28) Feel free to provide additional comments regarding data reproducibility, the publication process and the current academic environment	
1	Academic research is extremely competitive. Unfortunately, this creates an environment where moral principles and scientific rigor are often sacrificed in the pursuit of a 'perfect' story based on spectacular theories. Science is supposed to be a regimented and un-glamorous search for truth, based on observation, logical reasoning, and repetition. Instead, it has devolved into a pursuit of the next big paper that will result in grant funding, or a promotion, or national recognition. In such an environment, a single (often random) observation, that would to a rational mind appear to be a scientific artifact, is transformed into the basis of a whole research program. I have witnessed figure schematics being drawn up before a single experiment has even been attempted. Conclusions are drawn and cemented prior to any reproducible evidence. This leads to a shaky hypothesis quickly transforming into an irrefutable thesis. Experiments must then be conducted to show and back up this thesis (that may have no factual basis whatsoever). Often these types of projects are passed around from person to person until they reach the 'right' individual, and magically the data starts to match the thesis. I refuse to engage in such activities, but many people do not share my scruples. I do not judge these individuals, because they are simply doing what they feel is necessary to succeed or keep their jobs. There is often tremendous pressure from the PI to engage in fabrication, falsification, or misrepresentation. However, it does not make the activity any less repugnant. As a result, I do not believe that publishing in a high impact journal is a measure of one's worth as a scientist. In fact, I am less likely to believe the data published in Nature or Cell, than in a 'low' impact journal like PLoS One. Unfortunately, until the system changes, and no longer equates success with publication in high impact journals, matters will only deteriorate further. I have honestly lost faith in academic science, and believe it is a waste of tax payer and philanthropic money in its current state.
2	Although fabricated/falsified data are common in all fields, the person who is going to repeat the experiments is still critical for reproducibility.
3	As a graduate student leaning towards a more medical/industry career path, I don't believe I feel the same pressure of the 'publish or perish' paradigm. However, based on my experiences with my academic colleagues and mentors, I absolutely believe the pressure of this current paradigm is IMMENSE.
4	As long as cutthroat employment and grant award decisions are based on publications, there will always be a perverse incentive to 'massage' (or outright falsify) data. The system is completely broken, and I plan on leaving the world of academic science largely for this reason.
5	Based on the questions, I don't think this survey was appropriate for my field (neuroscience). No one I know uses cell lines or feels the need to replicate, let alone triplicate findings.
6	Big labs with famous PI can publish lower quality papers in high impact factor journals compared to other groups with less famous PIs. The review process should be always double blinded.
7	Biology in general has most reproducible issues maybe is because a lot of researchers DO REPEAT important results. Some other fields, however, do not have this issues probably because repeating other people's work seems a lack of innovation. I think this data reproducibility problems prevail in all the STEM fields in general because of the pressure to report positive data!
8	Coming from a relatively sheltered and upstanding academic environment, I don't know what the scope of issues are in other universities, or indeed even my own. But it wearies me to think

	that this is a much more severe problem in academia than I am aware of or accept. It is a little encouraging in a twisted way though, because it means that my data and outcomes, no matter how dismal at times, are at least authentic. And my ability to take ownership and responsibility of that as I set up interviews for Post-docs etc is going to be of value to me.
9	Current environment in tough. It feels that there is no place for young scientists anymore.
10	Data fraud is rampant in academic science and is deliberate more often than not. There is no downside under the current system as most studies are not questioned leading to more publications and a 'stronger' CV. Promotions are readily given to those publishing either a high impact paper or many papers irrespective of the validity of the studies. At […], I personally have witnessed at least three promotions along these lines. One of which has led to a 'mentor' that informs trainees that this is the method for advancement. As a final comment, and not progress into a prolonged diatribe directed against the academia, I do not foresee how this problem can be easily remedied because it is so pervasive.
