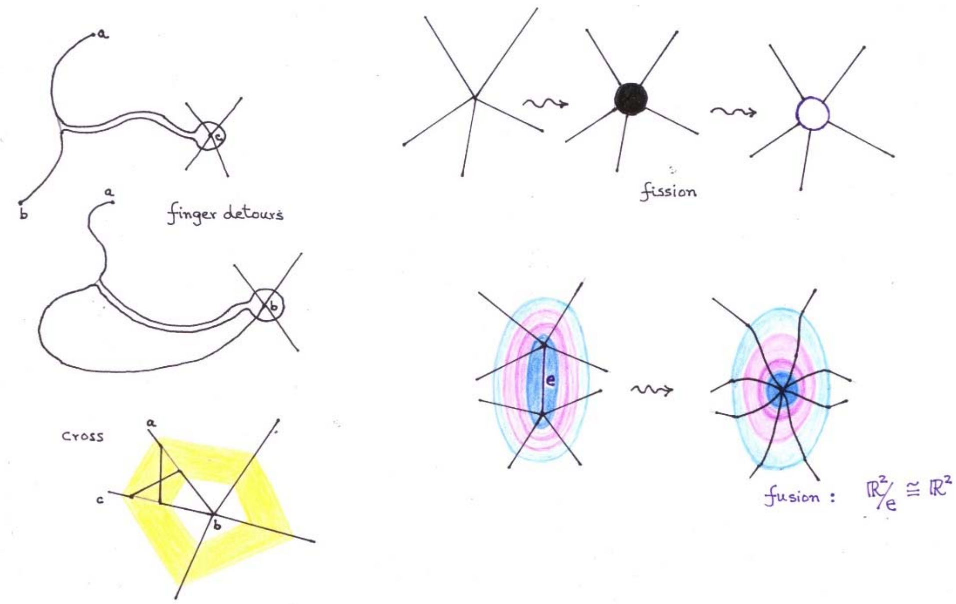
That is, starting with an edge incident to the vertex at infinity we keep on enlarging a cleared subtree, by first removing the self-cuts of the edge that we intend to clear, marking a thin channel along this arc till the pool around its next vertex, and putting on the cutting edges a nest of finger detours, one for each cut, that go along this channel and around the next pool as shown in the figure above. *q.e.d.* We note *each cut was cleared at the expense of that edge making a new cut on each of the other spokes of the next vertex.* So the new immersed K may have after removing self-cuts many cuts but they are all in the complementary open ball between arcs -- the parts of the $b_1(X)$ immersed edges {v,w} of K outside the closed tubular neighbourhood of T -- that join disjoint pairs of ends $\{v_w, w_v\}$ on its bounding circle. So, *arbitrarily close is yet another immersed* K *which has at most one cut between any pair of vertex disjoint edges from the* $b_1(X)$ *not in* T. For, the first *two cuts* with $v_w w_v$ as we travel along $a_b b_a$ can be avoided by *double switching* tracks near them so as to make the in-between journey on the other track, and continuing thus we'll be left with either one or no cut, depending on whether *the number of cuts is odd or even,* which happens iff *their ends do or do not alternate* on the circle. Further, if the two edges share a vertex, say v = a, then the remaining cut can also be avoided by switching tracks near it in such a way that as we travel from w via the common vertex to b the in-between journey is reversed. Also note that, *these* $2b_1(X)$ *ends and their cyclic order are determined by the given immersion and the choice of* T. This choice is unimportant and *we obtain an embedded K if any pair of distinct edges have an even number of cuts under the given immersion,* for this stronger property is preserved when we clear an edge.

Accordingly we delete from $K{\bullet}\bar{K}$ those simplices $\mathbf{s} \cup \bar{\mathbf{t}}$ for which $\mathbf{s} = \mathbf{t}$, or geometrically, all which meet the diagonal of the join $X{\bullet}\bar{X}$ (this is the union of segments $x\bar{y}$, the midpoints of the segments $x\bar{x}$ joining each point to its mirror image form its diagonal Δ). This ***bigger deleted join*** $K\lozenge\bar{K}$ is not a subcomplex, as against the usual deleted join $K{*}\bar{K}$, obtained by deleting from $K{\bullet}\bar{K}$ all simplices whose closure meets the diagonal, or combinatorially, all simplices $\mathbf{s} \cup \bar{\mathbf{t}}$ for which $\mathbf{s} \cap \mathbf{t}$ is nonempty. Its cohomology is that of its barycentric derived, for the case of just one edge this simplicial complex looks like this :-



So any $\{a,b,\bar{b}\}$ of $K\lozenge\bar{K}$ is realized as that shaded *bent triangle* with two true and one bent arm $b\bar{b}$ via its barycentre, and any $\{a,b,\bar{b},\bar{c}\}$ of $K\lozenge\bar{K}$, being the cone over its barycentre of two true and two bent triangles, as a *cell with five facets,* these four and a fifth *skew facet* between the bent triangles. We'll leave $K{*}\bar{K}$ as it is and shall use this, much smaller than its derived, *cell complex* to compute the cohomology of $K\lozenge\bar{K}$.

