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# Riemannian Geomery

**VS:** What topic do you want to start with?

**me:** Riemannian Geometry

**RB:** What is the definition of the cut locus? (*This was not on my syllabus*)

**me:** You mean the cut locus of a point $p \in M$?

**RB:** Yes

**me:** So if you have a geodesic $\gamma(t) = \exp_p(tv)$ from $p$ then it's minimizing for small $t$ but at some point $t_*$ it stops being and that point is the cut locus. (You do this for all $v$).

**RB:** What are some reasons why the geodesic would stop being minimizing?

**me:** So two possibilities are that

(1) $\gamma(t_*)$ is a conjugate point.

(2) there is another minimizing geodesic from $p$ to $\gamma(t_*)$.

**RB:** Prove that these are the only reasons.

**me:** So I think we need $M$ to be complete for that.

**me:** So let's assume that $t_*$ is the time where $\gamma$ intersects the cut locus and $t_0 > t_*$ is the first time where (1) or (2) happens. Now assume (2) happens at $t_0$. By definition of the cut locus we know that there's a curve to $\gamma\left(\frac{t_0+t_*}{2}\right)$ shorter that $\gamma$ so the other geodesic to $\gamma(t_0)$ is not minimizing. (*The logic of what I said isn't really correct but no one called me out.*)
Now let's assume (1) happens at $t_0$... So we know that $\exp$ is a diffeomorphism up to $t_0$ and hence also in a neighborhood of $\gamma|_{[0,t_0]}$. *I proceeded to be very confused for maybe 20-30 minutes.*
*The problem was that contradiction is not the correct strategy here. In particular it doesn't make sense to think about "what happens at $t_0$". By definition for every point after $t_*$ there will be a shorter geodesic connecting it to $p$, but the difficulty is in showing that there will be another geodesic to $\gamma(t_*)$ or a geodesic variation of $\gamma$ that doesn't move at $t_*$ up to first order.*)

**AG:** *at some point asks me what the definition of a conjugate point is.*

**me:** It's a point $\gamma(t)$ so that there exists a nonzero Jacobi field along $\gamma$ vanishing at $0$ and $t$.
*After some more confusion:*

**AG:** You have a shorter geodesic from $p$ to every $t > t_*$, not only to a single $t_0$.

**me:** So we can get a sequence $\gamma_k$ of geodesics whose endpoints approach $\gamma(t_*)$. But I don't see why they would converge in any way

**AG:** Are you sure?

**me:** Oh, so their tangent vectors $v_k$ at $p$ are going to have an accumulation point $v_\infty$. And $t \mapsto \exp_p(v_\infty t)$ is going to be a geodesic to $\gamma(t_*)$ of the same length as $\gamma$ so we get (2).

**RB:** But only if that geodesic is different from $\gamma$.

**me:** Right, so that must be where (1) comes in. We need to somehow use these $v_k$ to create a geodesic variation of $\gamma$. (*I got somewhat confused around here again because I was still thinking in terms of my first strategy*)

**me:** If we can get a continuous family $v(s), s \to 0$ (instead of just $v_k$) then we can use the variation $f(s,t) = \exp_p(tv(s))$. The endpoint of this is going to move like $\partial_s f(0,t_*) = d_{t_*v(0)} \exp_p(t_* v'(0))$. So we just need $v'(0) = 0$... Maybe we can replace $v(s)$ by $\tilde{v}(s) = v(s^2)$ to ensure this?

**RB:** I think you're off the right track. Suppose that there is no conjugate point up to time $t_*$. Then what can you say about the exponential map?

**me:** So we know that it's a diffeomorphism up to $\gamma(t_*)$. And so it's also a diffeomorphism onto a neighborhood of $\gamma([0,t_*])$. But then if we have those $\gamma_k$ "converging" to $\gamma$ for $k \gg 1$ they must be contained inside this neighborhood. But then $\gamma_k$ and $\gamma|_{[0,t_k]}$ (for the appropriate $t_k$) will be different geodesics to the same point inside this neighborhood which contradicts the injectivity of exp. (*the cleaner way of stating this would have been to say that we conclude $\gamma_k = \gamma_{[0,t_k]}$ in the case where the geodesics "converge" to $\gamma$.*)

**RB:** That sounds about right. Now consider $\mathbb{CP}^2$ with the Fubini-Study metric. What is the cut locus in this case?

