Teoretska praksa v inovacijsko naravnanem raziskovalnem okolju

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Politični napad na čisto znanost in teoretsko produkcijo ogroža same temelje tako »mehkih« kakor »trdih« znanosti. Kakšne pozicije znanstveniki zavzemajo v odnosu do spreminjajočih se pogojev raziskovanja? Ogledali si bomo epistemološko pozicijo in samorefleksijo v znanstvenih praksah, materialne pogoje raziskovanja (zlasti založništvo in merjenje znanstvenega vpliva), odzive na zunanje zahteve in družbeno pozicioniranje znanosti.

Ključne besede: epistemologija / znanstvena praksa / topična metoda / materialni pogoji raziskovanja / založništvo / merjenje znanstvenega vpliva

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V tem članku bomo poskušali na novo osvetliti trdovratno tematiko morebitnih epistemoloških konvergenc med t. i. »mehkimi« in »trdimi« znanostmi: med humanističnimi in družbenimi vedami na eni strani ter naravoslovnimi vedami na drugi. Odnose med tema tipoma znanosti so doslej običajno razlagali na način razlik med togima sistemoma vednosti, med nomotetičnimi in idiografičnimi znanostmi. Politični napad na »čisto znanost« in teoretsko produkcijo nasploh je, se zdi, ogrozil same temelje obeh vrst znanosti.[1] Spreminja se tudi celotno raziskovalno okolje: od univerz, ki morajo sprejeti korporativno upravljanje in svoje delo preoblikovati v »storitve« za trg, do raziskovalnih institucij, ki morajo upravičevati svoje delo z inovacijami za podjetja. Vse te spremembe naj bi izboljšale globalno konkurenčnost države. Hkrati se pri odločanju o financiranju raziskav in pri zaposlovanju na univerzi uporabljajo pretirano kvantificirana merila pod pretvezo »znanstvene odličnosti«, t. i. mednarodne konkurenčnosti domačih znanstvenikov. Akademsko založništvo, ki je pomemben institucionalni okvir predstavljanja in izmenjave raziskovalnih rezultatov, so prevzele profitno usmerjene mednarodne korporacije, ki postavljajo svoje pogoje za dostop do publikacij. Zunanji pritiski postavljajo niz vprašanj, med drugim vprašanja o epistemološki poziciji in samorefleksiji v znanstvenih praksah, o razumevanju materialnih pogojev raziskovalnega dela,

o odzivih na zunanje pritiske in o družbenem pozicioniranju znanosti. Vsa ta vprašanja so tesno povezana: sposobnost znanstvenikov, da analizirajo materialne pogoje lastnega dela in da zavzamejo stališče do njih, je odvisna tudi od epistemološkega pozicioniranja. Ali če pogledamo z druge strani: če se znanstveniki niso sposobni spoprijeti s pogoji lastnega raziskovanja, katero drugo nalogo bi jim še lahko zaupali?

Proti-epistemologija v družbenih vedah

Izraza »nomotetična« in »idiografična znanost« je leta 1894 vpeljal Wilhelm Windelband v okviru svoje kritike pozitivizma, a sama razprava je veliko starejša in seže vse do Giambattiste Vica in njegove polemike s kartezijanstvom kot kritično metodo Modernih, nasproti katere je Vico postavil topično metodo Starih. Georg Henrik von Wright je opisal nomotetične znanosti kot raziskovanje dogodkov, ki so ponovljivi in predvidljivi; take dogodke lahko za nameček osamimo, z njimi eksperimentiramo in izpeljemo splošne znanstvene zakone. Idiografične vede pa proučujejo minljive dogodke, ki jih lahko pojasnimo le z opisom. Wright (6) je spomnil, da je Georg Simmel zgodovinopisje primerjal z gledališčem in opisal metodo proučevanja preteklih dogodkov kot empatijo. V moderni dobi je ugled nomotetičnega prijema povečal nastanek novih disciplin, ki so si prisvojile raziskovalna področja izvirno idiografičnih znanosti (na ta način je nomotetična sociologija prevzela raziskovalno področje zgodovinopisja). Rastko Močnik (188–191) celo trdi, da so družboslovne vede kompromisna formacija, ki je nastala pod pritiskom galilejevske paradigme na humanistiko. Napetosti med nomotetičnimi in idiografičnimi znanostmi pa izhajajo ne le iz rivalstva med znanstvenimi disciplinami, ki so bliže enemu ali drugemu prijemu, ampak so lastna tudi vsaki raziskavi ali znanstvenem delu. Uspeh vsake discipline ali raziskave je dejansko odvisen od primerne kombinacije obeh prijemov.

Idiografične znanosti so se na napetosti nedavno odzvale, kot je videti, s protinapadom, ki se opira na antropologijo Clifforda Geertza in njegov prijem »domačinskega pogleda« (»native's point of view«). Geertzeva perspektiva je bila sprejeta s širokim konsenzom v humanistiki po objavi knjige *The Interpretation of Cultures* (Interpretacija kultur) v letu 1973 in je imela podoben vpliv kakor Saussurova lingvistika (objavljena leta 1916) na strukturalizem.[2] Geertzeva teza, da lahko govorimo o družbah le z njihovim lastnim jezikom in da teoretski aparat onemogoča proučevanje dejanskega funkcioniranja družb, diskvalificira humanistično uporabo sociološkihprijemov ali »reificiranih« kategorij, kot so struktura, razred in

razredni boj. Sledi diskvalifikcije lahko najdemo povsod v humanističnih vedah: v literarni vedi, zgodovinopisju, kulturnih študijih ipd. V Geertzevi proti-epistemologiji so primerna teoretska orodja *izolirane in fiksne družbene reprezentacije*, ki naj bi bile edini sprejemljivi posrednik v znanstvenem proučevanju. Geertzev prijem zato onesposobi osnovno načelo v idiografičnih znanostih, ki ga je Vico poimenoval »topična metoda«, po kateri morajo biti koncepti v humanistiki sposobni, da zagotovijo srečanje različnih možnih perspektiv na določeno problematiko pa tudi prostor za soočenje in primerjavo teh perspektiv. Ni treba posebej poudarjati, da je tej »post-geertzevski« epistemologiji samoumeven pogled na družbo kot na konsistentno in pomirjeno skupnost, v kateri obstajajo kvečjemu »mehke družbene razlike« (Breznik 285). Kakor vidimo, je lahko epistemologija tudi politično stališče.

Končni učinek je ta, da se epistemologija Clifforda Geertza ni zmožna soočiti s »progresističnim« teleološkim pogledom, ki ga znanosti vsiljujejo ideologije o »inovacijah«, podpori industriji, ekonomski učinkovitosti ipd. Ko se je odpovedala topični metodi v raziskavi, si je onemogočila dialektično razumevanje človeških pojavov, ki je nedvomno najpomembnejši prispevek humanistike v izmenjavi med nomotetičnimi in idiografičnimi znanostmi. Topični ali dialektični prijemi so bili tudi trdna obramba pred teleološkimi ideologijami, ki zlahka spodnesejo znanstveno delo. Potem ko so se odpovedale tem prijemom, so humanistične vede postale dovzetne za »spontano ideologijo znanstvenikov« (če naj uporabimo Althusserjev koncept), ki se pogosto pokaže skoz idejo »napredka«.