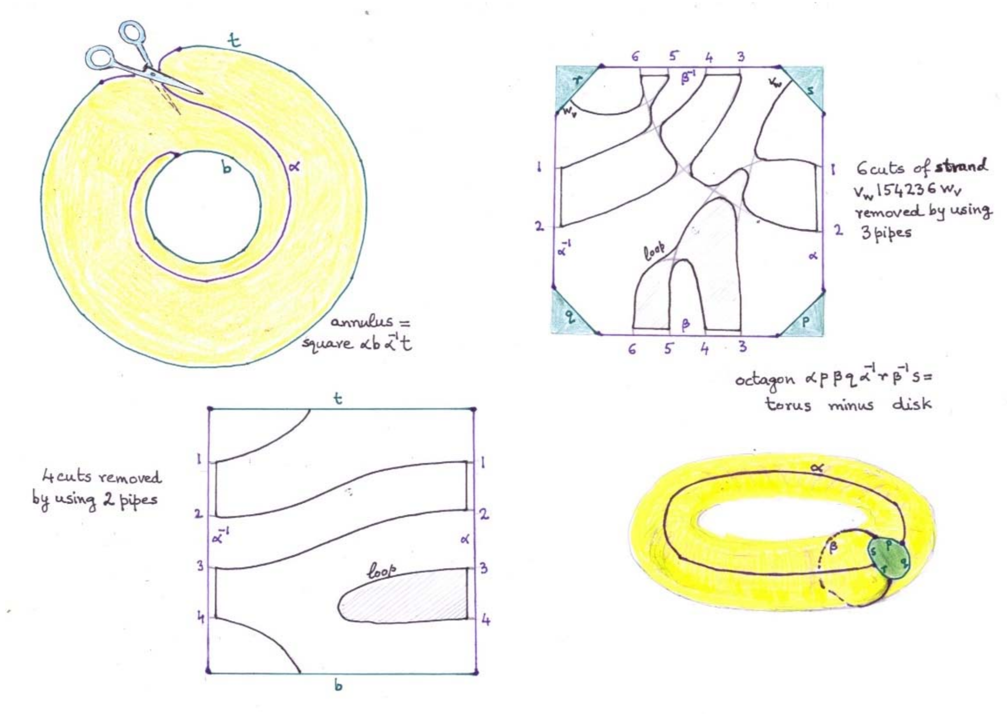
(Ʊ) The *symmetric cohomology class* $\vartheta(K)$ of the cochain of $K\lozenge\bar{K}$ which assigns to each 3-cell $\mathbf{s} \cup \bar{\mathbf{t}}$ the number of cuts mod 2 between the distinct edges **s** and **t** under an immersion of K identifies with an invariant $\vartheta(X)$ of the space X; furthermore, *this characteristic class* $\vartheta(X)$ *vanishes if and only if* X *is planar.*



$\vartheta(K) = 0$ means the cochain of the immersion is a sum of coboundaries of cochains nonzero mod 2 only on a 2-cell and its reflection. So we seek moves which add such coboundaries, making the moves in that sum would then give us an immersion with all pairs of distinct edges cutting an even number of times, and this we know how to modify to an embedding. *A thin finger detour of an edge around a vertex* adds the coboundary of a mirror pair of triangles, these being bent iff the edge has that vertex. The coboundary of a pair of skew facets is nonzero only on a pair of 3-cells $\{a,b,\bar{b},\bar{c}\}$ and $\{\bar{a},\bar{b},b,c\}$ and can be added by making {a,b} and {b,c} *cross* once again near b if they are *cyclically adjacent* around this common vertex, but – ***exercise*** – when they are not, the immersion cannot be modified in that annulus around b in such a way that there is an odd number of new crossings only between this pair of edges. Anyway, we are done if no vertex has more than three edges, and we have such an arbitrarily close immersed K_f : *blow up each vertex to a black hole and discard its interior.* We recall that the topological invariance of $\vartheta(K)$ uses the *biggest deleted join*, viz., $X \bullet \bar{X}$ minus diagonal, the main points being that *its symmetric homotopy type is the same as that of the smaller deleted joins,* and that using singular cochains *there is a definition of ϑ for any space with a fixed point free reflection.* Those blow-ups of vertices didn't change the symmetric homotopy type, and restriction gives us the characteristic class of a symmetric subspace, so $\vartheta(X) = 0$ implies $\vartheta(X_f) = 0$. Hence we can modify the immersed K_f to an embedded K_f a fusion of whose vertices back into those from which they had been created -- say by collapsing its totally new edges one by one to points -- then gives us an embedded K. *q.e.d.*