**me:** Ok let's see. So the Fubini-Study inner product $\langle u, v\rangle$ on $\mathbb{CP}^2$ by lifting $u$ and $v$ to $S^5$, perpendicular to the $S^1$ fiber and then taking the inner product of those lifts in $S^5$. My guess would be that the geodesics of $\mathbb{CP}^2$ are just the images of the geodesics in $S^5$ that are perpendicular to the fibers.

**RB:** That is correct.

**me:** So the cut locus of $S^5$ would be an equatorial $S^4$. But then you probably need to quotient this by $S^1$ somehow... So the $S^4$ is $S^5 \cap \mathbf{0} \times \mathbb{R}^5$. But $\mathbf{0} \times \mathbb{R}^5$ is not $S^1$ invariant...

**AG:** Maybe you can try a simpler problem first. For example $\mathbb{CP}^1$.

**me:** Right. So $\mathbb{CP}^1$ is diffeomorphic to $S^2$ and the Fubini-Study metric will just be the round metric on $S^2$. So the cut locus of say the north pole is just the equator.

**RB:** The equator?

**me:** Yes. Oh wait no, the south pole. Or in terms of $\mathbb{CP}^1$, the point at infinity. So then for $\mathbb{CP}^2$ is should be the $\mathbb{CP}^1$ at infinity. The preimage of that in $S^5$ is going to be $S^5 \cap \mathbf{0} \times \mathbb{C}^2$. There this explicit formula for the distance between two point in $\mathbb{CP}^n$. Maybe you could use that?

**RB:** That would work but we don't need that. $S^4$ will be be the cut locus if you take all geodesics starting at $[1:0:0]$ but you only need the ones that are perpendicular to the fiber.

**me:** Oh right. *I proceeded to sketch an (I think incorrect) argument that tried to show that the geodesics perpendicular to the fiber intersect $S^4$ in an integral submanifold of the foliation $\{v \in T_pS^5 \mid iv \in T_pS^5\}$. This would have to be the $S^5 \cap \mathbf{0} \times \mathbb{C}^2$.*

**RB:** What you're doing is too complicated. It's easy to see what the geodesics on $S^5$ perpendicular to the fibers look like.

**me:** So they would be like $(\cos t, u \sin t)$ for some $u \in \mathbb{C}^2$. So therefore they reach the cut locus at $t = \pi/2$

**RB:** Why?

**me:** Because at $t = \pi/2$ the geodesic $(\cos t, -u \sin t)$ is at the antipodal point so in $\mathbb{CP}^2$ there's two minimizing geodesics to $[0:u]$.

**VS:** It's been an hour already so maybe we should move on to the next topic.
*We decide to take a 3 minute break and that Lie Theory will be the next topic*

# Lie Theory

**VS:** Since we were talking about Riemannian geometry before: If you have a left-invariant metric on a Lie group then what can you say about its geometric properties?

**me:** Uh I don't know?

**VS:** Is it going to be complete.

**me:** Yes. Because if you look at the identity then every geodesic will exist for some minimum time $\varepsilon$. So if $\gamma$ is your geodesic then you know that at $\gamma(\varepsilon/2)$ the geodesic in direction $\gamma'(\varepsilon/2)$ will also exist for time $\varepsilon$ (here you use that there is an isometry of $G$ sending $e$ to $\gamma(\varepsilon/2)$). So the geodesic $\gamma$ at the identity exists for at least time $3\varepsilon/2$. And then you repeat this argument to get that the exponential map at the identity is defined on the whole tangent space.

**VS:** *asks a question about the exponential map for* $G = \mathrm{SL}_2\mathbb{R}$

**me:** Do you mean the Riemannian or the Lie exponential map? They don't agree in general for a left-invariant metric.

**RB:** Can you use Riemannian geometry to show that they don't agree for $\mathrm{SL}_2\mathbb{R}$?

**me:** *thinks for a bit.* Yes, because since $\mathrm{SL}_2\mathbb{R}$ with a left-invariant metric is complete the Riemannian exponential map is surjective. But the Lie exponential map on $\mathrm{SL}_2\mathbb{R}$ is not surjective.

**VS:** Can you explain why it is not surjective?

**me:** (*I had prepped this question the day before*). Of course. The matrix $B = \left(\begin{smallmatrix} -1 & 1 \\ 0 & -1 \end{smallmatrix}\right)$ is not the matrix exponential of an antisymmetric matrix. If we have $A \in \mathfrak{sl}_2\mathbb{R}$ and write its Jordan decomposition as $A = D + N$ with $D$ diagonalizable and $N$ nilpotent then $\exp(A) = \exp(D)\exp(N)$. If this is supposed to be $B$ then the eigenvalues of $A$ need to be $\pm ik\pi$ for $k$ odd. I particular they are distinct, so $N = 0$. But then $\exp(A)$ is diagonalizable which gives a contradiction.