Ideja napredka prispeva k razmeram, v katerih lahko kapitalistični interesi zasežejo znanstvene prakse, in trenutno to počnejo zelo uspešno.[3] Bili bi kratkovidni, če bi videli v ideologiji napredka zgolj »spontani« element znanstvenih praks;[4] ideologija napredka le v določenih situacijah vznikne kot spontana ideologija znanstvenih praks,[5] a je hkrati dominantna ideologija aparatov kapitalistične države: ideologija zagotavlja enotnost znanstvenih ideoloških aparatov in artikulira te aparate v odnosu do drugih aparatov kapitalistične države.[6] Državni aparati dosledno vsiljujejo to ideologijo in priganjajo k preusmerjanju znanstvenega dela v razvijanje inovacij za razvoj kapitalistične artikulacije produktivnih sil. Če si ogledamo najnovejšo administrativno stvaritev Evropske komisije, *Horizont 2020*, ali na primer najnovejšo slovensko raziskovalno strategijo, *Drzna Slovenija*, vidimo, da evropske in ustrezne nacionalne politike silijo znanost, da se preusmeri izključno na inovacije kot ekonomske faktorje za spodbujanje ekonomske »rasti« (pri čemer prikrivajo, da ekonomska rast dejansko pomeni varovanje in večanje profitov za kapitalske lastnike).[7] V tem okviru dobijo humanistične in družbene vede nalogo, da mirijo družbene konflikte – ki

izhajajo iz »inovativnega« prestrukturiranja delovnih procesov in nasploh iz izkoriščevalskih in represivnih odnosov, ki jih ustvarja prav gospodarska rast –, da se specializirajo za identitetne ideologije in da vzdržujejo družbeno kohezijo za akumulacijo kapitala.

Nove in stare revolucije

Materialni pogoji za raziskovalno delo, zlasti področje založništva, so postavljeni na novo, da bi ustrezali ideji napredka. Nastanek korporacij za založništvo, distribucijo in izmenjavo tiskanih del, ki zajemajo ves svet, je bil vsekakor velik korak naprej, a ta napredek še zdaleč ne zagotavlja dostopa do publikacij, lažjega kakor v Gutenbergovi kulturi tiska. Nasprotno, akademske publikacije (t. i. »akademsko elektronsko založništvo«) so običajno dostopne le članom in članicam univerz, ki si lahko privoščijo drage naročnine, niso pa dostopne vsem ostalim znanstvenim delavcem in »laikom«. Tudi dostop do literarnih in drugih tiskanih tekstov je vse bolj restriktiven, saj se postopoma opuščajo javno financirani kulturni programi. Novi model založništva sooblikuje določeno družbeno pozicijo znanosti, ki je očitno elitistična. Poleg tega založniški modeli narekujejo določeno »epistemologijo« (ki se osredotoča na novost, eksperiment in inovacijo), predpisuje raziskovalne teme (kot so družbena »kohezija«, »izključevanje« ali »identiteta«) ali, kot ugotavljata Bill Cope in Mary Kalantzis, celo prinaša »epistemološko motnjo« v znanstveno delo (gl. Cope and Kalantzis). Skrita struktura založništva dandanes v resnici »piše« znanstvene članke, romane in pesmi; uspešna je pri uveljavljanju norm med avtorji, saj zbuja videz nevtralnosti v odnosu do znanstvenih in umetnostnih praks ter deluje kot zdrava pamet, kot naravna sila, in ne kot rezultat človekovih odločitev in dejanj.

O novi revoluciji tiska, o elektronskem digitalnem založništvu, prevladuje prepričanje, da tehnološke možnosti utegnejo razširiti splošni dostop do publikacij in spodbuditi večjo družbeno enakost. S kratkim izletom v Gutenbergovo revolucijo tiska želim pokazati na zagate trmoglave vere v tehnološko determiniran razvoj. Moj primer poleg tega pokaže, da brez topičnega prijema ne bi mogli zgrabiti nepričakovanih družbenih korelacij.

Ozrimo se za hip nazaj v 15. stoletje, v čas Gutenbergove revolucije tiska. Francoski zgodovinar Christian Bec je na osnovi seznamov *Magistrato dei Pupilli* zbral podatke o firenških družinskih knjižnicah. »Urad za otroke« je kot varuh firenških sirot hranil podrobne sezname o družinskem premoženju. Seznami so zajemali tudi sezname knjig, zato je Bec lahko proučil, katere knjige so imela gospodinjstva. Sezname je razdelil na dve obdo-

bji, na prvo in drugo polovico 15. stoletja, med katerima je izumljen tisk. Odkril je, da so imele družinske knjižnice v prvi polovici 15. stoletja razmeroma malo knjig, a je bilo mogoče knjige najti po vseh gospodinjstvih ne glede na premoženjsko stanje gospodinjstva. Seznam najpogostejših knjig v firenških gospodinjstvih je presenetljiv: to so bili spisi v italijanskem jeziku (prevladujeta Dante in Boccaccio) in prevodi (zlasti Donatus), medtem ko verske knjige niso bile tako številne, kot bi morda pričakovali. V drugi polovici 15. stoletja so se knjižnice spremenile, kar zadeva število knjig ter priljubljene avtorje in naslove. Najpogostejši avtorji so bili Petrarka, Ciceron, Dante, Virgilij, Ovid, Boccaccio, Donatus in Tit Livij, izdani v latinščini, ki je bila seveda primerna za izobražene humaniste. V tem obdobju se je število majhnih družinskih knjižnic zmanjšalo, pojavile pa so se knjižnice s sto in več knjigami. Pomembno je, da je bila v Italiji v drugi polovici 15. stoletja tiskarska dejavnost najbolj razširjena in da je bilo na današnjem ozemlju Italije celo več tiskarskih preš kakor v Nemčiji, kjer so prvič uporabili to novo tehnologijo. Sámo mesto Firence je bilo v tem času na četrtem mestu v Evropi po številu novih tiskanih naslovov (Febvre in Martin). Četudi je bil dostop do tiskanih knjig v Firencah očitno enostavnejši kakor drugje, Bec sklene, da je imela po iznajdbi tiska večina Firenčanov slabši dostop do knjig kakor pred iznajdbo, ko so knjige ročno prepisovali. Samo bogati in dobro izobraženi posamezniki so si lahko privoščili tiskane knjige in neredko so jih kupovali zelo veliko, zlasti v latinskem jeziku. Vse ostale so ekonomska stratifikacija in finančne težave odvrnile od kupovanja knjig. Za nameček se je pojavila nova kulturna ovira: knjig, ki so jih tiskali v latinskem jeziku, večina ljudi ni znala brati.

Samuel K. Cohn ml., prav tako zgodovinar, je prišel do podobne ugotovitve, ko je proučeval naročila slik v oporokah. V prvi polovici 15. stoletja so mnogi naročili slike *post mortem*, volila pa so bila navadno majhne vsote. V zadnjih letih 15. stoletja pa so začela prevladovati naročila za velika freskarska dela, medtem ko so mala naročila skoraj zginila.

Ti ugotovitvi nasprotujeta trditvi Richarda Goldthwaita, da je Italija po zgubi gospodarskega prvenstva v volni in drugih osnovnih produktih zaostanek uspešno nadomestila z izvozom luksuznih dobrin. Po Goldthwaitu je višek renesančnega umetnostnega mecenstva dejansko trenutek, ko se je vzpostavila prva proto-kulturna industrija. O proto-kulturni industriji naj bi po Goldthwaitu lahko govorili zato, ker je bil delež premožnega prebivalstva v Firencah večji kakor v drugih delih Evrope. A to dejstvo lahko dokaže Goldthwaitovo tezo le, če firenški premožni sloj primerjamo s premožnimi sloji po Evropi: argument je veljaven samo, če družbo opazujemo od zgoraj navzdol. Če pa zamenjamo perspektivo, se pred našimi očmi pojavi velika množica zelo revnih ljudi. Revščina in pomanjkanje sta

bili v Firencah leta 1427 neprimerno obsežnejši kakor v Veliki Britaniji leta 1688 (ob domnevno brutalnih začetkih industrijskega kapitalizma) ali na primer leta 1962 (Cippola 5–17). Zato se nam zdi skoraj neverjetno, da bi lahko v visoki renesansi veliko ljudi sodelovalo na trgu kulturnih dobrin kot potrošniki. »Potrošnjo« kulturnih dobrin bi veliko bolje opisali kot »razkazovalno porabo« razmeroma majhne družbene skupine. Zato lahko sklenemo, da ni nujne korelacije med umetnostnim razcvetom in družbeno enakostjo; razni družbenoekonomski in kulturni faktorji (na primer ekonomske neenakosti, družbena stratifikacija in učena kultura v latinskem jeziku, ki ga večina ne zna) lahko spodbudijo protislovja med produktivnimi silami in produkcijskimi odnosi. Tehnološki razvoj utegne razširiti skupino tistih, ki jim tehnološki napredek lahko koristi, a hkrati utegne razlastiti mnoge druge, ki so že imeli korist od »socializacije« stare tehnologije. Na tem mestu moramo tudi opozoriti, da je ideja o nepovratnem in kontinuiranem »procesu civiliziranja« bržkone iluzorna: zgodovina družbe je v resnici polna prekinitev, medtem ko istočasni heterogeni procesi povzročijo re-kompozicijo in re-artikulacijo družbenih praks in institucij, o katerih ne moremo zagotovo trditi, da imajo karkoli opraviti z napredkom.