11	Dear Sir/Madam, Ek dum fantastic survey conducted. Zabardasth questions asked. Thank you for the opportunity.
12	Definitely I feel that students and postdocs are put under extreme pressure to provide = produce data which confirm! The PI's hypothesis. There is a lot of fear in the young people. Rarely young people are treated like valuable intellectual creatures, rather more and more often like technicians..... what is quiet upsetting. In my sincere feeling and opinion, there is no proper oversight, no control and no care of PI/manager, if the data is real. I feel that confirmation of PI's hypothesis is the most important task...... and this at the end often does not lead to the biological truth, but mistakes (aware or very often UNAWARE), omission of negative data, and presentation of data which fits...... I did work in the company for some time; there was so much oversight, control and care for a proper data and ZERO FALSIFICATION. More and more I feel that industry and academia are two different worlds.......
13	Ethics programs/reproducibility for students-postdocs are OK. But I feel the people who need it the most are middle-age/senior PIs that tend to have a looser approach to the scientific method (especially the group that has knowledge of how biology works and, thus, either your data supports the hypothesis or you are a wort less piece of sh*t that doesn't even know how to run a WB -words i have heard in my lab at a federal research lab by my +65yr old PI)
14	Given the fact that even nowadays we can still see sub-par quality articles on top-tier journals (i.e. Nature, Science, Cell), I disapproved the requirement of having a first-author publication in top-tier journal for an academic position. We have seen plenty of 'one hit wonder', and prestigious, big name scientists publish in the same top-tier journals despite the fact that some of the claims or results are disputable. Hence, the quality of ones work, in my opinion, should not be evaluate based on the impact factor of a given publication or manuscript. The merit of science, the depth of knowledge, and the value of the significance of ones hypothesis and data should be the key evaluation for future faculty hiring in a long run.
15	Government funded research should be publicly owned and must be freely available to increase scientific discovery. Freely available articles should be the norm and journals should no longer make money off of government-funded research. Impact factors are ridiculous as evidenced by the number of redacted articles and the fact that earth shattering results are rarely published in the highest impact factor journals. There should be no charge to publish an article and an NIH sponsored server should be created to mitigate the inherent problems in the current publication money making scheme.

16	I am grateful for the emphasis on better reporting, raw data publishing, and data reproducability. I wish it was easier to publish reproducability studies instead of being told that the work has already been done, it isn't new, and therefore unnecessary to publish.
17	I am very concerned about reproducibility and the pressures my PI puts on myself and others in the lab to cut corners, particularly in analyses. We use no source control in the lab so analysis scripts are literally handed around on thumb drives with nearly incomprehensible titles. There is no guarantee of version, and much of the code isn't even commented. Because our PI restricts the release of code and data, it is unlikely that mistakes will ever come to light and likely that code will disappear forever.
18	I believe the individual analyzing some data sets would perform it differently from another, and may account for differences in some instances.
19	I do aware publish or perish paradigm, but it doesn't affect my data at all. I can think more to get solid data before publication. I only publish what I observed in my hands repeatedly.
20	I dont know how others handle their data but I very often see that published data are more than often not reproducible or appropriate detailed methods are unavailable. Journals have started asking for more elaborate methods and there are guidelines for them as well. But I dont think the journals take the time to read them and make sure the information provided is enough or conforms to the guidelines. Additionally, I think there should be a Negative data repository/portal, which should be citable and treated as a publication. I think these will be more valuable than the so-called positive data published in high impact factor journals. This will prevent researchers from repeating the same experiments again, save money and on the brighter side get more publications to survive in the field.
21	I don't think the publish or perish impacts the way I publish because I had a wonderful graduate mentor who viewed the need to publish in Science very negatively and pushed me to think big picture and framework. I think this keep my science true but I do worry about getting a job without the major publications...I don't think this survey touched upon that aspect of publishing and how impact factor impacts job placement, tenure, and grants.
22	I have also had difficult experiences working with collaborators who I don't think do experiments correctly. There's no way to report to a journal, 'I'm a coauthor on this manuscript and agree with experiments A, B, C but not X, Y, Z'. It's frustrating that after helping collaborators for so long, I have to either sign a letter to the journal saying I accept all the manuscript's contents, or completely leave my name off the manuscript and not get any credit for the accurate work I have completed.