More notes

(ट) Solution of exercise. Choose a strand which has an even number of cuts with all others and make a scissors-cut along it to change the annulus into *a square with vertical sides identified* to this strand. We assert that *the number of cuts between any two strands is odd iff their ends alternate* on its perimeter. This is obvious if the chosen strand has no cuts at all. Otherwise its first two cuts, then the next two, etc., with any other strand can be cleared by using on the latter *thin pipes* -- see figure -- noting that the new cuts that this produces with another strand are even in number. After this piping we *discard the loops* that may have been produced, noting that these bound disjoint 2-cells in the interior of the square, so any other strand cuts them an even number of times. Then we can clear the cuts of the chosen strand with yet another strand using still thinner pipes, etc. The assertion was therefore valid. It implies that if two strands cut an odd number of times, a strand whose entry point is situated between theirs on the top side of the square shall cut one of them an odd number of times, irrespective of where its exit point is situated on the bottom side of the square. *q.e.d.*

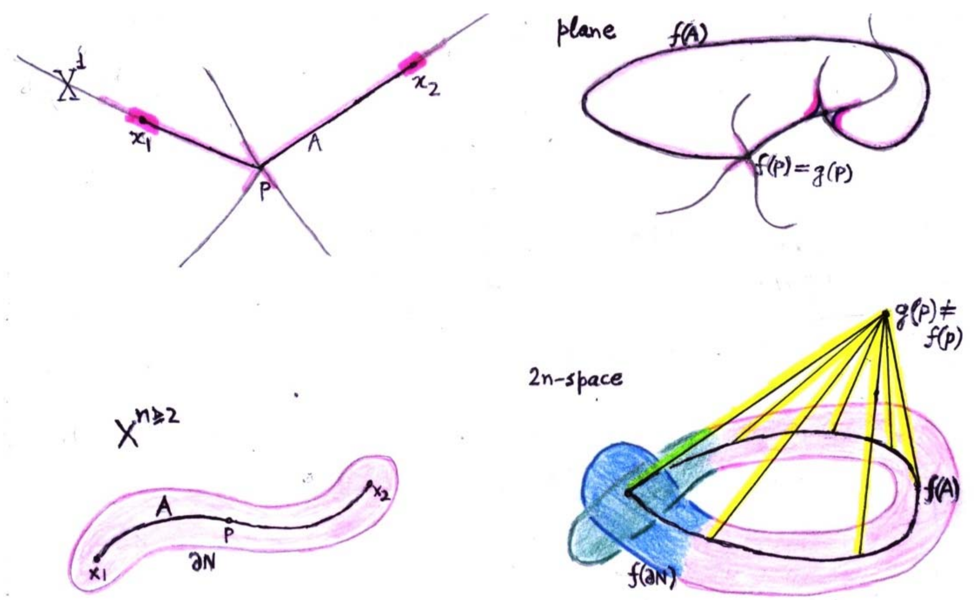


(ਖ) More generally consider strands on *any surface* with disjoint pairs of ends on its *boundary circles* such that scissors-cuts along some chosen strands turns the surface into *a polygon with some pairs of identified sides*, and the remaining strands cut each of these chosen strands an even number of times. Then too piping works and the same assertion is valid : *any two strands* $v_w w_v$ *and* $a_b b_a$ *cut an odd number of times iff their ends alternate on the perimeter*. Thus the exercise is not a dead end, its proof suggests a similar theory periodic with respect to groups like those defining the tilings {p,3} of *"213, 16A" and Mathematics* (2010).

(ਗ) To understand some natural questions that have arisen in the sequels to *Plain Geometry & Relativity* (2013), I'm trying to (re)learn some old constructions. This paper is a part of this remembering process as was its predecessor ਉਂਗਲੀਆਂ ਅਤੇ ਟਾਈਲਾਂ (July 2015) where fingers and trivalent tilings {p,3} were used to bound the diameter of disjoint strands with prescribed disjoint pairs of ends. More about how my thoughts returned to foliations and so Thurston I plan to put soon in an end note to the next and now much delayed installment of *PG&R*. Also, a translation of the previous paper should hopefully be on my website well before the American Mathematical Society publishes one, but that story of Sullivan's about Thurston has just appeared in his own words in the November 2015 Notices of this learned body, the 'senior dynamicists in the front row' of that memorable December 1971 seminar being presumably Smale and Shub.