Elektronsko založništvo je bilo revolucionarna iznajdba, primerljiva z nastankom in širjenjem tehnologije tiska v 15. stoletju. Ker se še zmerom razvija, ne moremo napovedati vseh možnih založniških in distribucijskih modelov. A že zdaj je precej jasno, da nova tehnologija s pomočjo zunajekonomskih sil, kot je pravni sistem avtorskih pravic, vsiljuje nove produkcijske odnose, ki omogočajo zasebno prisvajanje tehnologije in njenih rezultatov za dobičke individualnih kapitalov. Kakor tisk pred petimi stoletji je tudi elektronsko založništvo nemara razširilo skupino tistih, ki jim možnosti nove tehnologije koristijo in zagotovo je nova tehnologija izboljšala medsebojno povezanost bralskih skupnosti. A kljub temu ta tehnologija ogroža splošni dostop do publikacij, ki so ga v kulturi tiska že zagotovile javne knjižnice, ki pa jih elektronsko založništvo postopoma uničuje. Elektronsko založništvo je namreč odpravilo institucijo »javnega posojanja«, ki je v preteklosti zagotavljala splošni dostop do tiskanih tekstov. Javne knjižnice smejo ponuditi svojim članom le še oddaljeni dostop do elektronskih publikacij, če plačajo visoko licenco. Imetniki avtorskih pravic pa imajo pravico, da omejijo dostop do publikacij za ožje skupine knjižničnih članov; imajo pa tudi pravico, da zahtevajo visoka nadomestila. Zato knjižnice neredko omejijo dostop do elektronskih zbirk (akademskih e-časopisov, e-knjig in e-baz podatkov) na izbrano skupino članov. To je bilo nepredstavljivo, dokler je bilo poslanstvo javnih knjižnic neločljivo povezano s splošnim dostopom.

Sklepe, ki jih izpeljemo iz raziskav, je odvisen, kot smo videli, od gledišč, ki jih zavzamemo. Ni objektivnega prijema ali kvantitativne baze podatkov, ki bi sama na sebi omogočila refleksijo brez teoretskega premisleka, ki se začne že z izhodiščno identifikacijo predmeta raziskave. Humanistika ne more proučevati človeških zadev, ne da bi upoštevala pluralnosti gledišč. Zato z odpravo topične metode tvegamo ideološko zaslepitev ali vsaj neprevidna poenostavljanja.

Znanost o znanosti in merjenje znanstvenega učinka

Verjamemo, da implicitna epistemološka pozicija, ki jo znanstvenim praksam tiho vsiljujejo sedanje regulacije akademskega in raziskovalnega okolja, zlasti metode domnevno objektivne evalvacije znanstvenega dela, povečuje nemoč znanstvenikov, jim vsiljuje določene epistemične odločitve in izključuje druge, onemogoča produkcijo teoretske *problématique* in zahteva obravnavo ideoloških problemov. To ima globoke družbene učinke: radikalno spreminja pogoje raziskovalnega dela, določa, kje in kako morajo znanstveniki objavljati znanstvene rezultate, in atomizira znanstvene skupnosti (Močnik 441–510). Vsemu navkljub so znanstveniki doslej le medlo protestirali proti tej regulaciji. To nas mora skrbeti, saj razkroj institucionalizacije znanstvene prakse kot posebnega družbenega področja odpravlja institucionalno ločitev znanstvenega dela od ideoloških praks in procesov. Produkcija *epistemološkega reza* je zagotovo stalna naloga vsake teoretske prakse: v sedanji situaciji, ko je epistemološki rez institucionalno onemogočen, je treba teoretske prakse izvajati *v kljubovanju* institucionalnim pogojem možnosti.[8]

Objektivna evalvacija znanstvenega dela zajema več postopkov: kvantifikacijo podatkov za merjenje znanstvenega učinka vsakega znanstvenika s številom publikacij in citatov; ocenjevanje akademskih revij s »faktorjem vpliva«; mednarodno ocenjevanje univerz; mednarodno merjenje inovativnosti; in mednarodno ocenjevanje znanstvene »kompetitivnosti«. Elektronsko akademsko založništvo in upravljanje informacij sta omogočili zbiranje in obdelovanje ogromne količine podatkov: ta tehnološka možnost sama je, zdi se, kar zadostni argument, da se indeks citiranosti in faktor vpliva uporabljata kot glavna elementa pri evalvaciji znanstvenega dela. A večina drugih argumentov govori proti takšnemu ocenjevanju, saj je delitev raziskovalnih sredstev in mest na univerzi postala odvisna od tega kontroverznega merjenja.

Ko so akademske revije začele izdajati elektronske izvode, so se pojavile številne možnosti zbiranja podatkov iz objavljenih člankov. Metodologijo

indeksa citiranosti, ki so jo prvič preizkusili v dvajsetih letih 20. stoletja,[9] je bilo zdaj mogoče uporabiti v zares širokem obsegu. Ne smemo pozabiti, da je akademsko založništvo profitno usmerjena industrija z najvišjimi profitnimi stopnjami v založniškem sektorju. Med petimi založniki z največjim prometom v letu 2009 so kar trije založniki akademskih revij (Reed Elsevier, Thomson Reuters in Wolters Kluwer). Ti so skoncentrirali dovolj revij, da so lahko izkoristili novo poslovno priložnost, tako da zdaj strankam poleg samih revij in člankov ponujajo tudi metapodatke o avtorjih, publikacijah, citiranosti in faktorjih vpliva. Zaradi dvojne funkcije skoraj monopolnih korporacij na področju akademskega založništva morajo avtorji z njimi sodelovati zaradi dveh razlogov: glavni merili ocenjevanja znanstvenega dela sta objave v revijah z največjim faktorjem vpliva in citati, ki jih avtor zbere v člankih drugih avtorjev, objavljenih v isti skupini revij. Avtorji se morajo zato boriti za objave v revijah z največjim faktorjem vpliva in zbirati citate svojih objav, če želijo (še naprej) delati kot raziskovalci ali predavatelji.