23	I have been in a lab that PI has totally falsified data to publish. He has pushing me to produce only positive data without reproducibility from his material (cell line, virus etc). Paper got accepted at low impact factor journal but through connection.
24	I have never witnessed someone fabricating data, but sometimes, published results were 'to good to be true' and I was not able to reproduce some of them. Also, I feel that negative results should be included in the manuscript, but often PIs are refrained to do so.
25	I have noticed that many small responsibilities cause PhDs and postdocs spend time on unnecessary jobs. At least in some departments, the services are very limited. For example, each lab has to have their own printers, which means taking care of printer is a job done by PhDs/postdocs. Biohazards are not checked regularly, instead, PhDs and postdocs are responsible for calling in the service providers when necessary. There are not glassware washing services, no autoclaving services. Knowledge-edge talks are not happening that often. Grant application (K-99, etc) workshops have not happened since I have been here. In general, there seems more money is available for staff recreative activities than for things that

	will save a lot of time and distraction from PhDs/Postdocs or will help us focus on the right track. Thanks for the survey.
26	I have the feeling that scientific reports are being pressured into politically correct reports.
27	I have worked as a lab technician, graduate student, and now postdoc for 13 years, and I feel that the current level of pressure on young and early-career scientists who wish to pursue an academic career is higher now than in years past. Salaries for postdocs and graduate student stipends have only increased slightly over the past 10 years, making rapid career advancement desirable. Good job positions in both academia and industry are extremely competitive, and until there is either substantial job growth in the scientific sector or a reduction in PhD trainees there will be continued pressure on graduate students and postdocs to produce high-impact research results to advance their careers. While I think that relatively few would result to outright falsification of data, it would not be surprising to me to find that omission of data or experimental designs that lead to biased data are becoming more routine practices.
28	I often see that people ignore known fundamental limitations of a particular experiment or known off-target effects of particular siRNA sets (or don't even consider to check) when the current experimental results are nice and just go forward.
29	I once had a reviewer remark that while my studies were conducted thoroughly and results reported with great care, and that the paper was well-written, the overall result was not 'exciting' enough to publish. This was about three years of work in a full-length original article, submitted to a journal with an IF<5. I ended up cramming the only 'exciting' results into a supplemental figure of another paper.
30	I strongly support data reproducible in particular publicly funded projects should make their result freely available for other researchers to us. => I have become more sceptic on the review and publication process. My experience show that some reviewers look at the manuscript from their point of view and if that doesn't support their point they tend to be critical. some reviewers don't put the necessary effort to provide valuable comments beside the simple editorial issues. =>Journals ask for authors to suggest reviewers. I understand the necessity but feel it degrade the quality of the review as authors tend to suggest people who they think will provide only positive feedback
31	I tend to be more hesitant than my PI when interpreting the data. Sometimes, my PI has changed my wording for grants and papers that makes the data sound more convincing than it actually is. However, we keep the data as it is, so readers should be able to judge the validity of our statements for themselves.
32	I think data falsification or only showing positive results is common in most large labs. In my lab, my PI facilitates people willing to do this. For example, we have a virus […] that they have amazing functional data for. However, there is no evidence that the virus is working (there is evidence the virus is NOT working) but this paper is under revision at a big journal […]. The sad part is that he is more than aware that these happen in his lab- he just doesn't like people knowing it or believing that he facilitates it.
33	I think one problem is the ease with which larger well known labs are able to publish their data in high impact journals. Reading some of these papers, I wonder how on earth they got past review with so many missing controls, terrible images, etc. I think a double blind review process would serve the community much better. In my opinion.
34	I think sometimes reproducibility is difficult because everyone has been trained differently, and even when following the same protocol some people pipette differently, etc which causes the results to at times not be the same. Sometimes it may be intentional yes, sometimes it may

	be the reagents are bad etc, but I think sometimes it's solely due to variability in methodology and technique.
35	I think that 'cherry-picking' results is a serious problem in research dissemination. I think that many scientists feel the need to publish a lot, and in high profile journals.