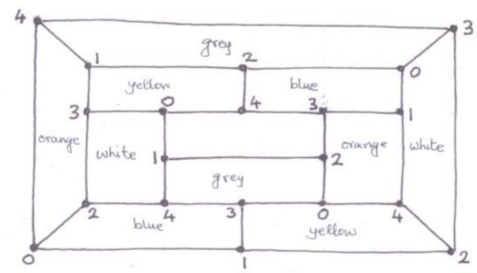
(ਘ) That cartesian string whirring in his head had to hop into a third dimension to avoid its tail, but the *philosophe* of *From Bina's Garden to Khovanov's Homology* (2012) is himself *sans une queue* : from Figure 1 you can figure out three *courbes de Jordan* that he can use in his *jardin* by switching tracks. If dim(K) = n the natural habitat of K is at least 2n+1 dimensional and how we see this *knot* in a spherical microscope – as in the sequel *The Joys of Forgetting* (2012) for dim(K) = 1 – is an immersion f of X^n in 2n-space. This suggests polynomials and homologies for n-knots in (2n+1)-space using 'n-dimensional switching' :-

(ड) *We can remove a cut* $f(x_1) = f(x_2)$ *if* x_1 *and* x_2 *can be joined by an arc* A *passing through at most one singular point of* X^n *and* f(A) *is a simple closed curve of* 2n-*space*. Such an A has a regular neighbourhood N which is a p.l. cone p•∂N of its boundary over an interior point, we'll change f in N to an embedding g. If n = 1 we can switch from f to g only near x_1 and x_2 such that g(N) = g(p)•f(∂N), and this shall be our guide again. If 2n > 2 + n for any g.p. choice g(p) in 2n-space – see *Embedding and unknotting of some polyhedra* (1987) which also precises the p.l. topology (of fine but finite and full simplicial subcompexes and maps between such subspaces which are simplicial) we have been implicitly assuming – the 2-dimensional cone D = g(p)•f(A) meets the n-dimensional f(X) only in f(A) (the 3 vertices of a 2-simplex of D plus the at most n+1 of a simplex of f(X) disjoint from f(A) being affinely independent). A regular neighbourhood of this disk D is a 2n-ball meeting f(X) only in f(∂N), and we define g by coning the latter away from f(X) over g(p).



(ढ) A polyhedron X^n stratifies naturally into objects, viz. manifolds, that arise from motion per notes 23 and 25* in *More mirror relativity* (2014). If *any two nonsingular points of* X^n *can be joined by an arc passing through at most one singular point* then for n ≠ 2 we can switch as above from any immersion f of X^n in our 2n-dimensional garden to an embedding. *For* n = 2 *switching probably works with an infinite iteration that gives a topological embedding,* but may fail under the lipschitz or relativistic constraint c < ∞. The cone g(p)•f(A) has no self-cuts (the ≤ 5 vertices of any 2 triangles are independent in 4-space) but this 2-disk D can cut f(X) away from f(A), so switching gives an immersion with possibly even more cuts. The set of all immersions g that can be made from f by a finite repetition of this construction may not contain any embedding. If disjoint arcs of the above type can be chosen for disjoint pairs of nonsingular points *a p.l. isotopy of* X^2 *in its natural habitat* makes the maximum distance δ(g) between the pairs of double points small. Using the previous paper the diameters of these disjoint arcs can also be kept small, so there should be an infinite sequence g_i which is both cauchy with respect to sup norm and such that $\delta(g_i)$ approaches zero: its pointwise limit φ gives a topological embedding of such a ***'manifold'*** X^2 in 4-space. *(*Note 25 of PG&R* was in fact in *More mirror relativity continued* (2015), so here I'd switched for the last 5 days of 2015 to typing its end note 27.)

(ਛ) Such an iteration echoes the *nowhere lipschitz arcs* in Riesz and sz-Nagy (1967), a friend of mine from long ago as I told you in *Straight to Mecca* (2013). Also, an interesting example of a `manifold' X^2 we have met before when we had identified the opposite faces of a dodecahedron as follows in``*213, 16A" and Mathematics* (2010), on whose pages 10 and 11 is given ample justification why the ensuing closed 3-manifold P^3 is verily a *Miss Universe!* The 2-skeleton of this cell complex has 5 vertices, 10 edges and 6 pentagonal faces; each edge is incident to three pentagons, so its space P^2 is not a pseudomanifold; but any two pentagons share one -- in fact two -- edges, so it satisfies the hypotheses of (ਚ).