Ideja o merjenju citatov v znanstvenih člankih, knjigah in referatih je nastala v dvajsetih letih, sčasoma pa se je razvila metoda »diskretnega merjenja, ki ne potrebuje respondentovega sodelovanja in samo po sebi ne vpliva na odgovor (tj. je neodzivno)« (L. C. Smith, nav po. Bornmann in Daniel 45). Ta navedek izdaja željo informacijske znanosti, da bi izdelala objektivno merjenje znanstvenega učinka. Eugene Garfield, ki je ustanovil Institute for Scientific Information (Inštitut za informacije o znanosti), za nameček trdi, da ima ta metodologija korenine v zgodovini znanosti (in citira Kuhnovo knjigo *The Structure of Scientific Revolutions*) in njen blagoslov:

> Določene predpostavke, tako zgodovinske kakor sociološke, podpirajo idejo o »kartiranju« znanosti, s tem da se identificirajo ključni članki in dogodki z analizo citatov. Osnovna enota analize pri kartiranju so najbolj citirani dokumenti. Predpostavlja se, da so ti članki ali knjige pokazatelji ključnih znanstvenih idej ali dogodkov. Med njimi so teoretske formulacije, spekulativne hipoteze, eksperimentalni rezultati, postopki ali metode in kombinacije vsega tega. Dokumenti, ki so v določenem obdobju zelo pogosto citirani, dobijo poseben status pomembnih idej na njihovem področju. (Garfield idr. 181)

Citati lahko zagotovo delno nakažejo[10] ideje, postopke, metode in koncepte v tekoči znanstveni produkciji; za zgodovino znanosti so dragoceno orodje pri rekonstruiranju nevidnih šol in znanstvenih paradigem. Te metode so verjetno precej uporabne v komulativnih nomotetičnih znanostih, ki se opirajo na najnovejše eksperimente, postopke in odkritja. Če pa se o znanstvenem delu informiramo le na podlagi grobih statističnih kazalcev, nam grozita poenostavljanje podatkov, ki jih imamo na voljo,

in padec v empiricizma, tj. v prepričanje, da je vse, kar lahko ugotovimo, že v domnevno objektivni realnosti, ki da ne potrebuje nadaljnjega intelektualnega premisleka (kar je dejansko problem Geertzeve epistemologije). Tvegamo tudi implikacijo, da je znanstvena publicistika v določenem založniškem okolju s posebnimi odnosi moči in profitno usmerjenostjo zvesto ogledalo »znanstvene produkcije«. Takšno razmišljanje je očitno problematično.

Odgovorili pa moramo še na najpomembnejše vprašanje: kaj je znanstvena produkcija? Kaj loči znanstveno delo od ideologije in omogoča znanosti, da izvede »epistemološki rez«, ki loči znanstveno delo od ideoloških praks? Louis Althusser nas uči, da moramo t. i. realni predmet, *l'objet réel*, ki obstaja neodvisno od našega mišljenja, znati ločiti od t. i. predmeta znanja, *l'objet de la connaissance*, ki je rezultat našega mišljenja in obstaja neodvisno od realnega predmeta. Tu gre za procesa, ki pripadata različnim ontološkim ravnem: na eni strani nastajanje in življenje realnih predmetov poteka v realnosti z delovanjem naravnih in zgodovinskih sil; na drugi pa je predmet znanja proizvod našega mišljenja v skladu s specifičnimi kognitivinimi procesi, ki kot svoja orodja uporabljajo koncepte. Četudi lahko koncepti »reproducirajo« realne predmete, ne pripadajo istemu področju življenja kakor realni predmeti in opravljajo specifične funkcije v procesu produkcije predmetov znanja. Znanstvena praksa je tedaj specifična družbena produkcija, ki proizvaja predmete znanja, se pravi, teoretska produkcija (Althusser idr. 3–79). To je začetna formalna identifikacija znanstvene prakse, ki seveda potrebuje nadaljnjo izpeljavo. Poleg tega moramo tudi upoštevati, da obstajata dva različna miselna procesa: ideološko in znanstveno mišljenje. Zato moramo vpeljati koncept epistemološkega reza kot orodja, s katerim lahko ločimo znanstveno misel od ideološke. Epistemološki rez je torej konstitutiven element teoretske prakse.

Garfield in njegovi kolegi so uporabili empiricistični prijem kot bližnjico in predstavljajo podatke o citiranosti kot potencialno orodje raziskovanja in nadzora znanstvenih praks. Takole predstavljajo svoj vizionarski pogled: »Naš Inštitut za informacije o znanosti (ISI) deluje ob osnovni predpostavki, da lahko podatke o citiranosti uporabimo kot kazalce o sedanjih, preteklih in morda tudi prihodnjih dejavnostih na področju znanosti.« (Garfield idr. 179–180)

Na podlagi citatov in morebitne rekonstrukcije idej, paradigem, metod in konceptov (tj. znanstvenega kartiranja) lahko dobimo le faktografski opis intelektualnih dejavnosti. To je še vedno empiricističen projekt, ki pove malo ali nič o znanstveni in/ali teoretski produkciji. Lahko nas pouči o širjenju idej v znanstveni produkciji, a prav tako utegne ponuditi le sliko o zvezdnem sistemu v znanosti ali ujame na delu elitistično akademsko

mrežo. V okviru Garfieldove metode ne moremo določiti narave dejstev, ki nam jih njegova metoda daje na voljo. Zgodovina znanosti, vsaj kakor jo razumejo Garfield in kolegi, se omejuje na faktografski opis na račun proučevanja znanstvene produkcije. To proučevanje zahteva upoštevanje posebnosti neke znanstvene prakse in načina, kako ta praksa proizvede epistemološki rez, ki loči »pred-znanstveno« zgodovino določenega znanstvenega področja od same znanstvene prakse (Althusser 6–7). To so verjetno osnovni in nujni koraki k znanosti kot posebni družbeni produkciji predmetov znanja.

Podatki o citatih so čudovit grob material za nadaljnjo raziskovanje zgodovine znanosti, ne morejo pa biti sami sebi namen. A pogosto so obravnavani prav kot samozadostni. Podjetje Thompson Scientific & Healthcare vse od leta 1982, ko je kupilo Garfieldov Inštitut za informacije o znanosti, ponuja podatke o citiranosti in druge produkte (faktor vpliva in pozneje H-index) za ocenjevanje znanstvenega dela znanstvenikov, fakultet, univerz in držav. Sčasoma se je ponudba razširila: Elsevier ponuja SciVerse Scopus Database, Google pa je razvil Google Scholar s prostim dostopom. Pod vplivom dobro argumentirane kritike (gl. O'Segen; Cameron; Bornmann in Daniel) so podatke o citiranosti delno izboljšali: časovno omejitev zbiranja podatkov o citatih so podaljšali z dveh na pet let, H-index pa je spremenil faktor vpliva. Kljub temu pa so temeljni argumenti proti uporabi podatkov o citiranosti ostali prezrti.

Težko razumemo, da so znanstvene skupnosti tako zlahka sprejele podatke o citiranosti za ocenjevanje raziskovanja, ki so vprašljivi kot metoda in neprimerni za ocenjevanje raziskovalnega dela. To je presenetljivo zato, ker sama metoda v marsičem nasprotuje metodološkim premisam družboslovja: epistemološko je v nasprotju z osnovnimi pogoji znanosti kot družbene prakse; z vidika ljudi, ki jih zadeva, pa ogroža eksistenčne pogoje znanstvenega dela. Kot smo videli, indeks citiranosti združuje mnoge škodljive poteze: slabi epistemološko refleksijo v znanstveni produkciji (vprašanje topične metode, epistemološkega reza), onemogoča nadzor nad spreminjajočimi se pogoji dela (založništvo, metoda ocenjevanja) in spodbuja servilen odnos od zunanjih ideoloških zahtev in pričakovanj. Te poteze so sistematično povezane in škodljive tako za »mehke« kakor za »trde« znanosti. Vse znanosti v enaki meri izzivajo, da zahtevajo nazaj svojo družbeno vlogo ter da se hkrati družbeno in politično angažirajo. Kot smo videli, ni znanosti brez družbene in politične vpletenosti.