36	I think that more and more papers with irreproducible experiments are being published because people are no longer concerned about contributing to science, they are racing to get to it to the best journal. There is so much pressure to produce data that is in line with pre-existing literature because P.I.s do not want to step on other P.I.s' toes. Racking up a high number of papers has become the currency for a good post-doc, or tenured position, not the quality. This is one of the reasons why I have opted out of being a scientist after I finish my PhD. It is not worth my energy or time doing science when your boss is asking for more and has no regard for the quality of the science. There's also too many M.D.'s flooding the bench sciences and they aren't trained to do science, most times. It has been my experience where they lack imagination and continue to do the same type of research without using the available, newer technologies, because they can't adapt to the changing times. Mot of them are now in tenured professor tracks in my program and they have turned it into a'who can publish fastest in medical journal' contests. It is very disheartening and hopefully it will end.
37	I think that overstating or exaggerating the conclusions of a study is one of the most detrimental things to the scientific method (at least in my field), as it results in the funneling of funds away from work that is equally, if not more, valid and thorough. I am lucky to have always worked in groups where care is taken not to exaggerate our findings, regardless of the potential future effects this may have on publicity for our group or our future funding. Unfortunately, in my experience, this seems to be the exception rather than the rule. I would much rather have my name attached to quality research in a lower-impact, more specialized journal than a hastily performed study with conclusions unsupported by the data in a higher impact publication.
38	I think the NIH and journal need to encourage repeating studies by other labs, and not place some much emphasis on novelty. When novel things are published they often die after that and we never hear anymore about them, because they are no longer novel.
39	I would never falsify data, but I often feel that it would make life easier. Oftentimes, I have repeated an experiment enough times that I am confident in the result scientifically, but it doesn't look pretty enough to publish. In those cases, it would be so much easier to photoshop together the nice lanes of all the gels! But I do understand why people wouldn't trust that. I also am not sure I always use statistics correctly. I do always repeat my studies, but sometimes I'm not sure what I can exclude as outliers.
40	If the facilities were you work are not appropriate to fostering animals and they have contamination, your results will variate in comparison with other institutions, also the genotype and phenotype of the animals will make the difference.
41	In addition to the points above, I am aware of incorrect data in a manuscript published by my research group. I have addressed the issue with the corresponding author (my PI) and no action has been taken by either the corresponding author, or the primary author on the manuscript.
42	in cancer biology field. accumulated too many researcher. Funding is less. Top journals' reviewers request too perfect data. Make scientists hide some data which oppose their hypothesis.

43	In general, the pressure to publish often makes us hasten experiments and not be as thorough. As a community, we could benefit from slowing down and repeating our finding more often.
44	In life science research, many a times we get samples/data from a collaborator and there is no way to validate the reproducibility and authenticity of the data and so on. We always hope or assume that what we get is correct and done the right way.
45	In our lab, we always work hard to produce data.
46	In the current research scenario publishing is the only criteria for judging scientific merit and the quality of work or per se science itself is secondary.
47	Incomplete reporting of methodology. Non-specific antibodies. Cutthroat competition without pay.
48	It is necessary to work in a more open, collaborative and multidisciplinary way since the beginning of each study in order to be able to pursue the actual research direction suggested by the results, instead of trying to make the study fit a specific field corresponding to the specific expertise of the PI and the source of the money, and to be able to properly interpret positive and negative data in a way that is not just 'confirming or not the hypothesis'. The discoveries would be much more helpful, translational and fast if the positive, negative and unexpected results would be interpreted with a multidisciplinary approach, also used for grant applications and hypothsis design before beginning the studies.
49	It is often hard to raise concerns about reproducibility and even outright fabrication, especially for early-career scientists. Unfortunately, some PIs just aren't willing to listen to or act on concerns. I have previously worked in a lab where many members were concerned about the behavior of one person in the lab, and although these concerns were brought to the PI's attention on multiple occasions, nothing was done. Years later, a minor inquiry into one figure of a paper led to the discovery that almost all of the data in that paper were highly manipulated or completely fabricated and the paper was retracted. This could have been prevented if earlier reports had been taken more seriously. Perhaps institution-wide anonymous reporting systems (or similar) could help in these situations. While federal whistle-blower laws may legally protect such reporting, it is still a risky and scary prospect for a trainee or junior scientist to really push hard with complaints about this type of research misconduct.