(ਜ) Its appearance is deceptive, this 2010 paper has in its mere 37 pages concise but full -- and imho beautiful -- proofs of many diverse results from across mathematics … but when it came to P^3 I had to content myself with just stating results … and frustratingly enough I still don't know why P^3 and indeed more generally *any homology 3-sphere embeds topologically in 4-space.* Yes, I know all the standard references -- the long and very detailed papers and now even a few books devoted to this yoga -- but to the best of my knowledge no one has mastered this material on his or her own, you must also talk to a guru. My wiring precludes this, so I'm going to indulge off and on in speculation like that iteration in (ਚ) above; hopefully, after all the wrong turns and blunders have been found and corrected, this shall show me the light …

(ਝ) An iteration works for n = 1 too and the badness can be confined to a finite subset, for example, *any p.l. knot can be `unknotted' in* 3-*space if we are allowed to tighten it all the way towards a point,* the limiting jordan curve observed in the spherical microscope is then piecewise linear off the image of this point. No fusion is occuring here, any other point escapes the tightening noose (by the way in (ਙ) above also we can avoid fusion by coning over the midpoint of e instead of collapsing it). But the ever tighter turns involved in tightening in a finite time show that the speed is not bounded : *this `unknotting' is not relativistic,* and how far any such observed limit is away from being lipschitz in time at this singularity seems to depend on the complexity of the knot, but I don't know if this measure of knottedness has been used ?

(ਞ) For n = 2 things are different : *any two p.l. embeddings of a `manifold'* $X^2$ *in* 5-*space are equivalent up to a piecewise linear isotopy*, see *Embedding and unknotting of some polyhedra* (1987). So maybe more than just (ਚ) is true and *any `manifold'* X^2 *p.l. embeds in* 4-*space?* Of course smooth surfaces do p.l. embed in 4-space, and these embeddings are made using their well known classification. For more examples I'll again invoke that impenetrable yoga mentioned in (ਜ) : in analogy with knots above the wildness in those mystery embeddings can be confined to a point : *there is a p.l. embedding in* 4-*space of any homology* 3-*sphere minus a point.* Therefore *not only* P^2 *but the* 2-*skeleton of any cellular homology* 3-*sphere embeds piecewise linearly in* 4-*space* … but such striking statements, in and of themselves, only increase that need in me to know more, and are but a poor substitute for a real understanding, of being able to `see' what is going on …

(ट) The beauty that I behold in mathematics is not 'cold and austere', it lies in its patterns of thought, its proofs : they become 'mine' only after considerable effort, even pain, but its obverse is pleasure, an active sensation that beats skin-deep beauty any day ! There is to me a clear distinction between mathematics and the business of mathematics, even between doing mathematics and talking mathematics. Also, I'm aware that what works for me to comprehend—minimization of symbols and a compulsion to abbreviate (maybe because my brain does not have enough storage capacity : so long blog posts full of symbols are an automatic turn off, and I shudder to think that if by some mischance I had been a student of such a mentor I would have learnt nothing at all despite his obvious good intentions)—makes me harder to comprehend by those used to slowing down only when notation becomes denser. Again, clearly everything is so very dependent on the flow of time in a life, and for mathematics as a whole, the life of our species. This dependence has never been, and never shall be anything remotely like that clichéd : 'steady progress'. Personally, I often completely forget what I just did a few days ago, or bring it up all distorted, missing this time that key point, but occasionally this recycling in time—which I feel manifests the very biology of thinking—does make a click : a connection is made and understanding obtained. There are papers dashed off 'just for the record' that have managed to illuminate me, and others much more detailed and very carefully written, that have left me, even after I have checked all their lemmas and formulas, none the wiser. There are a few precious names in my mind, that I know at once from past experience, whose writings I'll understand, and others, writing on the same topic in the same style – say less or more formally – that I'll get very little from. As Gromov points out, often something 'trivial' to the author, or even that subculture, never gets put down in so many words, and without this an outsider – which I almost always am – has no chance of divining what is going on. Far as I am concerned what I can't understand is not proved ! Which is of course pretty silly of me, considering my limitations, but what can I do, that's the way I am (here, by the way, I do except all that which I did understand, but have now forgotten, even that which I feel I would understand, but don't have the time for). So for a mind like mine – not 'deep' at all, and maybe therefore also allergic to the misuse of this word as a euphemism – and fixated on finding simple, some would say over-simple, ways of seeing for itself 'well-known' theorems, most of what is already out there and considered 'solidly established' is in fact no different than IUTT !