A kakšen je predlog na nasprotni strani? Oglejmo si sklepni argument, zakaj je indeks citiranosti vendarle primerna metoda za ocenjevanje raziskovanja, razvit v članku Bornmanna in Daniela »What do Citation Counts Measure?«. Avtorja podrobno predstavita argumente tako za indeks citira-

nosti kakor proti njemu. Članek skleneta z ugotovitvijo, da na mikro-ravni, na ravni lokalne znanstvene produkcije, res obstaja večja možnost, da citati na odsevajo znanstvenega vpliva citiranega dela. Avtorji naj bi ne nazadnje pogosteje citirali avtorje, ki jih osebno poznajo, in lahko si na primer pomagajo z vzajemnim citiranjem, zato po Bornmannu in Danielu zajemanje podatkov o citiranju na nižji agregatni ravni bržkone ne more prikazovati znanstvenega vpliva citiranih del. Toda na višji agregatni ravni naj bi popačenja zginila, saj »delo [...] obravnava relevantna znanstvena skupnost kot pomembno in ustrezno (kot jedro raziskovanja), na kar družbene spremenljivke in procesi ne vplivajo bistveno« (Bornmann in Daniel 70). Ta argument je prava akrobacija, saj dopušča, da iz več negativnih premis o lokalnih znanstvenih skupnostih izpeljemo pozitiven sklep o širši mednarodni znanstveni skupnosti, četudi predpostavljeno širšo znanstveno skupnost sestavljajo le razne lokalne znanstvene skupnosti. Na višji agregatni ravni, kjer institucionalna svetovna hegemonija zares deluje, naj bi zginila akademska potuha in hierarhični konformizem, vzniknila pa naj bi resnica. Resnica o odnosih moči, bi lahko dodali.

Namesto sklepa: znanost na borzi

Precej mogoč izid procesa, ki smo ga opisali, je znanstveno delo kot investicija na borzi. Založniki so spremenili akademsko založništvo v nekakšno borzo s sistemom kvanitifikacije in valorizacije enot, kot so publikacija, citat, zavrnitev članka, faktor vpliva, H-indeks ipd. Proces »listinjenja« spreminja ne-denarne vrednosti v kvantitativne vrednosti, ki lahko vstopijo v proces monetizacije znanstvenih podatkov. Kvantitativne vrednosti, ki nastanejo v tem procesu, avtorji zamenjujejo za mesta na univerzi, financiranje raziskav, nagrade in prestiž; nacionalni financerji jih uporabljajo kot merila pri razdeljevanju raziskovalnega denarja, za mednarodno primerjavo in uvrščanje na lestvici mednarodne »znanstvene kompetitivnosti«; založniki pa jih uporabljajo za izčrpavanje javnih virov za izobraževanje in raziskovanje. Zdi se, da sistem deluje dobro, saj agente poveže v mrežo vzajemnih obveznosti in ugodnosti. Založniki so tako ustvarili konsistenten sistem »denarne odvisnosti«, kjer sámo znanstveno delo nima ustrezne cene.

OPOMBE

[1] Sedanji predsednik vlade in minister za znanost sta nedavno ostro krčenje proračuna za javno visoko izobraževanje utemeljila z argumentom, da naj slovenske univerze bolje izkoristijo »notranje rezerve«, saj naj bi povprečen univerziteten učitelj delal od štiri do šest ur na teden.

[2] Claude Lévi-Strauss je leta 1950 (gl. Lévi-Strauss) oblikoval predlog, ki je zelo blizu Geertzevi ideji. Po Lévi-Straussu naj bi primeren prijem konceptualiziral Maussovo »totalno družbeno dejstvo« kot »durkheimovsko stvar« in hkrati »domačinsko predstavo«. Moramo poudariti, da je Lévi-Strauss predlagal izhod iz Geertzeve slepe ulice, še preden je Geertzu sploh postala problem.

[3] Uporabljamo sintagmo »kapitalistični interesi«, kolikor prav ti učinkujejo za »državnimi interesi«.

[4] V tem primeru bi sodile v »kumulativno« fazo znanstvene prakse med dvema »prelomoma« v znanstveni problematiki: to je značilna epistemična situacija v aplikativnih znanostih in v perifernih praksah tistih znanosti, kjer epistemične »prelome« pogojujejo večje finančne investicije. Aplikativne znanosti imajo kapitalistični interesi na splošno najraje, periferne prakse pa so politično in institucionalno prevladujoče na perifernih področjih (kot je Slovenija).

[5] Gl. zgornjo opombo.

[6] Althusserjevi tezi, da dominantna ideologija poveže različne ideološke aparate, lahko dodamo, da dominantna ideologija poveže tudi polja regionalnih aparatov, kakršen je znanstveni ideološki aparat v raznih oblikah materialne eksistence njegove (znanstvene) ideologije: univerze, akademije znanosti, inštituti ipd. Dominantna ideologija določa predvsem merila financiranja, načine financiranja (kjer vse bolj prevladuje financiranje »po projektih«, kar lajša nadzor), merila zaposlovanja (ki je vse bolj prekarno, kar onemogoči solidarnost med znanstveniki in te podreja zahtevam kapitala in države). Način, kako dominantna ideologija poveže znanstveno polje, je vse bolj v protislovju z logiko znanstvene prakse: zagovarja individualizem in tekmovanje, kjer so prakse kolektivne in kooperativne, zahteva kratkoročno uporabnost za kapital, kjer so prakse v osnovi dolgoročne in imajo lastna merila »uporabnosti«. V EU je integraciji dominantne ideologije skoraj uspelo izriniti teoretske prakse in njihove nosilce iz znanstvenih institucij (tudi z univerz) in iz sistema financiranja. (Gl. Breznik in Močnik.)

[7] Za več o slovenski raziskovalni strategiji gl. Žagar in Korsika (ur.).

[8] Sedanja tendenca znanstvenih in akademskih institucij, da bi odpravile institucionalizacijo epistemičnega reza, tj. ločitev teoretskih in ideoloških praks, uničuje »ločitev principa moči, principa zakona in principa znanja«, ločitev, ki je po Lefortu »prelomen dogodek«, konstitutiven element modernosti in njene politične emancipacije (Lefort 65 op. 8).

[9] Za prvo dokumentirano uporabo indeksa citiranosti gl. Gross in Gross.

[10] »Do določene mere« pravim zato, ker avtorji citirajo tekste zaradi različnih razlogov in citat ne odseva vselej znanstvenega vpliva teksta ali avtorja.

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Theoretical Practice in Innovation-driven Research Environment

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The political assault on pure science and theoretical production imperils the very foundations of both 'soft' and 'hard' sciences. What positions are scientists taking towards the changing conditions of research? I will look at the epistemological position and self-reflection in scientific practices, the material conditions of research (particularly publishing and scientific impact measuring), the responses to external demands and the social positioning of science.

Keywords: epistemology / scientific practice / the topical method / the material conditions of research / publishing / scientific impact measuring

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In this article, I will try to shed some new light on the age-old question of possible epistemological convergences between the so-called 'soft' and 'hard' sciences: the humanities and social sciences on one side and natural sciences on the other. Relations between the two kinds of science have so far been understood rather as discrepancies between two rigid systems of knowledge, that is, as the difference between nomothetic and idiographic sciences. However, the recent political assault on 'pure science' and theoretical production in general seems to imperil the very foundations of both kinds of science.[1] Simultaneously, the whole research milieu is changing: from the university, which is constrained to submit to corporate governance and to engage in market-oriented 'services', to research institutions, which are pressed to justify their existence by providing innovations for enterprises. All these transformations are supposed to improve the global competitiveness of states. At the same time, exceedingly quantitative selection criteria for funding or personnel decisions are being introduced under the aegis of the so-called 'scientific excellence', that is, the international competitiveness of local scientists. Academic publishing, a decisive institutional setting for presentation, distribution and exchange of research findings, is being taken over by profit-driven multinational corporations, which impose their own conditions on the access to pub-

lishing. These external pressures pose a series of questions, specifically the questions regarding the epistemological position and self-reflection in scientific practices, the comprehension of material conditions of research work, the responses to external demands and the social positioning of science. I see these questions as closely interrelated: the capacity of scientists to analyse their own material conditions of research and to take a stance towards these conditions depends on their epistemological positioning. And vice versa, if scientists are not capable to cope with their own conditions of research, with what other task can we entrust them?