50	It is very difficult to reproduce the data from papers that I am following. Overtime there is some or the other problem, Many a time, I had to device my own methods rather than the published results
51	Journals need to start reporting acceptance rates in addition to impact factor.
52	Junior PIs or less politically connected PIs are at a large disadvantage when publishing. Completely ethical behavior is thought of as naive. While many people uphold the ethical standards in theory, in practice many people are willing to play with statistics or bury data that does not support a hypothesis in order to get papers out. The highly competitive, paper-driven environment of academia has completely squashed curiosity and discovery in favor of fame and notoriety.
53	Many of my troubles reproducing data come from insufficient details when applying a published or internal protocol or checking poorly indexed and described notebooks.
54	Many PhD programs are beginning to require students to produce a paper that is 'accepted' by a peer-reviewed journal before graduating. This adds to a superficial sense of 'publish or perish' to their studies, even before deciding whether to pursue a career in academia.

	Programs with such a requirement should provide a structure that prevents this from becoming a major roadblock to graduation (e.g. by encouraging faculty to provide small projects & opportunities to publish quickly), or remove it entirely.
55	More transparent methodology in manuscripts would be helpful.
56	Most of the researchers persue a paper published in high impact journal such as Nature and Science, which makes them competitive in job markets
57	Much of the problem lies in sloppy work and poor record keeping, as well as not providing details in methods. Just look at method sections for Western blots in many articles where the dilution of the antibodies is not even reported.
58	My lab is historically experiencing that many people leave the lab within 1 year, unless you are trapped by their positions (e.g., he was a graduate student). This was because the people who dominates lab is lunatic (only blames, scream and insult people's skill and personality if we cannot provide the result they want)
59	Needs to be better way of reporting negative data. Definitely stigma given the way 'lay' people sometimes interpret scientific data and results. High pressure to find positive data, if it doesn't fit with hypothesis, why continue to look at it.
60	Negative data and disproved hypotheses are not considered by higher impact journals to be advancing the field and are therefore ignored. The level of extra material requested by reviewers is proportional to impact factor, regardless of relevance. Funding/ journal acceptance is influenced by trends more than robustness or repeat-ability. It is impossible to get a paper accepted with an ambiguous discussion/ conclusion.
61	'Never trust what is published - anywhere, more so if it is published in Cell/Nature/Science. You can only trust the data, if you - working solely to reproduce the experiment - can reproduce the published data.'
62	Not having the mindset to 'publish or perish' has, I believe, been part of the reason I have not been able to succeed in academic science.
63	One issue I have encountered is that negative results are not publishable on their own. I think it is valuable for other scientists to know that a certain hypothesis is not supported so that they can formulate new hypotheses. Additionally, my perception is that publication in a good journal often requires an immense amount of data that in the past would have been published in several manuscripts over time. In general, I think that funding and employment in the life sciences, even outside of academia, is so competitive now that some people feel pressured to cut corners and produce subpar data.
64	One topic that seems challenging is how to allocate resources. One could use resources to do a very strong, reproducible study with large sample sizes, lots of controls, study both males and females but be limited to fewer experiments per manuscript. However, the perception is that this type of approach would lead to a lower impact publication, depending on the outcome. Alternatively, resources could be spread out to do many more experiments to try and get published in high impact journal.
65	People are too rush to pursue the quantity of papers in the 'publish or perish' system.
66	People dont take into account these from the article. 1) Location CA is way different than NY 2) equipment used 3) if you believe in stats, than by chance 5% of all studies are wrong 4) From my experience technology development. I mean when new tech comes out, usually we find previous hypothesis are wrong. This doesnt mean a problem in the work, but just better

	resolution. I truly believe the problem is way blown out of proportion. Yes this happens, but many times, I think it is one of the above reasons.