(ठ) As an example of 'recycling in time' consider this conjecture : *if a simplicial complex* K^n *embeds in* 2n-*space the number of its* n-*simplices is less than a constant* C(n) *times its* (n-1)-*simplices*. Though I'd posed this conjecture at many places starting 1980 or so, I had completely forgotten, till a few weeks ago, that it is also there in *Unknotting and colouring of polyhedra* (1988). Besides it is shown there that, *the bound holds if* K^n *occurs in* its natural habitat (2n+1)-*space in a unique way up to p.l. isotopy*. As I type this I see how akin it is, at least superfically, to what was proved by Kalai, viz., *the bound also holds if* K^n *occurs in* (2n+1)-*space on the boundary of a convex polytope,* in his beautiful paper, *The diameters of graphs of convex polytopes and f-vector theory* (1991). Surely we two must have noticed this analogy when we worked together on this problem in 1994 but I don't remember (maybe my old notebook will help) ? What I do remember off-hand is I had tried to understand that - at once simple and enigmatic - method he had used in this paper … and if you have imbibed *Note 19 of PG&R* (2014) you would know why I start musing … shifting is interpretable in terms of generic p.l. cartesian motions … there is some geometrico-combinatorial lipschitz lower bound … like $E = mc^2$ … implied by the relativistic restriction $c < \infty$ (dreams like this now and then also come true and keep us hooked) ? On the other hand … continues this association of ideas … maybe this c dependence blows up in the classical limit and so the conjecture is false for … *simplicial complexes* K^2 *which embed topologically but not piecewise linearly in 4-space* ? I don't know if that yoga has produced such examples too … anyway lots of K^n do knot piecewise linearly in (2n+1)-space and using these maybe one can show falsity even under p.l. embeddability ? The unplanned new cycle in time is done, I'm now back on track typing what I had planned to get to at once, viz. that, just a few days ago a nagging thought had arisen again : *do the 2-skeletons of simplicial homology 3-spheres, which all embed in 4-space, already cook the conjecture ?* Answer : *this at least is not so,* and nothing was needed on my part to eventually see this ! I had simply forgotten what I myself had proved in *On coloring manifolds* (1981) – see also its sequel *Heawood inequalities* (1987) – viz., *if* K^n *embeds in a pseudomanifold* X^{n+1} *there is a similar bound* C *depending only on the betti number* $b_n(X^{n+1};Z_2)$. If the conjecture is true a like homological inductive argument going down the skeleta of a simplicial 2n-sphere, and not using shifting at all, might do the job, but I had then not been able to make much headway thus …

(ਡ) As another example here is a formula, $\pi_i(\mathit{Arithmetic}) = 0 \ \forall\ i \geq 1$, from a letter from Gromov in 1988, for which he had congratulated me ! For a minute or so I was sure he was confusing me with someone else till I recalled *Formal theories are acyclic* (1985). The proofs that we humans try to make, or make out, are all in an undefined sense of this word, cf. page 12 of *"213, 16A" and Mathematics* (2010). In a part of mathematics called logic words like theory, statement and proof are given exact definitions to pose well-defined questions about them. Perhaps because of this, there is definitely more than the usual stuffiness to be seen in this subculture, as evidenced by constant highfalutin talk about foundations of mathematics, etc. Also, at least to my untrained eye, this part of our garden had always appeared a bit shabby. Why on earth do these fellows not think of formal proofs as paths in a space, and instead of being content with Gödel's theorem, which says it is disconnected, look also at its higher connectivities ? Which had prompted this whimsy, pointing out that, with one set of natural but liberal definitions one gets acyclicity, but also, in its last paragraph, that with more pragmatic definitions one will get non-trivial homology and homotopy. I don't know if Gromov's frequent allusions to this paper to guests at his *institut* contributed in any way, but since then, things have improved vastly, and a number of people, notably Voevodsky, are engaged in this beautification of logic (however I'm not too excited about automatic proofs, my fear being we might outsource, along with our pain, also our pleasure to machines). However I do know, since just a few days, that the accidental discovery in 1990 in his manuscripts of a scratched-out problem, rather imaginatively dubbed *Hilbert's twenty-fourth problem* -- see the American Mathematical Monthly of 2003 -- has played a no small part in spurring on this HoTT topic ! Hilbert too had asked for relationships between proofs, a criterion for simplicity of proof, etc. Proves once again, methinks, that there is great wisdom in that old saying : great men think alike !