**VS:** Since you mentioned $\mathfrak{sl}_2\mathbb{R}$, what are all Lie groups that have this as its Lie algebra.

**me:** $\mathrm{SL}_2\mathbb{R}$ has fundamental group $\mathbb{Z}$

**VS:** Why?

**me:** Because by the polar decomposition it is diffeomorphic to $SO(2)\times\{\mathrm{symmetric positive definit matrices}\}$ and the second factor is diffemorphic to $\mathbb{R}^n$.

So we have a universal cover $\widetilde{\mathrm{SL}_2\mathbb{R}}$ and then you can take its quotient by subgroups of $\mathbb{Z}$. And the smallest of those quotients will be $\mathrm{SL}_2\mathbb{R}$ and the other ones... They might all be isomorphic to $\mathrm{SL}_2\mathbb{R}$ ?

**AG:** What is the center of $\mathrm{SL}_2\mathbb{R}$?

**me:** It's $\pm\mathrm{Id}$. Right, there is also $\mathrm{PSL}_2\mathbb{R}$. So then I guess that means that the fundamental group of $\mathrm{PSL}_2\mathbb{R}$ is a product or semidirect product of $\mathbb{Z}/2$ and $\mathbb{Z}$? (*At this point I got confused for a while again.*)

**AG:** So if you have a discrete normal subgroup of a Lie groups then what can you say about it?

**me:** It must be contained in the center. *I gave the standard argument for why.*

**AG:** So you said that if a simply connected Lie group $G_0$ has the same Lie algebra as $G$ then it must be a quotient of $G$.

**me:** Yes because the map of Lie algebras $\mathfrak{g} \to \mathfrak{g}_0$ can be integrated to a map $\tilde{G} \to G_0$ from the universal cover of $G$ and this map is an isomorphism.

*With some more help* ($What is the fundamental group of $S_1/(\mathbb{Z}/2)$*) I realized that* $\pi_1\mathrm{PSL}_2\mathbb{R} = \mathbb{Z}$.

So $Z(\tilde{\mathrm{SL}_2\mathbb{R}}) = \mathbb{Z}$.

**AG:** If you quotient $\tilde{\mathrm{SL}_2\mathbb{R}}$ by a subgroup $k\mathbb{Z}$ of its center, what is the center of that quotient going to be?

**me:** $\mathbb{Z}/k$. So all of the quotients are nonisomorphic Lie groups.

**VS:** Let's maybe move on to some algebra. Can you tell us something about the Killing form and its properties?

**me:** *Defines the Killing form for an action $\mathfrak{g} \curvearrowright V$ and for the adjoint actions.* Its symmetric and bilinear.

**VS:** What else can you say about it?

**me:** I don't know what you mean.

**VS:** *some kind of hint*

**me:** Oh it's $\mathrm{ad}$-invariant, so $B(\mathrm{ad}(z)x, y) + B(x, \mathrm{ad}(z)y) = 0$.

**VS:** There is a theorem about solvable Lie algebras that is important in the structure theory.

**me:** Do you mean Engel's theorem?

**VS:** Can you state it?

**me:** *I first gave a wrong statement that was a mix of Engel's and Lie's theorem. When point this out.*

**VS:** Can you give a correct statement of either Engel or Lie?

**me:** If $\mathfrak{g}$ is solvable and $\mathfrak{g} \curvearrowright V$ is a finite representation then $\mathfrak{g}$ acts by upper triangular matrices in some basis. (*Lie's theorem*)