67	Personally I think publishing papers should not be the main point of judging a researcher. But unfortunately in this world, it has become the main thing.
68	Politics is the most important factor in publishing and pursuing an academic career: I have the feeling that working in a well recognized lab helps more than producing good data, and this makes me think that my future career is not going to be as sparkling as some of my former colleagues with more loose professional ethics but in a more 'prestigious' environment. Nevertheless, I'm not willing to corrupt my professional Ethics in exchange for a faster reward and I promised myself I will be as much unbiased as I can consciously be.
69	Publishing a result that has already been published is almost impossible, even if complementary in a certain extent. And when it is, it is in a low impact factor journal that has no real impact on the scientific community or on the author career. When publishing a result contradictory to a previous report, it is very difficult if the previous report was lead by a 'big boss'. Whereas scientific research should correspond to an effort to describe the reality of things, black and white hypotheses are common and we see too many studies with sexy titles that introduce a bias in how to understand the work. Sometimes, you can even read in the article something contradictory to what says the title, which appears as a marginal results that is not emphasized. Truth is often in the grey zone, but the run towards the sensational study is preventing these kind of studies to come out in big journals. I think the scientific community should redefine the Impact Factor, or at least the consideration of this impact factor for funding or evaluation purposes.
70	publishing is important, but not this level of perishing as such
71	Publishing more may have some short term benefits but in long run having correct data which is reported correctly is important.
72	Reproducibility - It would be my preference that completed data sets always be reported as thoroughly as possible, but I feel like there may be a perception that the slightest inconsistency could be interpreted to invalidate an entire study. Especially for those of us working with animals, but for biologists more generally also, we need to appreciate the immense complexity of the systems with which we work, and that every experimental outcome may not be immediately explainable in full. Publication process - As someone who has been interested in pursing a PI position but also open to other possibilities, my recent experience with the publication process has likely made my decision to explore alternatives. We have a manuscript that was in review at a high-impact journal for nearly 18 months and 3 revision cycles, was substantially improved by experiments performed in response to reviewer critiques (the results of which further confirmed or extended our original conclusions), but was nonetheless rejected. It was then rejected by a lower but still moderate-to-high impact journal in the same publication family without additional review. It seems that the opinion of one reviewer, whose critique relied on publications that at best were cursory to our study and in some instances likely inaccurate or at least over-interpreted, had a disproportionately significant impact on the decision. I can appreciate that my bias will always be in favor of my paper, but this does not change my feelings on this experience.
73	Reproducibility is harder even the figure published in nature is not reproduce at once need to repeat thrice. The paper need to have proper methodology if its published in high if journal. The working environment is very difficult under certain PI. Some of PI think postdoc as labor

	and visit the work bench of postdoc more then 8 times a day. Visiting work bench by PI create mentally pressure. Some PI comment on small things like wasting their money.
74	Reviewer or editor mostly they raise some questions without asking our answer they simply reject. Most of the questions are very simple to answer, even though they reject and nowadays every one running behind the paper for either securing grant or getting some position, no one really doing research, thats why mostly they provide falsified data. I feel our database is fully loaded with bogus data. When ever I tried to reproduce some data in reputed journal related to my experiment I failed to get the results. Eventually funding agencies always force 90 % people to produce bogus data.
75	Reviewers are now asking for more and more staff, some of them are really difficult and sometime even unnecessary to strengthen the main conclusion of the study. The review process is very time consuming in many cases.
76	Reviewing process should be more strict, authors don't know the reviewers' identification (however they can suggest names of the potential reviewers) it should be two-way process i.e. reviewers should not know the author's identification to get unbiased reviews
77	Several PIs I have met do not emphasize reproducibility, even in informal contexts like lab meeting. This is frustrating, since it takes a conscientious scientist like myself three times as long to produce a figure as a competitor. Journals should try to require submission of all three repeats/ full blots/etc. Publication process needs to be abbreviated and blinded (i.e., reviewers should not know author names), and ability to get an academic position should not be defined by ability to publish in a top-tier journal only (since so much luck and politics plays into this).