(ਢ) Lighten up ! is another bit of great wisdom. Also, it is a no-brainer that a human learns and thinks far better in its mother tongue than in computerese. My mother tongue is not only beautiful it is very rich, its lexicon may soon contain that of this language as a strict subset ! For example, the ਪੰਜਾਬੀ ਟ੍ਰਿਬਿਊਨ of today, February 23, 2016, has an op-ed, ਨਿਊਜ਼ ਚੈਨਲਾਂ ਨੂੰ 'ਮੀਡੀਆ ਟਰਾਇਲ' ਤੋਂ ਪਰਹੇਜ਼ ਕਰਨ ਦੀ ਲੋੜ, and a news item, ਸਮਾਰਟ ਸਿਟੀ ਪ੍ਰਾਜੈਕਟ ਦਾ ਪੈਸਾ ਰਾਖਵਾਂ ਹੋਵੇਗਾ. Writing ਉਂਗਲੀਆਂ ਅਤੇ ਟਾਈਲਾਂ (2015) -- once the math was in place -- was a breeze, but the onerous translation I'd kept on postponing -- hoping someone would do it for me -- for six months. So out of laziness I've relapsed in this continuation, but retained the 35 main characters of our pretty alphabet, in order ੳ ਅ ੲ ਸ ਹ ਕ ਖ ਗ ਘ ਙ ਚ ਛ ਜ ਝ ਞ ਟ ਠ ਡ ਢ ਣ ਤ ਥ ਦ ਧ ਨ ਪ ਫ ਬ ਭ ਮ ਯ ਰ ਲ ਵ ੜ, to label these notes. Who knows ! you too may find this added stock useful whenever you feel the paucity of the alphabets of the classical and current european languages for your notational needs.

(ਣ) Even *formal texts become prettier if we complete each* ( ) *to a circle,* which had set me musing in *Tweaking* (2013) -- I just retweaked it a bit to remove some slips -- on *a relativistic logic whose statements are* *written using such circles drawn on* -- not a plane sheet of paper, but on one which after some now unknown identifications of its boundary edges has become -- *a closed surface.* This use of homology in logic is very different from that in (उ) -- and probably still not hot ! -- but tries to capture the dependence of lay reasoning on differing points of view. Though *all circles on a surface of genus* g *organize following Thurston into a sphere of dimension* 6g-7 *which bounds its Teichmüller space* it seems now – see Note 7 of *Tweaking* – that Mochizuki is using only aristotlean logic in IUTT. However it appears he is using some $c < \infty$ deformations of addition – multiplication stays put, indeed cayley distance loses the archimedean restriction, so from the whole numbers we now go naturally to those fairy-tale fields of Hensel – which relativistic understanding enables him in the classical limit $c = \infty$ to prove that relationship between addition and multiplication ? *The feeling that there is a proof of the four colour problem via surfaces of positive genus has been in the air for a long time …* Heawood himself, after finding that gap – now filled by using electronic machines – in Kempe's proof, had shown that *the chromatic number of a* K^1 *embedding in a surface* X^2 *with* $b_1 > 0$ *is bounded by a nice expression* $H(b_1)$ *such that* $H(0) = 4$. This bound $H(b_1)$ is valid also for any pseudomanifold X^2 with $b_1 > 0$, see *On coloring manifolds* (1981). The hope roughly is that after appropriate changes in definitions this bound generalizes to numerous discrete group quotients B^2 / Γ of the 2-disk -- even many which are not Hausdorff, so we are now in fact doing Connes' noncommutative geometry, the real number $b_1 > 0$ being a von Neumann dimension -- so many that we would be able to justify the above plugging-in of $b_1 = 0$ by some limiting procedure ? When we recall that the Jones' polynomials of knots -- now understood via chromatic polynomials, so a nice paper of Beraha comes to mind -- also arose from von Neumann algebras, and Khovanov's homology which refines them also involves disjoint circles just like relativistic logic, the feeling begins to develop that the off-beat author of *Laws of Form* (1972) may not have been too far off the (since then) beaten track, also there are some pointers in this book as to how the truth values of statements on closed surfaces ought to be complex numbers.