**VS:** Does this work for any field?

**me:** No. I think this only works over the complex numbers (or any algebraically closed field of characteristic zero).

**VS:** So what can you say about the Killing form of a (complex) solvable Lie algebra?

**me:** I must always be zero I think? *I try to prove this but of course it doesn't work?*

**VS:** What is the definition of solvability?

**me:** You have $\mathfrak{g}' = [\mathfrak{g}, \mathfrak{g}]$, $\mathfrak{g}'' = [\mathfrak{g}', \mathfrak{g}']$ etc. and $\mathfrak{g}$ is solvable if this eventually becomes zero.
Oh so actually what you can say is that $B([\mathfrak{g}, \mathfrak{g}], \mathfrak{g}) = 0$.
Because $B([x, y], z) = \mathrm{tr}(\mathrm{ad}([x, y])\,\mathrm{ad}(z)) = \mathrm{tr}(\mathrm{ad}(x)\,\mathrm{ad}(y)\,\mathrm{ad}(z) - \mathrm{ad}(y)\,\mathrm{ad}(x)\,\mathrm{ad}(z))$. *I try to commute the maps in the trace to make it vanish but it doesn't work*
Ah you can use that $\mathrm{ad}([x, y])$ is strictly upper triangular by Lie's theorem and then so is $\mathrm{ad}([x, y])\,\mathrm{ad}(z)$ hence its trace vanishes.

**VS:** If you have a compact Lie group, what can you say about its Killing form?

**me:** $B_{\mathfrak{g}} < 0$.

**VS:** Is it always definite?

**me:** Right, $B_{\mathfrak{g}} \leq 0$.

**VS:** Why?

**me:** *gives the standard argument with a $G$-invariant metric and using that $\mathrm{ad}(x)$ has imaginary eigenvalues*

**SS:** What about compact *complex* Lie groups?

**me:** They are always abelian, because $\mathrm{Ad} : G \to \mathrm{GL}(\mathfrak{g})$ goes from something compact into something affine, hence its image is a point. So $\mathrm{Ad}$ is trivial and since it's the differential of the conjugation map we conclude that $G$ is abelian.

**RB:** Can you show that $G$ compact if $B_{\mathfrak{g}} < 0$.

**me:** Let's see. Assume $G$ were noncompact. Then there should be a 1-parameter subgroup that is isomorphic to $\mathbb{R}$ ... (*I think I had an argument for this but I don't remember it anymore.*)
So we have $x \in \mathfrak{g}$ with $\exp(tx) \neq e$ for $t \neq 0$...

**RB:** I was thinking of a Riemannian geometry proof of this but this is fine too.

**me:** Oh that sounds like a good idea. So if we extend $-B_\mathfrak{g}$ left-invariantly it will be a bi-invariant metric on $G$. And for bi-invariant metrics we know that the Levi-Civita connection is $\nabla_X Y = \frac{1}{2}[X,Y]$ on left-invariant vector fields.
So the curvature will be - *I spent some time deriving this* - $R(X,Y)Z = \frac{1}{4}[[X,Y],Z]$. And then we have that

$$0 \geq B_\mathfrak{g}(x,y) = \operatorname{tr}(\operatorname{ad}(x)\operatorname{ad}(y)) = \operatorname{tr}(z \mapsto [x,[y,z]]) = \operatorname{tr}(x \mapsto -4R(Y,Z)X)$$

And the last thing is $4\mathrm{Ric}$ or $-4\mathrm{Ric}$, I'm not sure about the sign

**RB:** I think it's $+4\mathrm{Ric}$.

**me:** OK so the we get that $\mathrm{Ric} \geq 0$, so $\mathrm{Ric} \geq \varepsilon$ for some $\varepsilon > 0$ at the identity and then this bound will also hold at every other point. And then by Bonnet-Myers we know that $G$ must be compact because its diameter is finite.

**VS:** Does anyone want to ask more questions? *No.* Ok then let's take a 3 minute break and continue with the next topic.

# Complex geometry

**SS:** So what is you favorite theorem in complex geometry, so far?

**me:** I'd say the Hodge theorem.

**SS:** What's the statement?

**me:** $\mathcal{A}^k(X) = \operatorname{Im}\bar\partial \oplus \operatorname{Im}\bar\partial^* \oplus \mathcal{H}^{p,q}$ is an orthogonal direct sum.

**SS:** What is $X$ here?

**me:** $(X,h)$ is a hermitian manifold.
Oh and it needs to be compact of course.

**SS:** What are some consequences of the Hodge theorem?

**me:** It implies that $H^{p,q}(X) \cong \mathcal{H}^{p,q}(X)$. So it gives you Serre duality, and finiteness of dolbeault cohomology groups because the kernel of $\Delta_{\bar\partial}$ needs to be finite.
And if $X$ is Kähler then we have $\Delta_{\bar\partial} = \frac{1}{2}\Delta = \Delta_\partial$ which gives you the Hodge decomposition $H^k(X) \cong \oplus_{p+q=k} H^{p,q}(X)$.