78	Some labs submit a trashy paper only good enough to get pass the editor so that it at least gets reviewed. Then they focus on addressing the reviewer comments and get the minimal story out. This saves a lot of time and efforts. My lab, on the other, hand wants a good story to be sent out for review- which I only partially agree with. I have noticed a lot of crappy half-baked stories get published even from RO1 funded labs with famous scientists as PIs.
79	Some of the issues you raise are things that I think about a lot. Sometimes, I think that a scientist like me cannot survive in this current academic environment - it's become too much like industrial science. It's for this reason that I am not interested in becoming a principle investigator or pursuing a research career in academia.
80	sometimes one person can repeat the data but in another person's hand is not reproducible. cell lines can get contaminated by carelessness or by ??
81	The current academic environment equates success with the number of high profile publications someone has. Many of the results reported in big journals are difficult to reproduce. In my experience, researchers just give up after failing to reproduce published data citing -'His/Her lab is far more successful (multiple Nature/Science/Cell papers) than mine. They probably have better people and resources.'
82	The current academic environment is all based on publishing papers. It isn't conducive to risk-taking, which is needed for constant breakthroughs. It has also caused many PIs to start treating grad students and post-docs like lab techs, micromanaging every decision and experiment to the point that these students and postdocs are not always receiving the best education. Scientists need to be allowed to mess up experiments, it's how they learn. But with funding so tight, and the pressure to publish so high, PIs don't feel like they can allow their post docs and grad students that much freedom.
83	The current academic system does not support the brightest and smartest, but the lucky ones. Those that were lucky enough to get the paper into Nature, Cell, Science and those ones that have a PI who is influential enough, and persistent enough to call the editor over and over

	again. I have witnessed this in several cases. It is a mafia-like system. Initially, it was shocking to learn from colleagues about all the NCS papers that can't be reproduced or are flawed in so many ways. But I guess it's just the logical consequence of the forces in place. How to change this? By recruiting people and promoting scientists that know what they are doing, not the ones that got lucky. Take the time to evaluate the scientific contribution of a candidate, which can't be measured with one big paper.
84	The fact that our career is tied to publishing in top journals creates such pressure to obtain results and drives individuals to make decisions to falsify data. I have seen a PI push her postdoc to falsify data and threatened her job and visa status if she didn't comply. Once you witness that, it is hard to trust the data in these journals. It is such a toxic environment that most postdocs choose to leave this career track.
85	The high impact journals need to ask a third-party to repeat the main experiments in the manuscript. The reviewer can pick up some easy experiments to repeat like Western blot, qPCR and etc.
86	The issue is poor methodological training and poor understanding of scientific theory by many of the current generation of graduate students and scientists. We have entered a dangerous era where the techniques often used comprise a field of their own (ie mass spec) and the user usually knows very little about the theory or methodology of the technique. This is in stark contrast to previous generations before 'high-tech' techniques, where scientists would understand the theoretical aspect of the problem they were trying to solve, then choosing or developing their own techniques with the tools they had. Currently, for example, we have scientists and students using sequencing cores who can't answer one question about the illumina platform, mush less sanger. (Other 'cores' are applicable here as well.) The problem is not the cost saving services that cores provide or the level of technology we have attained, but the level of training a typical biomedical science graduate student enters school with (not enough, for example psychology). Then they are forced to specialize in a particular small area to obtain their degree, unable to strengthen themselves holistically.
87	The people I've interacted with (mentors, colleagues, reviewers) have all displayed a high standard of scholarship/integrity. I have a high standard for myself. As I said before, I think the key is approaching science as answering questions (whatever the answer may be) rather than trying to prove a hypothesis. I do worry that the current academic environment favors fast/flashy results over good quality research and long-term vision. And thus I do worry whether I'll be able to sustain an academic career.
88	The pressure to publish is enormous, which leads to hurrying in many cases. For example, I performed a triplicate experiment on one day, but ideally I would like to perform the same experiment on another day too to test the intermediate precision of the experiment. However, this is not always possible due to time constraints. Also, my ability to learn new techniques/areas is hampered by constant pressure to publish (because of the job market).