(उ) I recall that the -- trivial but crucial -- first paragraph of *Plain Geometry & Relativity* (2013), which had set this ball rolling some time ago, had not one but two numbers : its radius c and its dimension n, which were both assumed finite for pragmatic reasons. In *Tweaking* (2013) there is also something about *equivalence classes, under evolution in the cartesian soup, of its compact shapes*. We would like to equip these classes with the binary operations induced by disjoint union and cartesian product, but for the latter we need $n = \infty$, so we had for a while worked in this paper in the contractible but *roomy* space R^∞ of infinite sequences having only finitely many nonzero terms. (Note how this is in marked contrast to adding and multiplying equivalence classes of segments having the same cayley length, to which we had alluded in the last note, when it is addition which requires us to go to the classical limit $c = \infty$.) Limiting ourselves to everything smooth, we had then recalled the fabulous discovery of Pontryagin that, *despite all this roominess, there exist closed smooth manifolds, for example* CP^2, *which are immortal, they never appeared out of, and shall never vanish into the soup* ! More formally this ring is called the *intrinsic* – intrinsic because we side-stepped embedding constraints by giving ourselves so much room – *homology of a point* or the *oriented cobordism ring* Ω. Much is known about this and other similar rings defined assuming smoothness, but off-hand I don't remember exactly how much is known about analogous *lipschitz cobordism rings* which are more basic from our point of view. Things become a lot harder if we remove roominess, and work with a fixed $n < \infty$ – this may be roughly the TQFT of Atiyah et al that I'm speaking of now ? – so in the rest of this paper I had descended to crass simplicity, that is to $n = 2$, when finite disjoint unions of circles are the only compact closed manifolds, and had dispensed with their orientation too. This had revealed *an amusing parallel of Pontryagin's homology with aristotlean logic à la Spencer-Brown, only now the immortality of one circle is postulated*, that is, we forbid capping it with a 2-

disk. So, is this basic urge that we logical beings have—the need to separate what is true and everlasting from that which is false and ephemeral—is this merely an echo in our brain of things actually out there in the external world which are immortal, or is this ‘external world’ a fiction which came into being with that cleaving of The One into the duality in which we the living live ? Sufficiently dramatic, I think, for me to take a little break here ! Besides, Note 8 of *Tweaking* has reminded me once again of something that I should not postpone any longer, but after attending to this, I do plan to resume these, or else the parallel *PG&R* notes. So I’ll close this installment now after the following hurried bibliographical remarks.

(ਥ) *Nothing can be more natural than wanting to classify the immortal shapes in the cartesian soup!* So, no wonder, we are still fascinated by the problems posed by Pontryagin about ‘his’ characteristic classes and numbers, and the new ones raised by their partial solutions. *This story began probably in 1927 or so when this blind teenager discovered and proved the famous criterion for planarity of a graph,* which is often attributed only to Kuratowski (1930). It would be great if this manuscript of Pontryagin is extant, but since his mother alone was writing for him then, it may be there is not much to search in ? Anyway, his later definitions of characteristic classes of smooth manifolds were via *immersion cycles*, so it is likely that for graphs as well he started with a planar immersion and then tried to improve it ? Also he was indeed working under Alexandrov’s supervision with *linking and intersection numbers* only, on a recent higher dimensional generalization of the Jordan Curve Theorem that had been given by Alexander using these very tools. This project of his finally culminated in a sweeping and elegant theorem : the ith integral homology of a compact subspace of an n-sphere is isomorphic to the (n-i-1)th cohomology of its complement with coefficients in *the topological dual of the group of integers* Z, i.e., the multiplicative group of all complex numbers of size one ! Alas, textbooks tend to favour the complicated formulas using Tor and Ext, which to my mind is a great pity, for calculations are definitely not the end all of mathematics. But coming back to the late 1920’s there is a note in Kuratowski’s paper which tells us he had heard of Pontryagin’s work via Alexandrov. Anyway he himself goes in his paper after a bigger fish : *characterize subspaces of the plane that are orbits of motions!* That is, those which are continuous images of the closed interval, because he is working in the classical context $c = \infty$. He solved this problem partially, but comfortably more than what was required for graph planarity. I don’t know the present status of this physical problem, also whether there is a characterization of planar subspaces which arise as lipschitz images of the interval, i.e., as orbits under the relativistic constraint $c < \infty$? It seems to me that any solution of the graph planarity problem which is also going to shed some light on the still open problem of characterizing polyhedra X^2 which embed topologically in 4-space – a homotopy theoretic criterion like the one used in a 1960’s paper by Harris may be both necessary and sufficient ? – cannot be purely graph theoretic (such algorithmic proofs of this planarity criterion were found starting 1960 or so). In *A one-dimensional Whitney trick and Kuratowski’s graph planarity criterion* (1991) a certain move was assumed to change only the parity of cuts between two adjacent edges, however, as in (ਹ) above, their cyclical adjacency was needed. Incidentally, exercise (ਟ) which shows no modification of the stated kind in that annulus shall work, also seems related to a result of Pontryagin that I stumbled on about six weeks after solving it : it is called *Pontryagin’s cycle removal theorem.* For more about the years I was playing around a lot in this part of our garden see also my other papers of that time; you can then come up to speed, as I myself am trying to, by reading the beautiful paper of Melhikov, *The van Kampen obstruction and its relatives* (2009).