**SS:** You mentioned Kähler manifolds. Does the Hodge theorem impose any cohomological restrictions on Kähler manifolds?

**me:** Yes the odd Betti numbers need to be even because the Hodge diamond needs to be symmetric.o

**SS:** What would be the Hodge diamond of $\mathbb{CP}^2$ for example.

**me:** *I write it down.* You know it has to be this because the Betti numbers of $\mathbb{CP}^2$ are $1,0,1,0,1$.

**SS:** Can you give an example of a non-Kähler manifold?

**me:** The Hopf surface would be one. It's defined as $M = \mathbb{C}^2/\mathbb{Z}$ where the $\mathbb{Z}$-action is generated by $x \mapsto 2x$. Then $M$ is diffeomorphic to $S^1 \times S^3$ so its Betti numbers are $1,1,0,1,1$ which means it can's be Kähler.

**SS:** What does this $\mathbb{Z}$ action to at $(0,0)$?

**me:** Oh right you need to remove the origin from $\mathbb{C}^2$ in the definition.

**SS:** Now consider you have a compact complex surface $X$ with a holomorphic submersion $\pi$ to a compact curve $S$. If the fibers of $\pi$ are biholomorphic to $\mathbb{P}^1$ can you show that $X$ is projective?

**me:** So you could show that there is a positive line bundle on $X$. Or you could use that $S$ embeds into some $\mathbb{P}^N$. Then if you find a holomorphic map $f : X \to \mathbb{P}^M$ such that $f \times \pi : X \to \mathbb{P}^M$ is an embedding you would be done.

**SS:** How do you know that $S$ is projective?

**me:** All curves are algebraic by the Kodaira embedding theorem? Do you want me to elaborate on this?

**SS:** Yes.

**me:** So we know that $S$ is Kähler and $H^2(S;R) \cong \mathbb{R}$ generated by $\omega$. So if we take any point $p \in S$ then $\int_S c_1(\mathcal{O}(p)) = \deg[p] = 1 > 0$ hence $\mathcal{O}(p)$ is positive.

**SS:** Why does the integral being positive mean that the bundle admits a positive metric? Oh you said that $H^2 = \mathbb{R}$. OK, and how is the embedding map defined?

**me:** *Defines the Kodaira embedding map.*

**SS:** OK continue.

**me:** So I think you need to find a relatively ample line bundle $L$ on $X/S$. Then you can pull back a positive line bundle $T$ from $S$ and $L \otimes T$ will be ample.

**SS:** Can you define relatively ample?

**me:** It means that the restriction of $L$ to every fiber is ample.

**SS:** Why would that tensor product be ample?

**me:** Because the curvature $F_{L\otimes\pi^*T} \cong F_L + \pi^* F_T$ will be positive in vertical directions because of the $F_L$ and positive in horizontal directions because of the $\pi^* F_T$. At least if you replace $T$ by a high enough power.

**SS:** If you know that the restriction of $L$ to every fiber has a positively curved metric how do you know that there is a single metric on $L$ that has positiver vertical curvature?

**me:** *I had missed this gap and needed to ask for clarification until I realized what the question was.* So you can take a positively curved metric on the fiber of a point $p$. Then for a small neighborhood $U$ of $p$, $\pi^{-1}(U)$ is a product so we can take the product metric. These metrics can be glued together with a partition of Unity on $S$ that you pull back to $X$. This does not change the horizontal curvature because $\pi^*\varphi$ is constant in vertical directions.

**SS:** So how do you find a relatively ample line bundle?

**me:** I guess you can try $\mathcal{O}([X_p])$ where $X_p = \pi^{-1}(p)$. *I compute the restriction of this to $X_p$ but it's the trivial bundle $\mathcal{O}$.*

**SS:** So what other way is there to get line bundles except for divisors?

**me:** I don't know. I mean if $X$ is supposed to be algebraic then every line bundle must come from a divisor...

**SS:** Is there some kind of interesting line bundle that exists on every complex manifold?

**me:** *thinks for a minute.* Oh, the canonical bundle! *I used the adjunction formula to compute the restriction of $K_X$ to a fiber and get that its $\mathcal{O}(-2)$.* So then $K_X^\vee$ is relatively ample.

**SS:** OK.

*Song then also asked me to show that the Hopf surface is a holomorphic fiber bundle over $\mathbb{P}^1$ whose fibers are elliptic curves. He also asked me to state the general Hirzebruch-Riemann-Roch formula and the special case for surfaces. I managed to do those things without big difficulties.*