Counter-epistemology in social sciences

The notions of nomothetic and idiographic science were introduced by Wilhelm Windelband in 1894 as part of his critique of positivism; however, the debate itself goes back to Giambattista Vico and his polemics against Cartesianism as the critical method of the Moderns, which Vico contrasted with the topical method of the Ancients. G. H. von Wright described nomothetic sciences as examination of events that repeat themselves and can be anticipated; such events can be moreover isolated with the aim of carrying out experiments whose observation may engender scientific general laws. On the other side, idiographic sciences such as historiography study transient events, which we comprehend in the form of description. Wright (6) stressed that Georg Simmel compared historiography to theatre and defined the method of comprehending past events as empathy. During the modern age, the prestige of the nomothetic approach give birth to many new disciplines by annexing research areas that initially 'belonged to' the idiographic sciences (in this way, the nomothetic sociology 'appropriated' the field that had been covered by historiography). Rastko Močnik (188–191) even claims that social sciences are a compromise formation resulting from the pressure of the Galileian paradigm upon the humanities. However, tensions between nomothetic and idiographic sciences not only stem from competing scientific disciplines implementing one approach or the other, but they are also inherently present in every research or scientific work. The success of every discipline and of each research depends on a suitable combination of both approaches.

Idiographic sciences recently responded to these tensions with what seems to be a counter-attack based on Clifford Geertz's anthropology and his 'native's point of view' approach. Geertzian perspective has probably been the most widely accepted approach in the humanities since the publication of *The Interpretation of Cultures* in 1973, with an impact comparable

to the impact of Saussure's linguistics (published in 1916) upon structuralism.[2] Geertz's idea that we can speak about societies only in their own languages, and that theoretical apparatuses invalid our comprehension of the functioning of societies, disqualifies the use of sociological approaches or 'reified' concepts such as structure, class and class struggle in the humanities. Traces of this disqualification can still be found all over the spectre of the humanities, including literary studies, historiography and, say, cultural studies. In Geertz's 'counter-epistemology' *isolated and fixed social representations* are presumably the only acceptable intermediaries in research examination. As a consequence, his approach impedes the basic principle of idiographic science, due to which Vico called this science the 'topical method', according to which concepts should be able to provide a meeting point of various possible perspectives on a certain problem as well as space for their confrontation and comparison. Needless to say, this particular 'post-Geertzian' epistemology also takes for granted a vision of society as consistent and pacified community in which only 'soft social divisions' may exist (Breznik 285). Epistemology is therefore also a political statement.

As a consequence, Geertz's epistemology is helpless when confronting the teleological viewpoint that is currently imposed upon science by the ideologies of 'innovation', service to industry, economic efficiency and the like. By removing the topical method from research, this viewpoint renounced the dialectical comprehension of human affairs, which is the most important contribution of the humanities to the interchange between nomothetic and idiographic sciences. Topical or dialectical approaches were a solid impediment against the teleological ideologies that easily undermine scientific efforts. After having given up these approaches, the humanities gave way to the 'spontaneous ideology of scientists' (to use Louis Althusser's concept) that frequently manifests itself as the idea of 'progress'.

It is precisely by way of the idea of progress that capitalist interests can grasp the scientific practice, something they are doing very efficiently at the moment.[3] It would be short-sighted to consider the ideology of progress merely as a 'spontaneous' component of scientific practices;[4] while the ideology of progress emerges as the spontaneous ideology of scientific practices only in specific and well determined situations,[5] it is the dominant ideology of the apparatuses of capitalist state: it secures the unity of scientific ideological apparatuses and articulates them onto other apparatuses of the capitalist state.[6] State apparatuses rigorously enforce this ideology and accelerate the re-orientation of scientific work into innovations for the development of the capitalist articulation of productive

forces. Looking at the European Commission's latest administrative creation, *Horizon 2020*, or at the latest Slovenian national research strategy, *Drzna Slovenia* (Daring Slovenia), we see that the politics of the European Union and its national epigones compel science to work exclusively on innovations as economic factors for raising economic 'growth' (concealing the fact that economic growth actually means securing and increasing profits for capital owners).[7] In this framework, the humanities and social sciences are assigned the task of pacifying conflicts that inevitably arise from the 'innovative' restructuring of the labour processes and generally from exploitative and repressive relations produced by economic growth itself; the humanities and social sciences are being encouraged to specialise in identity ideologies in order to maintain social cohesion for the accumulation of capital.

Old and new revolutions

The material conditions of research work, especially in the publishing area, are now being redesigned so as to comply with the idea of progress. The development of worldwide establishments for the publication, distribution and exchange of printed texts was certainly a great leap forward, but this development hardly guarantees access to printed texts greater than in the early modern print culture. Academic publications (the so-called 'academic electronic publishing') are restricted to the members of universities that can afford to pay expensive subscriptions, while remaining inaccessible to the majority of scientific workers. Similarly, access to literary and other printed works is increasingly restrained due to progressive dissolution of public cultural programmes. The new models of publishing impose a certain elitist social position of science. Moreover, these models dictate a certain 'epistemology' (focusing on novelty, experiments and innovation), prescribe research topics (such as social 'cohesion' and 'exclusion', 'identities', etc.), or, according to Bill Cope and Mary Kalantzis, bring 'epistemological disruption' into scientific work (see Cope and Kalantzis). It is therefore hidden publishing structures that nowadays 'write' scientific articles, novels and poems; they efficiently impose norms upon writers as they assume a neutral look with respect to scientific or artistic practices and operate in a common-sense way, like the forces of nature, not as a product of human decisions and actions.

The new print revolution, electronic digital publishing, is associated with a belief that technological possibilities may open a larger general access to printed works and promote greater social equality. With a brief

digression into Gutenberg's print revolution I would like to show that the persistent belief in technologically determined progress can be problematic. The following case is moreover instructive because it shows that without the topical approach we would not be able to grasp unexpected social correlations.

Let me briefly go back to the fifteenth century, to the time of Gutenberg's invention of print. French historian Christian Bec reconstructed Florentine family libraries from this period on the basis of the inventories of *Magistrato dei Pupilli*. The 'Magistrate of the Pupils' exercised custody over Florentine orphans and maintained a detailed registry of family heritage for each child. This inventory included the list of books of every household, on the basis of which Bec examined which books the households possessed. He divided the inventories into two periods, divided by the invention of print. According to his findings, in the first half of the fifteenth century, family libraries contained a relatively small number of books, which were, however, equally distributed among Florentine households regardless of the economic strength of families. The list of the most frequent works in Florentine households of that period is quite surprising: these were either writings in Italian (with Dante and Boccaccio prevailing) or translations (especially Donatus), while religious books were not as frequent as one might expect. In the second half of the fifteenth century, the amount of books in family libraries as well as the preferences of readers changed. The most frequent authors were Petrarca, Cicero, Dante, Virgil, Ovid, Boccaccio, Donatus and Tit Livy in Latin language, a selection more suitable for educated humanists. The number of small family libraries was progressively decreasing, while big family libraries with hundreds of books appeared in that period. We should know that Italy surpassed even Germany, where the invention of print technology took place, in the number of printing presses soon after their invention; and as for the number of printed books in that period, Florence occupied the fourth place in Europe (Febvre and Martin). Even though the accessibility of printed books in Florence was wide, Bec concludes that after the invention of print, most Florentines had a more limited access to books than during the preceding period, when books had been copied by hand. Only wealthy and well educated individuals could afford printed books, and they were buying a lot of them, particularly in Latin. Additionally, a new cultural barrier appeared with the books printed in Latin, which most people could not read.

Another historian, Samuel K. Cohn, Jr., drew a similar conclusion from his examination of commissions of paintings in testaments. During the first half of the fifteenth century, many people were commissioning

religious paintings *post mortem* for a low price, while during the last years of the fifteenth century, big commissions of large frescos started to prevail, while small commissions almost disappeared.