89	The publication sphere is saturated and biased towards established PIs. Funding is hard to obtain and biased again through study sections and academic politics. Too little emphasis on rigor and too much emphasis on new techniques and 'sexy' topics. Little value placed on negative results that should be reported.
90	The reproducibility in my current lab is low. My current mentor is very laid-back and hypothesis driven. The practices inside the lab sometime doesn't follow strict disciplines. I tried to discipline my project as much as I can and report all the results I have.
91	There is an unavoidable direct link between scientific output and individual career which inherently compromises the conduct of research. It is ironic that we have to report 'conflicts of

	interest' when the most relevant conflict (that science is a major or the sole part of our livelihood) is considered not to be a conflict.
92	There is pressure to publish in a small period of time to promote your career. This is very stressful and frustrating due to the fact that scientific experiments majority of the times fail by definition. Scientific careers and funding depends on publication number and our success and promotion is measured by that. Unfortunately, this cycle is also linked to scientists life prospective and salaries are maintained low for a long period of time. Fundamentally, flaws on the system drive scientists to a rally for publishing without double thinking, troubleshooting and fully optimizing. Another thing is that we are afraid reporting negative results. Maybe we should, it will save money from repeating failed experiments/ideas and it might be a way of measuring how much work you perform as a scientist despite of the publication success recorded on pubmed.
93	There needs to be standards for data reproducibility. If a technique is novel or not established, high replicates should be required to show reproducibility in at least first figure. Representative images and data, needs to be standardized. I think we need more journals that support so called 'negative' data. If the NIH cares about how money is spent, we need to stop reinventing the wheel. Meaning, if we know method X, Y, and Z does not work in a specific cell line it must be reported so others may not repeat those same useless experiments. Also, I specifically hear PI's pressuring postdoc's to publish in high tier journals to be successful. Also, some PI's will not allow certain data to be published because it didn't reach the caliber they think it needs to be, even though the data is sound. Some PI's are on their own agenda, want high tier journals, and disregard postdoc work. Sometimes getting a high tier journal is a matter of chance yet, somehow this is treated as some sort of sign about the potential of a postdoc/student. These high pressure environment create mistakes or
94	There needs to be visible consequences administered with enough transparency that closed door disclosures are redundant when a person who is senior and established engages in research misconduct. Otherwise, as a trainee, you keep your head down, do as your told, and let things slide because without tangible consequences for those who do wrong there's no point in raising a red-flag.
95	This survey seems directed almost exclusively toward cell-line assay labs. While the reproducibility of cell-line assays is critical in terms of reproducibility and integrity, so too is downstream computational analysis. More work needs to be done in educating students in how to present research in a self-critical way. I understand that many have the 'publish or perish' paradigm, and so they feel pressured to make groundbreaking papers, instead of focusing on making really solid experimental designs with the appropriate results.
96	Unless we stop the direct effect of high IF publication to directly career of a scientist, nothing can be done. We need much less scientists in biology, slow down, think couple of times, publish less amount but reproducible data. Ultimately, career trajectory directly correlated with the IF of journal drives ir-reproducibility.
97	Until the publish or perish and the regular impact factor philosophy to be promoted it is stopped there will be falsification, fabrication and plagiarism. The egotism, egoism, honor, arrogance, so on is much much more than honesty and truth. Change the rules!
98	Using mice makes it sometimes difficult to repeat experiments with accuracy due to variability.
99	We have to repeat our experiments and try to make our findings reproducible.

100	What about animal research (e.g. genetic drift, use of litter controls, not appropriately separating litters when harem or trio breeding)? What about overstating your results and/or use of standard error of the mean when inappropriate? There are many more probing questions that should be in this questionnaire.
101	When billions of dollars available for research, all kinds of people will flock to it. That makes publish or perish the logical process by which the 'best researchers who publish' get to survive. That means both 'best researchers' as well as 'best publishers', the latter consisting of truthful and deceitful researchers. We must trust in reproducibility to find those people. We must also have a stronger and louder judgement and enforcement mechanism by which we punish the falsifiers and deter future repetition of that behavior.