These two conclusions contradict Richard Goldthwaite's claim that after having lost the world economic leadership in basic products (such as wool), Italy successfully substituted this regression with the export of luxury goods. According to Goldthwaite's interpretation of this substitution, the peak period of the Renaissance arts sponsorship is actually the moment when the first proto-cultural industry was established. Goldthwaite tries to prove his claim by noting that during this period, the share of wealthy population in Florence was greater than in other parts of Europe. However, this argument is correct only if we compare the Florentine wealthy social strata with other wealthy social groups in Europe; that is, the argument holds if we look at the society from the top down. But if we change the perspective and look from the bottom up, a mass of very poor people appears before our sight. The poverty and deprivation of the poor in Florence in 1427 were incomparably worse than the situation of the poor in Great Britain in 1688 (at the presumably brutal beginning of industrial capitalism) or, say, in 1962 (Cippola 5–17). As a consequence, it seems impossible to see in the high Renaissance a period when a considerable portion of the population could participate as consumers on an accessible cultural market. The process would be much better defined as 'conspicuous consumption' by a relatively small social group. From this observation we can draw the conclusion that there is no necessary correlation between artistic prosperity and social equality; various socio-economic and cultural factors (such as economic inequalities, social stratification and learned culture in Latin inaccessible to the majority of people) may generate contradictions between productive forces and relations of production. Technological development may enlarge the group of those who benefit from it, while expropriating many others who have already taken advantage from the 'socialisation' of previous technology. It is also appropriate to note here that the idea of an irreversible and continuous 'civilising process' is illusory; the history of society is actually full of disruptions, and simultaneous heterogeneous processes result in re-compositions and re-articulations of social practices and institutions for which it is not possible to say whether they have anything to do with progress.

Electronic publishing was a revolutionary innovation comparable to the introduction of print technology in the fifteenth century. It is still developing, so we cannot anticipate all possible publishing and distribution models. However, it is already very clear that the new technology, assisted by extra-economic forces such as copyright regulation, will certainly im-

pose new relations of production, which will support the appropriation of technology and its results for individual capital gains. Like print five centuries ago, electronic publishing may have increased the number of its beneficiaries. But it also imperils the general access to published works, particularly by undermining the public library system. Electronic publishing has eliminated the institution of 'public lending', which had been offering general access to printed works. Public libraries are only allowed to offer to their members distant access to e-published works if they pay costly licences. Moreover, copyright holders have the right to limit access to their works to a special group of library members, while being entitled to request exorbitant compensations. Hence, libraries usually restrict the access to their e-collections (academic e-journals, e-books and databases) to the exclusive group of their members. This was inconceivable in the previous period, when public libraries were tightly associated with general access they had to facilitate.

The conclusions we usually draw from our research depend, as we can see, on the perspective we take. There is no objective approach or quantifiable data that would by themselves make possible any reflection without theoretical elaboration, which starts with the first determination of the object of research. The humanities cannot examine human affairs without taking into consideration a plurality of perspectives. This is why a rejection of the topical method exposes research to imprudent simplifications, if not ideological deception.

Science of science and scientific efficiency measuring

In my view, the implicit epistemological position, tacitly imposed upon scientific practices by the new methods of presumably objective evaluation of scientific work as well as by other regulations of academic and research establishments, increases the powerlessness of scientists, forces upon them certain epistemological choices while excluding others, obstructs the production of theoretical *problématique* and imposes ideological problems. It has deep social effects: it radically changes the conditions of research work, decides where and how scientists should publish their research results and atomises scientific communities (Močnik 441–510). Yet so far, the resistance of scientists to the newly imposed regulations has been weak and inefficient, which should be a matter of particular concern, since the erosion of the institutionalisation of scientific practices as a special social field abolishes the institutional separation of scientific work from ideological practices and processes. The production of the *epis-*

temological break is certainly the permanent task of any theoretical practice: however, in the present situation when the epistemological break is institutionally undermined, theoretical practices have to be exercised *against* their institutional conditions of possibility.[8]

The so-called objective evaluation of scientific work comprises several sets of procedures: the quantification of data for measuring scientific efficiency of every scientist by the number of publications and citations; the evaluation of academic journals by their 'impact factor'; university ranking; international innovation scoreboard; and international science ranking. Electronic academic publishing and information management made manageable enormous quantities of scientific data: this technological possibility alone seems to be a sufficient reason to impose the citation index and impact factor as the main elements of the evaluation of scientific work. However, most of other arguments speak against the use of such measurement, as the allocation of research funds and the distribution of university posts have been subjected to these controversial measuring procedures.

Electronic publishing of academic journals brought about numerous possibilities of collecting data from the published articles. It made possible a large-scale use of the citation index methodology. It is important to note that academic publishing is a profit-driven industry with the highest profit rates in the publishing sector. In 2009, three of the top five publishers with the largest turnover were publishers of academic journals (Reed Elsevier, Thomson Reuters and Wolters Kluwer). They have concentrated enough journals to take advantage of the new business opportunity and are now offering to their clients not only journals and articles, but also metadata about authors, publications, citations and impact assessments. Because of this double function of the quasi-monopolistic academic publishing corporations, authors are forced to cooperate with them for two reasons: the main two criteria for the evaluation of scientific work are publications in journals with the highest impact factor and citations of authors in the articles written by other authors for the same group of journals. So the authors have no other choice than to fight for publications in journals with high impact factors and for as many citations of their works as possible, if they want to keep working as researchers or university teachers.

The idea to measure citations in scientific articles, books or conference proceedings originates from the 1920s;[9] gradually, it was developed into 'unobtrusive measures that do not require the cooperation of a respondent and do not themselves contaminate the response (i.e. they are non-reactive)' (L. C. Smith, qtd. in Bornmann and Daniel 45). The citation betrays the information scientists' desire to forge an objective measurement of scientific efficiency. Eugene Garfield, the founder of the Institute

for Scientific Information, argues that this methodology has roots in the history of science (he cites Kuhn's *The Structure of Scientific Revolutions*) and its legacy:

> Certain presuppositions, both historical and sociological, underlie the idea of 'mapping' science by identifying key papers and events through citation analysis. The basic unit of analysis in mapping is the highly cited document. The assumption is that these articles and books are markers for critical scientific ideas or events, taken in the broadest sense. This includes theoretical formulations, speculative hypotheses, experimental results, procedures or methods, and any combination of these. The fact that some documents have been highly cited within a specified time-period confers upon them a special status as providing important 'ideas' in their respective areas or specialties. (Garfield et al. 181)

Citations certainly can, to a certain extent, indicate[10] ideas, procedures, methods and concepts of the current scientific production; for the history of science they are amazing tools for the reconstruction of invisible scientific currents and paradigms. These methods are probably quite applicable in cumulative nomothetic sciences, which rely on up-to-date experiments, procedures and discoveries. However, if we only take rough statistical indicators as information about scientific work, we tend to simplify the available data and reproduce the empiricist notion that all we can assess is there in the presumed objective reality and does not need any further intellectual intervention (which is the problem of Geertz's epistemology). By proceeding in this manner we also imply that scientific publishing in a particular publishing setting with certain power relations and profit-driven motives can be taken as an exact mirror of what we believe to be 'scientific production'. This type of reasoning is obviously very difficult to accept.

However, a crucial question about scientific production remains answered. What separates scientific work from ideology and enables science to make the epistemological break, which differentiates it from ideological practices? Louis Althusser taught us that we must know how to separate what he calls the real object, *l'objet réel*, which exists independently of our thoughts, from the object of knowledge, *l'objet de la connaissance*, which is the product of our reasoning and exists independently of the so-called real object. The two processes belong to different ontological realms: on one side, formation and duration of the real object pertains to reality by the working of natural and historical forces; on the other side, the object of knowledge is produced by our thoughts in accordance with specific cognitive processes that use concepts as their tools of production. Albeit concepts may 'reproduce' real objects, they do not belong to their realm of existence, and perform specific functions in the production of the objects of knowledge. Scientific practice is therefore theoretical production,

a special social production that produces objects of knowledge (Althusser et al. 3–79). This is an initial formal identification of scientific practice that of course needs further elaboration. Moreover, we have to take into consideration that there exist two different thinking processes: ideological and scientific thinking process. This is why we have to introduce the concept of epistemological break as the tool for differentiating scientific thought from ideological thought. This makes the epistemological break a necessary constitutive element of theoretical production.

Garfield and his colleagues took a quite empiricist shortcut and presented citation data as a possible instrument to survey and control scientific practices. This is how they presented their visionary claim: 'At the Institute for Scientific Information (ISI), we operate on the fundamental assumption that citation data can be used as indicators of present, past, and perhaps future activity in science.' (Garfield et al. 179–180)

However, what we can get from citations and the reconstruction of ideas, paradigms, methods and concepts (that is, from science mapping) is a mere factographic description of intellectual activities. This remains an empiricist project that tells little or nothing about scientific and/or theoretical production. It may inform us about the spread of ideas in scientific production, but it can also reveal merely a star system in science or uncover an elitist academic network. Within the framework of Garfield's method we cannot determine the nature of the 'facts' established by that method. The history of science in Garfield's sense yields factographic description rather than an examination of scientific production. In order to examine scientific production, we have to take into consideration the specificity of a scientific practice, and examine the way it produces the epistemological break, which differentiates the 'pre-scientific' history of a particular scientific area from scientific practice proper (Althusser 47). These are probably the minimal necessary steps to approach science as a special social production of scientific objects of knowledge.

Citation data are an amazing rough material for further research in the history of science, but cannot pretend to be an aim in itself. But this is precisely what happened. Since 1982, when Thompson Scientific & Healthcare bought Garfield's Institute for Scientific Information, the company offers a citation index and its derivatives (the impact factor and H-index) for evaluating the scientific work of a scientist, department, university or country. Since then, the supply has been extended: Elsevier offered the SciVerse Scopus Database, and Google offered Google Scholar with free access. Under the influence of well-argued criticism (see O'Segen, Cameron, as well as Bornmann and Daniel, for a review of arguments) the databases have been improved, but only partially: the time span in which

citations are collected was extended from two to five years, and H-index replaced the initial impact factor. However, the fundamental argument against the use of citation data for evaluating research has been put aside.

It is difficult to understand why scientific communities so easily accepted citation data for evaluating research, something that is contestable as a method and inappropriate for evaluating research work. This is surprising because the method itself is probably in many aspects inconsistent with the social science methodology; from the epistemological point of view, it contradicts the basic conditions of science as social practice; from the point of view of the people involved, it jeopardises the very existence and work of scientists. As we have seen, the citation data method combines a number of detrimental features: the weakening of epistemological reflection in scientific production (the question of topical method, the epistemological break); the scientists' incapacity to control and question the changing conditions of work (such as publishing and evaluation systems); and as a consequence, the servile attitude towards external ideological demands and expectations. These features are systematically interconnected and equally harmful to the 'soft' and the 'hard' sciences. They indiscriminately challenge all the sciences to reclaim their social role and to act socially and politically. There is no science without social and political involvement.

What is the proposition on the other side? To answer this question we should look at the final argument why citation data should nonetheless be considered an appropriate method for evaluating research, as it was proposed by Bornmann and Daniel. The authors meticulously present arguments for and against citation indexes. They conclude their examination with an assessment that at the micro-level, at the level of local scientific production, there is a greater possibility that a citation does not reflect the scientific impact of the work cited. Authors more often cite works by authors with whom they are personally acquainted, they may build up reciprocal exchange of citations to help each other, etc. Therefore, the low aggregation level of citation data is likely to produce, according to the authors, results that do not reflect the scientific impact of the work. But at the high aggregation level such distortions disappear, since the highly cited 'work […] is accepted by the relevant scientific community as important and correct (the core of research), and it is more or less uninfluenced by social variables and processes' (Bornmann and Daniel 70). This argument is a real acrobatic feast that makes possible a deduction from all the negative premises valid for local scientific communities a positive conclusion applicable to a larger scientific community, although this presumed larger scientific community consists but of local scientific communities. At the high level of aggregation, where institutional world hegemony really operates, academic con-

nivance and hierarchical conformism are washed out, and, presumably, the truth appears. The truth of the power relations, one should add.

Conclusion: Science on the stock exchange

A quite possible outcome of the processes outlined above is scientific work as an investment on the stock exchange. Publishers have transformed academic publishing into a close equivalent of a stock exchange with a system of quantification and valuation of items such as publication, citation, rejection of articles, impact factor, H-index, etc. The process of 'securitisation' transforms non-monetary values into quantitative values that can enter into the process of monetisation of scientific data. Quantitative values created in this process are exchanged by authors for university posts, research funding, rewards and prestige; the national funders use them as quantitative research funding criteria and as international score rates of national scientific competitiveness; and publishers use them to plunder public funds for education and research. The system seems to work well as it binds its agents together by a network of reciprocal obligations and benefits. Publishers have thus created a binding system of 'monetary dependence' where scientific work itself cannot find its appropriate price.

NOTES

[1] Recently, both the current Prime Minister and the Minister of Science justified severe cuts in the financing of public high education by claiming that Slovenian universities should make a better use of their 'internal reserves' and adding that the average university teacher works four to six hours per week.

[2] A suggestion that comes close to Geertz's idea was formulated by Claude Lévi-Strauss in his 1950 'Introduction à l'œuvre de Marcel Mauss' (Lévi-Strauss). For Lévi-Strauss, Maussian 'total social fact' should be conceptualised both as a 'Durkheimian thing' and as a 'native representation'. It should be noted that Lévi-Strauss proposed a way out of Geertzian dead-end even before its appearance.

[3] I use the wording 'capitalist interests' insofar as these are the real interested party behind the 'state interests'.

[4] In this case, it would pertain to the 'cumulative' phase of a scientific practice located between two 'ruptures' of the theoretical problematic: this is the typical epistemic situation of the applied sciences and also of peripheral practices in those sciences where epistemic 'ruptures' are conditioned by important financial inputs. Applied sciences are favoured by the capital interests in general, while the peripheral practices are politically and institutionally dominant in peripheral zones such as Slovenia.

[5] See footnote 4.

[6] Althusser's thesis that the dominant ideology unifies various ideological apparatuses should be extended into a thesis that the dominant ideology also unifies the field of regional apparatuses such as the scientific ideological apparatus in its various modes of material existence of (scientific) ideology: universities, academies of science, research institutes, etc. In particular, the dominant ideology determines the criteria of financing, the mode of financing (mostly 'by projects', in order to secure efficient control), criteria of recruitment of the personnel and modes of employment of personnel (mostly precarious, in order to undermine the solidarity between scientists and to subordinate them to the requirements of capital and the state). The way how the dominant ideology integrates the scientific field is increasingly in contradiction with the logic of scientific practices: it promotes individualism and competition where practices are collective and co-operative, it imposes short term utilitarianism for capital where practices are in principle long-term and have their own specific criteria of 'utility'. In the EU, integration by the dominant ideology has all but excluded theoretical practices and their agents from scientific institutions (including universities) and from the system of financing. (See Breznik and Močnik.)

[7] For more on the Slovene research strategy, see Žagar and Korsika, eds.

[8] The current tendency of scientific and academic institutions towards the abolishment of the institutionalisation of the epistemological break, that is, of the separation between theoretical and ideological practices, is destroying 'the separation of the principle of power, the principle of law and the principle of knowledge', considered by Claude Lefort as the central and 'unprecedented event' constitutive of modernity and its political emancipation (Lefort 65n8).

[9] For the first documented application of a citation index, see Gross and Gross.

[10] I say 'to a certain extent' because there are various reasons why one cites a certain work, and they do not always reflect scientific impact on the author.

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